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Mirror for Americans: A Century of Reconstruction History

JOHN HOPE FRANKLIN

PERHAPS NO HUMAN EXPERIENCE is more searing or more likely to have a long-range adverse effect on the participants than violent conflict among peoples of the same national, racial, or ethnic group. During the conflict itself the stresses and strains brought on by confrontations ranging from name-calling to pitched battles move people to the brink of mutual destruction. The resulting human casualties as well as the physical destruction serve to exacerbate the situation to such a degree that reconciliation becomes virtually impossible. The warring participants, meanwhile, have done irreparable damage to their common heritage and to their shared government and territory through excessive claims and counterclaims designed to make their opponents' position appear both untenable and ludicrous.

Situations such as these have occurred throughout history; they are merely the most extreme and most tragic of numerous kinds of conflicts that beset mankind. As civil conflicts—among brothers, compatriots, coreligionists, and the like—they present a special problem not only in the prosecution of the conflict itself but in the peculiar problems related to reconciliation once the conflict has been resolved. One can well imagine, for example, the utter bitterness and sense of alienation that both sides felt in the conflict that marked the struggle for power between the death in 1493 of Sonni Ali, the ruler of the Songhay empire, and the succession of Askia Muhammad some months later. The struggle was not only between the legitimate heir and an army commander but also between the traditional religion and the relatively new, aggressive religion of Islam, a struggle in which the military man and his new religion emerged victorious.¹

Historians have learned a great deal about these events, although they are wrapped in the obscurity and, indeed, the evasive strategies of the late Middle Ages. Despite the bitterness of the participants in the struggle and the dissipating competition of scholars in the field, we have learned much more about the internal conflicts of the Songhay empire of West Africa and about the details of Askia Muhammad's program of reconstruction than we could possibly

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¹ Nehemiah Levtzion, "The Long March of Islam in the Western Sudan," in Roland Oliver, ed., *The Middle Age of African History* (London, 1967), 16–17.

have anticipated—either because the keepers of the records were under his influence or because any uncomplimentary accounts simply did not survive. Interestingly enough, however, the accounts by travelers of the energetic and long-range programs of reconstruction coincide with those that the royal scribes provided.²

Another example of tragic internal conflict is the English Civil War of the seventeenth century. The struggle between Charles I and those who supported a radical Puritan oligarchy led not only to a bloody conflict that culminated in the execution of the king but also to bizarre manifestations of acrimony that ranged from denouncing royalism in principle to defacing icons in the churches. Not until the death of Oliver Cromwell and the collapse of the Protectorate were peace and order finally achieved under Charles II, whose principal policies were doubtless motivated by his desire to survive. The king's role in the reconstruction of England was limited; indeed, the philosophical debates concerning, as well as the programs for, the new society projected by the Protectorate had a more significant impact on England's future than the restoration of the Stuarts had.

Thanks to every generation of scholars that has worked on the English Civil War and its aftermath, we have had a succession of illuminations without an inordinate amount of heat. Granted, efforts to understand the conflict have not always been characterized by cool objectivity and generous concessions. But, because historians have been more concerned with understanding the sources than with prejudging the events with or without the sources, we are in their debt for a closer approximation to the truth than would otherwise have been the case.³

I daresay that both the Africanists concerned with Songhay and the students of the English Civil War will scoff at these general statements, which they may regard as a simplistic view of the struggles that they have studied so intensely. I am in no position to argue with them. The point remains that, whether one views the internal conflicts of the people of Songhay in the fifteenth century, the English in the seventeenth century, or the Americans in the nineteenth century, the conflict itself was marked by incomparable bitterness and extensive bloodshed. The aftermath, moreover, was marked by continuous disputation over the merits of the respective cases initially as well as over the conduct of the two sides in the ensuing years. These continuing disputations, it should be added, tell as much about the times in which they occurred as about the period with which they are concerned. And, before I do violence either to the facts themselves or to the views of those who have studied these events, I shall seek to establish my claim in the more familiar environment of the aftermath of the Civil War in the United States.

² Leo Africanus, *The History and Description of Africa*, 3 (New York, n.d.): 823–25; and Mahmoud Kati, *Tarikh El-Fettach*, ed. O. Houdas and M. Delafosse (Paris, 1913), 13–54.

³ See, for example, Christopher Hill, *Puritanism and Revolution: Studies in Interpretation of the English Revolution of the 17th Century* (New York, 1964), esp. chap. 1; and David Underdown, *Royalist Conspiracy in England* (New Haven, 1960).

IN TERMS OF THE TRAUMA and the sheer chaos of the time, the aftermath of the American Civil War has few equals in history. After four years of conflict the burden of attempting to achieve a semblance of calm and equanimity was almost unbearable. The revolution in the status of four million slaves involved an incredible readjustment not only for them *and* their former owners but also for all others who had some understanding of the far-reaching implications of emancipation. The crisis in leadership occasioned by the assassination of the president added nothing but more confusion to a political situation that was already thoroughly confused. And, as in all similar conflicts, the end of hostilities did not confer a monopoly of moral rectitude on one side or the other. The ensuing years were characterized by a continuing dispute over whose side was right as well as over how the victors should treat the vanquished. In the post-Reconstruction years a continuing argument raged, not merely over how the victors did treat the vanquished but over what actually happened during that tragic era.

If every generation rewrites its history, as various observers have often claimed, then it may be said that every generation since 1870 has written the history of the Reconstruction era. And what historians have written tells as much about their own generation as about the Reconstruction period itself. Even before the era was over, would-be historians, taking advantage of their own observations or those of their contemporaries, began to speak with authority about the period.

James S. Pike, the Maine journalist, wrote an account of misrule in South Carolina, appropriately called *The Prostrate State*, and painted a lurid picture of the conduct of Negro legislators and the general lack of decorum in the management of public affairs.⁴ Written so close to the period and first published as a series of newspaper pieces, *The Prostrate State* should perhaps not be classified as history at all. But for many years the book was regarded as authoritative—contemporary history at its best.⁵ Thanks to Robert Franklin Durden, we now know that Pike did not really attempt to tell what he saw or even what happened in South Carolina during Reconstruction. By picking and choosing from his notes those events and incidents that supported his argument, he sought to place responsibility for the failure of Reconstruction on the Grant administration and on the freedmen, whom he despised with equal passion.⁶

A generation later historians such as William Archibald Dunning and those who studied with him began to dominate the field. Dunning was faithfully described by one of his students as "the first to make scientific and scholarly investigation of the period of Reconstruction."⁷ Despite this evaluation, he was as unequivocal as the most rabid opponent of Reconstruction in placing upon

⁴ Pike, *The Prostrate State: South Carolina under Negro Government* (New York, 1873).

⁵ See the very favorable comments by Henry Steele Commager in the introduction to a reissue of *The Prostrate State* (New York, 1935).

⁶ Durden, *James Shepherd Pike: Republicanism and the American Negro, 1850-1882* (Durham, N.C., 1957), 214-19.

⁷ Hamilton, "William Archibald Dunning," *Dictionary of American Biography*, 3, pt. 1: 523.

Scalawags, Negroes, and Northern radicals the responsibility for making the unworthy and unsuccessful attempt to reorder society and politics in the South.⁸ His "scientific and scholarly" investigations led him to conclude that at the close of Reconstruction the planters were ruined and the freedmen were living from hand to mouth—whites on the poor lands and "thriftless blacks on the fertile lands."⁹ No economic, geographic, or demographic data were offered to support this sweeping generalization.

Dunning's students were more ardent than he, if such were possible, in pressing their case against Radical Republicans and their black and white colleagues. Negroes and Scalawags, they claimed, had set the South on a course of social degradation, misgovernment, and corruption. This tragic state of affairs could be changed only by the intervention of gallant men who would put principle above everything else and who, by economic pressure, social intimidation, and downright violence, would deliver the South from Negro rule. Between 1900 and 1914 these students produced state studies and institutional monographs that gave more information than one would want about the complexion, appearance, and wearing apparel of the participants and much less than one would need about problems of postwar adjustment, social legislation, or institutional development.¹⁰

Perhaps the most important impact of such writings was the influence they wielded on authors of textbooks, popular histories, and fiction. James Ford Rhodes, whose general history of the United States was widely read by contemporaries, was as pointed as any of Dunning's students in his strictures on Reconstruction: "The scheme of Reconstruction," he said, "pandered to ignorant negroes, the knavish white natives, and the vulturous adventurers who flocked from the North. . . ."¹¹ Thomas Dixon, a contemporary writer of fiction, took the findings of Rhodes's and Dunning's students and made the most of them in his trilogy on Civil War and Reconstruction. In *The Clansman*, published in 1905, he sensationalized and vulgarized the worst aspects of the Reconstruction story, thus beginning a lore about the period that was dramatized in *Birth of a Nation*, the 1915 film based on the trilogy, and popularized in 1929 by Claude Bowers in *The Tragic Era*.¹²

Toward the end of its most productive period the Dunning school no longer held a monopoly on the treatment of the Reconstruction era. In 1910 W. E. B. DuBois published an essay in the *American Historical Review* entitled, significantly, "Reconstruction and Its Benefits." DuBois dissented from the prevailing view by suggesting that something good came out of Reconstruction, such as educa-

⁸ Dunning, *Reconstruction, Political and Economic, 1865-1877* (New York, 1907), 116, 120, 121, 213.

⁹ Walter L. Fleming, ed., *Documentary History of Reconstruction: Political, Military, Social, Religious, Educational, and Industrial, 1865-1906*, 1 (New York, 1966): 267.

¹⁰ For some of the best examples of the work of Dunning's students, see Walter L. Fleming, *Civil War and Reconstruction in Alabama* (New York, 1905); and Joseph G. de Roulhac Hamilton, *Reconstruction in North Carolina* (New York, 1914).

¹¹ Rhodes, *History of the United States*, 7 (New York, 1906): 168.

¹² Dixon, *The Leopard's Spots: A Romance of the White Man's Burden* (New York, 1902), *The Clansman: An Historical Romance of the Ku Klux Klan* (New York, 1905), and *The Traitor: A Story of the Rise and Fall of the Invisible Empire* (New York, 1907); and Bowers, *The Tragic Era: The Revolution after Lincoln* (New York, 1929).

tional opportunities for freedmen, the constitutional protection of the rights of all citizens, and the beginning of political activity on the part of the freedmen. In an article published at the turn of the century, he had already hinted "that Reconstruction had a beneficial side," but the later article was a clear and unequivocal presentation of his case.¹³

DuBois was not the only dissenter to what had already become the traditional view of Reconstruction. In 1913 a Mississippi Negro, John R. Lynch, former speaker of the Mississippi House of Representatives and former member of Congress, published a work on Reconstruction that differed significantly from the version that Mississippi whites had accepted. Some years later he argued that a great deal of what Rhodes had written about Reconstruction was "absolutely groundless." He further insisted that Rhodes's account of Reconstruction was not only inaccurate and unreliable but was "the most one-sided, biased, partisan, and prejudiced historical work" that he had ever read.¹⁴ A few years later Alruthus A. Taylor published studies of the Negro in South Carolina, Virginia, and Tennessee, setting forth the general position that blacks during Reconstruction were not the ignorant dupes of unprincipled white men, that they were certainly not the corrupt crowd they had been made out to be, and that their political influence was quite limited.¹⁵

The most extensive and, indeed, the most angry expression of dissent from the well-established view of Reconstruction was made in 1935 by W. E. B. DuBois in his *Black Reconstruction*. "The treatment of the period of Reconstruction reflects," he noted, "small credit upon American historians as scientists." Then he recalled for his readers the statement on Reconstruction that he wrote in an article that the *Encyclopaedia Britannica* had refused to print. In that article he had said, "White historians have ascribed the faults and failures of Reconstruction to Negro ignorance and corruption. But the Negro insists that it was Negro loyalty and the Negro vote alone that restored the South to the Union, established the new democracy, both for white and black, and instituted the public schools."¹⁶ The *American Historical Review* did no better than the *Encyclopaedia Britannica*, since no review of *Black Reconstruction*, the first major scholarly work on Reconstruction since World War I, appeared in the pages of the *Review*. The work was based largely on printed public documents and secondary literature because, the author admitted, he lacked the resources to engage in a full-scale examination of the primary materials¹⁷ and because DuBois thought of his task as the

¹³ DuBois, "The Freedmen's Bureau," *Atlantic Monthly*, 87 (1901): 354-65, and "Reconstruction and Its Benefits," *AHR*, 15 (1909-10): 781-99.

¹⁴ Lynch, *The Facts of Reconstruction* (Boston, 1913), Preface, 92-99 (this entire volume is reprinted in John Hope Franklin, ed., *Reminiscences of an Active Life: The Autobiography of John Roy Lynch* [Chicago, 1970], xxvii-xxxviii), and *Some Historical Errors of James Ford Rhodes* (Boston, 1922), xvii. The latter work originally appeared as two articles in the *Journal of Negro History*: "Some Historical Errors of James Ford Rhodes," 2 (1917): 345-68, and "More about the Historical Errors of James Ford Rhodes," 3 (1918): 139-57. Also see John Garraty, ed., *The Barber and the Historian: The Correspondence of George A. Myers and James Ford Rhodes* (Columbus, Ohio, 1956), 29-38.

¹⁵ Taylor, *The Negro in South Carolina during the Reconstruction* (Washington, 1924), *The Negro in the Reconstruction of Virginia* (Washington, 1926), and *The Negro in Tennessee* (Washington, 1941).

¹⁶ DuBois, *Black Reconstruction: An Essay toward a History of the Part Which Black Folk Played in the Attempt to Reconstruct America, 1860-1880* (New York, 1935), 713.

¹⁷ *Ibid.*, 724.

exposure of the logic, argument, and conclusions of those whose histories of Reconstruction had become a part of the period's orthodoxy. For this task he did not need to delve deeply into the original sources.

From that point on, works on Reconstruction represented a wide spectrum of interpretation. Paul Herman Buck's *Road to Reunion* shifted the emphasis to reconciliation, while works by Horace Mann Bond and Vernon L. Wharton began the program of fundamental and drastic revision.¹⁸ No sooner was revisionism launched, however, than E. Merton Coulter insisted that "no amount of revision can write away the grievous mistakes made in this abnormal period of American history." He then declared that he had not attempted to do so, and with that he subscribed to virtually all of the views that had been set forth by the students of Dunning. And he added a few observations of his own, such as "education soon lost its novelty for most of the Negroes"; they would "spend their last piece of money for a drink of whisky"; and, being "by nature highly emotional and excitable . . . , they carried their religious exercises to extreme lengths."¹⁹

By mid-century, then, there was a remarkable mixture of views of Reconstruction by historians of similar training but of differing backgrounds, interests, and commitments. Some were unwilling to challenge the traditional views of Reconstruction. And, although their language was generally polite and professional, their assumptions regarding the roles of blacks, the nature of the Reconstruction governments in the South, and the need for quick—even violent—counteraction were fairly transparent. The remarkable influence of the traditional view of Reconstruction is nowhere more evident than in a work published in 1962 under the title *Texas under the Carpetbaggers*. The author did not identify the carpetbaggers, except to point out that the governor during the period was born in Florida and migrated to Texas in 1848 and that the person elected to the United States Senate had been born in Alabama and had been in Texas since 1830.²⁰ If Texas was ever under the carpetbaggers, the reader is left to speculate about who the carpetbaggers were! Meanwhile, in the 1960s one of the most widely used college textbooks regaled its readers about the "simple-minded" freedmen who "insolently jostled the whites off the sidewalks into the gutter"; the enfranchisement of the former slaves set the stage for "stark tragedy," the historian continued, and this was soon followed by "enthroned ignorance," which led inevitably to "a carnival of corruption and misrule."²¹ Such descriptions reveal more about the author's talent for colorful writing than about his commitment to sobriety and accuracy.

Yet an increasing number of historians began to reject the traditional view and to argue the other side or, at least, to insist that there was another side. Some took another look at the states and rewrote their Reconstruction history. In the new version of Reconstruction in Louisiana the author pointed out that

¹⁸ Buck, *The Road to Reunion* (Boston, 1937); Bond, *Negro Education in Alabama: A Study in Cotton and Steel* (Washington, 1939); and Wharton, *The Negro in Mississippi* (Chapel Hill, 1947).

¹⁹ Coulter, *The South during Reconstruction* (Baton Rouge, 1947), xi, 86, 336.

²⁰ W. L. Nunn, *Texas under the Carpetbaggers* (Austin, 1962), 19, 25n.

²¹ Thomas A. Bailey, *The American Pageant: A History of the Republic* (Boston, 1961), 475–76.

"the extravagance and corruption for which Louisiana Reconstruction is noted did not begin in 1868," for the convention of 1864 "was not too different from conventions and legislatures which came later."²² Others looked at the condition of the former slaves during the early days of emancipation and discovered that blacks faced freedom much more responsibly and successfully than had hitherto been described. Indeed, one student of the problem asserted that "Reconstruction was for the Negroes of South Carolina a period of unequaled progress."²³ Still others examined institutions ranging from the family to the Freedmen's Savings Bank and reached conclusions that were new or partly new to our understanding of Reconstruction history.²⁴ Finally, there were the syntheses that undertook, unfortunately all too briefly, to make some overall revisionist generalizations about Reconstruction.²⁵

UP TO THIS POINT my observations have served merely as a reminder of what has been happening to Reconstruction history over the last century. I have not intended to provide an exhaustive review of the literature. There have already been extensive treatments of the subject, and there will doubtless be more.²⁶ Reconstruction history has been argued over and fought over since the period itself ended. Historians have constantly disagreed not only about what significance to attach to certain events and how to interpret them but also (and almost as much) about the actual events themselves. Some events are as obscure and some facts are apparently as unverifiable as if they dated from several millennia ago. Several factors have contributed to this state of affairs. One factor, of course, is the legacy of bitterness left behind by the internal conflict. This has caused the adversaries—and their descendants—to attempt to place the blame on each other (an understandable consequence of a struggle of this nature). Another factor is that the issues have been delineated in such a way that the merits in the case have tended to be all on one side. A final factor has been the natural inclination of historians to pay attention only to those phases or aspects of the period that give weight to the argument presented. This inclination may involve the

²² Joe Gray Taylor, *Louisiana Reconstructed, 1863-1877* (Baton Rouge, 1974), 49.

²³ Joel Williamson, *After Slavery: The Negro in South Carolina during Reconstruction, 1861-1877* (Chapel Hill, 1965), 63. Also see Roberta Sue Alexander, "North Carolina Faces the Freedmen: Race Relations during Presidential Reconstruction, 1865-1867" (Ph.D. dissertation, University of Chicago, 1974).

²⁴ For examples of such work, see Herbert G. Gutman, *The Black Family in Slavery and Freedom, 1750-1925* (New York, 1976); John W. Blassingame, *Black New Orleans, 1860-1880* (Chicago, 1973); and Carl R. Osthaus, *Freedmen, Philanthropy, and Fraud: A History of the Freedmen's Savings Bank* (Urbana, Ill., 1976).

²⁵ Rembert Patrick, *The Reconstruction of the Nation* (New York, 1967); Kenneth M. Stampp, *The Era of Reconstruction, 1867-1877* (New York, 1965); and John Hope Franklin, *Reconstruction after the Civil War* (Chicago, 1961).

²⁶ See, for example, A. A. Taylor, "Historians of the Reconstruction," *Journal of Negro History*, 23 (1938): 16-34; Francis B. Simkins, "New Viewpoints of Southern Reconstruction," *Journal of Southern History*, 5 (1939): 49-61; Howard K. Beale, "On Rewriting Reconstruction History," *AHR*, 45 (1939-40): 807-27; T. Harry Williams, "An Analysis of Some Reconstruction Attitudes," *Journal of Southern History*, 12 (1946): 469-86; Bernard A. Weisberger, "The Dark and Bloody Ground of Reconstruction Historiography," *ibid.*, 25 (1959): 427-47; Vernon L. Wharton, "Reconstruction," in Arthur S. Link and Rembert W. Patrick, eds., *Writing Southern History: Essays in Historiography in Honor of Fletcher M. Green* (Baton Rouge, 1965), 295-315; and John Hope Franklin, "Reconstruction and the Negro," in Harold M. Hyman, ed., *New Frontiers of the American Reconstruction* (Urbana, Ill., 1966), 59-76.

omission of any consideration of the first two years of Reconstruction in order to make a strong case against, for example, the Radicals. Perhaps such an approach has merit in a court of law or in some other forum, but as an approach to historical study its validity is open to the most serious question.

Perhaps an even more important explanation for the difficulty in getting a true picture of Reconstruction is that those who have worked in the field have been greatly influenced by the events and problems of the period in which they were writing. That first generation of students to study the postbellum years "scientifically" conducted its research and did its writing in an atmosphere that made the conclusions regarding Reconstruction foregone. Different conclusions were inconceivable.²⁷ Writing in 1905 Walter L. Fleming referred to James T. Rapier, a Negro member of the Alabama constitutional convention of 1867, as "Rapier of Canada." He then quoted Rapier as saying that the manner in which "colored gentlemen and ladies were treated in America was beyond his comprehension."²⁸

Born in Alabama in 1837, Rapier, like many of his white contemporaries, went North for an education. The difference was that instead of stopping in the northern part of the United States, as, for example, William L. Yancey did, Rapier went on to Canada. Rapier's contemporaries did not regard him as a Canadian; and, if some were not precisely clear about where he was born (as was the *Alabama State Journal*, which referred to his birthplace as Montgomery rather than Florence), they did not misplace him altogether.²⁹ In 1905 Fleming made Rapier a Canadian because it suited his purposes to have a bold, aggressive, "impertinent" Negro in Alabama Reconstruction come from some non-Southern, contaminating environment like Canada. But it did not suit his purposes to call Yancey, who was a graduate of Williams College, a "Massachusetts Man." Fleming described Yancey as, simply, the "leader of the States Rights men."³⁰

Aside from his Columbia professors, Fleming's assistance came largely from Alabamians: Thomas M. Owen of the Department of Archives and History, G. W. Duncan of Auburn, W. W. Screws of the *Montgomery Advertiser*, and John W. Du Bose, Yancey's biographer and author of *Alabama's Tragic Decade*.³¹ At the time that Fleming sought their advice regarding his Reconstruction story, these men were reaping the first fruits of disfranchisement, which had occurred in Alabama in 1901. Screws's *Advertiser* had been a vigorous advocate of disfranchisement, while Du Bose's *Yancey*, published a decade earlier, could well have been a campaign document to make permanent the redemption of Alabama from "Negro-carpetbagger-Scalawag rule."³² It is inconceivable that such

²⁷ For a discussion of the impact of the scientific study of history on research and writing, see W. Stull Holt, "The Idea of Scientific History in America," in his *Historical Scholarship in the United States and Other Essays* (Seattle, 1967), 15-28.

²⁸ Fleming, *Civil War and Reconstruction in Alabama*, 523. Fleming knew better, for in another place—deep in a footnote—he asserted that Rapier was from Lauderdale, "educated in Canada"; *ibid.*, 519n.

²⁹ Loren Schweninger, *James T. Rapier and Reconstruction* (Chicago, 1978), xvii, 15.

³⁰ Fleming, *Civil War and Reconstruction in Alabama*, 12. For an account of Yancey and other white Southerners in the North to secure an education, see John Hope Franklin, *A Southern Odyssey: Travelers in the Antebellum North* (Baton Rouge, 1976), 45-80.

³¹ Fleming, *Civil War and Reconstruction in Alabama*, viii-ix; and Du Bose, *Alabama's Tragic Decade, 1865-1874* (Birmingham, Ala., 1940). Du Bose's work is a collection of his newspaper articles published in 1912.

³² Du Bose, *The Life and Times of William Lowndes Yancey* (Birmingham, Ala., 1892), 407-22.

men would have assisted a young scholar who had any plans except to write an account of the Reconstruction era that would support their views. In any case they could not have been more pleased had they written Fleming's work for him.

But the "scientific" historians might well have been less pleased if they had not been caught up in the same pressures of the contemporary scene that beset Fleming. They, like Fleming, should have been able to see that some of the people that Fleming called "carpetbaggers" had lived in Alabama for years and were, therefore, entitled to at least as much presumption of assimilation in moving from some other state to Alabama decades before the war as the Irish were in moving from their native land to some community in the United States. Gustavus Horton, a Massachusetts "carpetbagger" and chairman of the constitutional convention's Committee on Education in 1867, was a cotton broker in Mobile and had lived there since 1835. Elisha Wolsey Peck, the convention's candidate for chief justice in 1867, moved to Alabama from New York in 1825. A few months' sojourn in Illinois in 1867 convinced Peck that the only real home he could ever want was Alabama. Charles Mayes Cabot, a member of the constitutional convention of 1865 as well as of the one of 1867, had come to Alabama from his native Vermont as a young man. He prospected in the West in 1849 but was back in Wetumka in the merchandising business by 1852.³³ Whether they had lived in Alabama for decades before the Civil War or had settled there after the war, these "carpetbaggers" were apparently not to be regarded as models for Northern investors or settlers in the early years of the twentieth century. Twentieth-century investors from the North were welcome provided they accepted the established arrangements in race relations and the like. Fleming served his Alabama friends well by ridiculing carpetbaggers, even if in the process he had to distort and misrepresent.

In his study of North Carolina Reconstruction published in 1914, Joseph G. de Roulhac Hamilton came as close as any of his fellow historians to reflecting the interests and concerns of his own time. After openly bewailing the enfranchisement of the freedmen, the sinister work of the "mongrel" convention and legislatures, and the abundance of corruption, Hamilton concluded that Reconstruction was a crime that is "to-day generally recognized by all who care to look the facts squarely in the face." But for Reconstruction, he insisted, "the State would to-day, so far as one can estimate human probabilities, be solidly Republican. This was clearly evident in 1865, when the attempted restoration of President Johnson put public affairs in the hands of former Whigs who then had no thought of joining in politics their old opponents, the Democrats." Hamilton argued that in his own time some men who regularly voted the Democratic ticket would not call themselves "Democrats." In an effort to appeal to a solid Negro vote, the Republicans had lost the opportunity to bring into their fold large numbers of former Whigs and some disaffected Democrats. In the long run the Republicans gained little, for the Negroes, who largely proved to

³³ Thomas McAdory Owen, *History of Alabama and Dictionary of Alabama Biography*, 4 vols. (Chicago, 1921), 2: 845-46, 4: 1335, 3: 278. For a discussion of the problem of defining carpetbaggers in Alabama, see Bond, *Negro Education in Alabama*, 65.

be "lacking in political capacity and knowledge, were driven, intimidated, bought, and sold, the playthings of politicians, until finally their so-called right to vote became the sore spot of the body politic."³⁴ In his account of Reconstruction, which placed the blame on the Republican-Negro coalition for destroying the two-party system in North Carolina, Hamilton gave a warning to his white contemporaries to steer clear of any connection with blacks whose votes could be bought and sold if the franchise were again extended to them.

And the matter was not only theoretical. In 1914, while Hamilton was writing about North Carolina Reconstruction, Negro Americans were challenging the several methods by which whites had disfranchised them, and Hamilton was sensitive to the implications of the challenge. He reminded his readers that, after the constitutional amendment of 1900 restricting the suffrage by an educational qualification and a "grandfather clause," the Democrats elected their state ticket. His eye was focused to a remarkable degree on the current political and social scene. "The negro has largely ceased to be a political question," he commented, "and there is in the State to-day as a consequence more political freedom than at any time since Reconstruction."³⁵ The lesson was painfully clear to him, as he hoped it would be to his readers: the successful resistance to the challenges that Negroes were making to undo the arrangements by which they had been disfranchised would remove any fears that whites might have of a repetition of the "crime" of Reconstruction. Segregation statutes, the white Democratic primary, discrimination in educational opportunities, and, if necessary, violence were additional assurances that there would be no return to Reconstruction.

UNFORTUNATELY, THE PERSISTENCE OF THE DISPUTE over what actually happened during Reconstruction and the use of Reconstruction fact and fiction to serve the needs of writers and their contemporaries have made getting at the truth about the so-called Tragic Era virtually impossible. Not only has this situation deprived the last three generations of an accurate assessment of the period but it has also unhappily strengthened the hand of those who argue that scientific history can be as subjective, as partisan, and as lacking in discrimination as any other kind of history. A century after the close of Reconstruction, we are utterly uninformed about numerous aspects of the period. Almost forty years ago Howard K. Beale, writing in the *American Historical Review*, called for a treatment of the Reconstruction era that would not be marred by bitter sectional feelings, personal vendettas, or racial animosities.³⁶ In the four decades since that piece was written, there have been some historians who have heeded Beale's call. It would, indeed, be quite remarkable if historians of today were not sensitive to some of the strictures Beale made against those who kept alive the hoary myths about Reconstruction and if scholars of today's generation did not attempt to

³⁴ Hamilton, *Reconstruction in North Carolina*, 663.

³⁵ *Ibid.*, 666-67.

³⁶ Beale, "On Rewriting Reconstruction History," 807-27.

look at the period without the restricting influences of sectional or racial bias. And yet, since the publication of Beale's piece, several major works have appeared that are aggressively hostile to any new view of Reconstruction.³⁷ Nor has Beale's call been heeded to the extent that it should have been.

If histories do indeed reflect the problems and concerns of their authors' own times, numerous major works on Reconstruction should have appeared in recent years. After all, since the close of World War II this nation has been caught up in a reassessment of the place of Negroes in American society, and some have even called this period the "Second Reconstruction."³⁸ Central to the reassessment has been a continuing discussion of the right of blacks to participate in the political process, to enjoy equal protection of the laws, and to be free of discrimination in education, employment, housing, and the like. Yet among the recent writing on Reconstruction few major works seek to synthesize and to generalize over the whole range of the freedmen's experience, to say nothing of the problem of Reconstruction as a whole. Only a limited number of monographic works deal with, for example, Reconstruction in the states, the regional experiences of freedmen, the freedmen confronting their new status, aspects of educational, religious, or institutional development, or phases of economic adjustment.

In recent years historians have focused much more on the period of slavery than on the period of freedom. Some historians have been most enthusiastic about the capacity of slaves to establish and maintain institutions while in bondage, to function effectively in an economic system as a kind of upwardly mobile group of junior partners, and to make the transition to freedom with a minimum of trauma.³⁹ One may wonder why, at this particular juncture in the nation's history, slavery has attracted so much interest and why, in all of the recent and current discussions of racial equality, Reconstruction has attracted so little. Not even the litigation of *Brown v. The Board of Education*, which touched off a full-dress discussion of one of the three Reconstruction Amendments a full year before the decision was handed down in 1954, stimulated any considerable production of Reconstruction scholarship.⁴⁰ Does this pattern suggest that historians have thought that the key to understanding the place of Afro-Americans in American life is to be found in the slave experience and not in the struggles for adjustment in the early years of freedom? Or does it merely mean that historians find the study of slavery more exotic or more tragic and therefore more at-

³⁷ See, for example, Coulter, *The South during Reconstruction*; and Bailey, *The American Pageant*, chap. 24.

³⁸ See C. Vann Woodward, "The Political Legacy of Reconstruction," in his *The Burden of Southern History* (New York, 1961), 107.

³⁹ For some of the works that deal with these themes, see John W. Blassingame, *The Slave Community: Plantation Life in the Antebellum South* (New York, 1972); Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974); Robert William Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 2 vols. (Boston, 1974); Gutman, *The Black Family in Slavery and Freedom*; and David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, N.Y., 1966), and *The Problem of Slavery in the Age of Revolution, 1770-1823* (Ithaca, N.Y., 1975).

⁴⁰ A few who were associated with counsel for the plaintiffs have published some of their work. See, for example, Alfred H. Kelly, "The Congressional Controversy over School Segregation, 1867-1875," *AHR*, 64 (1958-59): 537-63; and John Hope Franklin, "Jim Crow Goes to School: The Genesis of Legal Segregation in Southern Schools," *South Atlantic Quarterly*, 58 (1959): 225-35.

tractive than the later period of freedom? Whatever the reason, the result has been to leave the major thrust of the Reconstruction story not nearly far enough from where it was in 1929, when Claude Bowers published *The Tragic Era*.

That result is all the more unfortunate in view of what we already know and what is gradually and painfully becoming known about the period following the Civil War. With all of the exhortations by Howard Beale, Bernard Weisberger, and others about the need for more Reconstruction studies, the major works with a grand sweep and a bold interpretation have yet to be written. Recent works by Michael Perman and Leon F. Litwack, which provide a fresh view respectively of political problems in the entire South and of the emergence of the freedman throughout the South, are indications of what can and should be done in the field.⁴¹ And, even if the battle for revision is being won among the professionals writing the monographs (if not among the professionals writing the textbooks), it is important to make certain that the zeal for revision does not become a substitute for truth and accuracy and does not result in the production of works that are closer to political tracts than to histories.

Although it is not possible to speak with certainty about the extent to which the Reconstruction history written in our time reveals the urgent matters with which we are regularly concerned, we must take care not to permit those matters to influence or shape our view of an earlier period. That is what entrapped earlier generations of Reconstruction historians who used the period they studied to shape attitudes toward problems they confronted. As we look at the opportunities for new syntheses and new interpretations, we would do well to follow Thomas J. Pressly's admonition not to seek confirmation of our views of Reconstruction in the events of our own day.⁴² This caveat is not to deny the possibility of a usable past, for to do so would go against our heritage and cut ourselves off from human experience.⁴³ At the same time it proscribes the validity of reading into the past the experiences of the historian in order to shape the past as he or she wishes it to be shaped.

The desire of some historians to use the Reconstruction era to bolster their case in their own political arena or on some other ground important to their own well-being is a major reason for our not having a better general account of what actually occurred during Reconstruction. To illustrate this point, we are still without a satisfactory history of the role of the Republican Party in the South during Reconstruction. If we had such a history, we would, perhaps, modify our view of that party's role in the postbellum South. We already know, for example, that the factional fights within the party were quite divisive. The bitter fight between two factions of Republicans in South Carolina in 1872 is merely one case in point. On that occasion the nominating convention split in two and each faction proceeded to nominate its own slate of officers. Only the absence of any opposition party assured a Republican victory in the autumn

⁴¹ Perman, *Reunion without Compromise: The South and Reconstruction, 1865-1868* (Cambridge, 1973); and Litwack, *Been in the Storm So Long: The Aftermath of Slavery* (New York, 1979).

⁴² Pressly, "Racial Attitudes, Scholarship, and Reconstruction: A Review Essay," *Journal of Southern History*, 32 (1966): 90.

⁴³ J. R. Pole, "The American Past: Is It Still Usable?" *Journal of American Studies*, 1 (1967): 70-72.

elections.⁴⁴ In some instances blacks and whites competed for the party's nomination to public office, thus indicating quite clearly the task facing a Negro Republican who aspired to public office.⁴⁵ That is the task that John R. Lynch faced when he ran for Congress in 1872 and defeated the white incumbent, L. W. Pearce, who was regarded even by Lynch as "a creditable and satisfactory representative."⁴⁶ And it was not out of the question for white Republicans to work for and vote for white Democrats in order to make certain that Negro Republican candidates for office would be defeated.⁴⁷ So little is known of the history of the Republican Party in the South because the presumption has generally been that Lincoln's party was, on its very face, hostile to Southern mores generally and anxious to have Negroes embarrass white Southerners. Indeed, had historians been inclined to examine with greater care the history of the Republican Party in the South, they would have discovered even more grist for the Democratic Party mill.

Thus, studying works on Reconstruction that have been written over the last century can provide a fairly clear notion of the problems confronting the periods in which the historians lived but not always as clear a picture of Reconstruction itself. The state of historical studies and the level of sophistication in the methods of research are much too advanced for us to be content with anything less than the high level of performance found in works on other periods of United States history. There is no reason why the facts of Reconstruction should be the subject of greater dispute than those arising out of Askia Muhammad's rule in Songhay or Cromwell's rule in Britain. But we are still doing the spade-work; we are still writing narrowly focused monographs on the history of Reconstruction. We need to know more about education than Henry L. Swint, Horace Mann Bond, and Robert Morris have told us.⁴⁸ Surely there is more to economic development than we can learn from the works by Irwin Unger, George R. Woolfolk, Robert P. Sharkey, and Carl Osthaus.⁴⁹ And race, looming large in the Reconstruction era, as is usually the case in other periods of American history, is so pervasive and so critical that the matter should not be left to Herbert G. Gutman, Howard Rabinowitz, John H. and La Wanda Cox, Thomas Holt, and a few others.⁵⁰

⁴⁴ Edward F. Sweat, "Francis L. Cardozo—Profile in Reconstruction Politics," *Journal of Negro History*, 46 (1961): 217–32. For examples of other intraparty conflicts, see Robert H. Woody, "Jonathan Jasper Wright, Associate Justice of the Supreme Court of South Carolina, 1870–77," *ibid.*, 18 (1933): 114–31; and Schweninger, *James T. Rapier and Reconstruction*, 75, 144.

⁴⁵ Schweninger, *James T. Rapier and Reconstruction*, 114.

⁴⁶ Franklin, *Reminiscences of an Active Life: The Autobiography of John Roy Lynch*, 101–02.

⁴⁷ Thomas B. Alexander, *Political Reconstruction in Tennessee* (Nashville, 1950), 204–05, 240–41; and Taylor, *Louisiana Reconstructed, 1863–1877*, 214.

⁴⁸ Swint, *Northern Teacher in the South* (Nashville, 1941); Bond, *Negro Education in Alabama*; and Morris, "Reading, 'Ritin', and Reconstruction" (Ph.D. dissertation, University of Chicago, 1976).

⁴⁹ Unger, *The Greenback Era: A Social and Political History of American Finance, 1865–1879* (Princeton, 1965); Woolfolk, *The Northern Merchants and Reconstruction, 1865–1880* (New York, 1958); Sharkey, *Money, Class, and Party: An Economic Study of Civil War and Reconstruction* (Baltimore, 1959); and Osthaus, *Freedmen, Philanthropy, and Fraud: A History of the Freedmen's Savings Bank*.

⁵⁰ Gutman, *The Black Family in Slavery and Freedom*; Rabinowitz, *Race Relations in the Urban South* (New York, 1978); Cox and Cox, *Politics, Principles, and Prejudice, 1865–1866: Dilemma of Reconstruction America* (New York, 1963); and Holt, *Black over White: Negro Political Leadership in South Carolina during Reconstruction* (Urbana, Ill., 1977).

RECENT SCHOLARSHIP ON THE RECONSTRUCTION ERA leaves the impression that we may be reaching the point, after a century of effort, where we can handle the problems inherent in writing about an internal struggle without losing ourselves in the fire and brimstone of the Civil War and its aftermath. Perhaps we have reached the point in coping with the problems about us when we no longer need to shape Reconstruction history to suit our current needs. If either or both of these considerations is true, we are fortunate, for each augurs well for the future of Reconstruction history. It would indeed be a happy day if we could view the era of Reconstruction without either attempting to use the events of that era to support some current policy or seeking analogies that are at best strained and provide little in the way of an understanding of that era or our own.

"Not since Reconstruction" is a phrase that is frequently seen and heard. Its principal purpose is to draw an analogy or a contrast. Since it usually neither defines Reconstruction nor makes clear whether it is a signpost of progress or retrogression, searching for some other way of relating that period to our own may be wise, if not necessary. In the search for the real meaning of Reconstruction, phrases like "not since Reconstruction" provide no clue to understanding the period. Worse still, they becloud the relationship between that day and this. To guard against the alluring pitfalls of such phrases and to assure ourselves and others that we are serious about the postbellum South, we would do well to cease using Reconstruction as a mirror of ourselves and begin studying it because it very much needs studying. In such a process Reconstruction will doubtless have much to teach all of us.

From the Land of Canaan to the Land of Guinea: The Strange Odyssey of the "Sons of Ham"

WILLIAM MCKEE EVANS

[Noah] became drunk and lay uncovered in his tent. And Ham, the father of Canaan, saw the nakedness of his father and told his two brothers outside. Then Shem and Japheth took a garment, laid it upon both their shoulders, and walked backward and covered the nakedness of their father; their faces were turned away, and they did not see their father's nakedness. When Noah awoke from his wine and knew what his youngest son had done to him he said,

"Cursed be Canaan;
a slave of slaves shall he be to his brothers."

He also said,

"Blessed by the Lord my God be Shem;
and let Canaan be his slave.
God enlarge Japheth,
and let him dwell in the tents of Shem;
and let Canaan be his slave."¹

THIS STRANGE STORY HAS TROUBLED THEOLOGIANs for perhaps two thousand years. Since it is first found in the Old Testament, a common source of the Jewish, Christian, and Muslim traditions, moralists have understandably expected to find in it some illustration of ethics or justice. Yet the story offends our moral sensibilities at almost every point. In the first place, even when the extreme sensitivity that the ancient Hebrews are supposed to have had about nudity is taken into account, Noah's curse dwarfs Ham's offense. By any ordinary standard, ancient or modern, Noah overreacted. Second, the curse fell not upon the offender at all, but upon Canaan, a son of Ham, who, in terms of the story, is

I have benefited greatly from the discussions that have followed the presentation of earlier versions of this article to the California State Polytechnic University, Pomona, chapter of Phi Alpha Theta, to the Southern California History Guild, and to the 1977 National Endowment for the Humanities seminar, "Development of Institutions and Ideology in Colonial America," held at Northwestern University. Also, because the subject covers many areas of specialization, I have requested and received readings from an unusually large number of colleagues. Especially important suggestions have been offered by Timothy H. Breen, Richard Sigwalt, George M. Fredrickson, Daniel Pipes, L. Carl Brown, William H. McNeill, James M. McPherson, Herbert Shapiro, Eric Foner, Otto Olsen, David B. Davis, George B. Tindall, Thomas Reeve, Jerome Van Camp, Charles Verlinden, Bernard Lewis, Hans Ruyter, and Richard D. Weis; and I appreciate their comments.

¹ Gen 9:21-27 (Revised Standard Version). All succeeding biblical references are enclosed in parentheses in the text; they are from the RSV unless otherwise noted.

totally innocent. Over the centuries theologians have often held ideas about nudity and respect for parents similar to those of the ancient Hebrews. Yet they have consistently failed to find in Ham's behavior sufficient justification for Noah's curse upon Canaan.²

The story of Ham presents far fewer difficulties, however, if it is considered not as a moral tale but as a sacred myth. One of the characteristics of myths, including sacred ones, is their amorality. In these accounts, divine or semidivine beings often commit deeds that violate or transcend the moral restraints of the ordinary people who believe in the myth.³ An ethnographical study of northern Ghana has shown that, among the Tallensi people, the most elaborate myths are those that interpret situations of sharp social polarization, that interpret the most tension-producing areas of their society.⁴ If such a principle has wide application, it would suggest that the Curse of Noah, which was molded and elaborated for more than two thousand years, has functioned to interpret an area of continuing social tension. This inquiry explores historically this area of tension in an effort to determine why certain peoples have come to be regarded as heirs to Noah's curse and others as heirs to his blessing, destined to have their needs served by ethnic inferiors. By studying the shifting ethnic identifications of the "sons of Ham," by following their journey in myth from the land of Canaan to the land of Guinea, we can perhaps learn something about the historical pressures that shaped modern white racial attitudes.

MOST SCHOLARS BELIEVE that the story of Noah's curse, in its biblical form, is a product of the kingdom of David and Solomon and thus reflects the circumstances of the Hebrews in the tenth century B.C. The Hebrews believed themselves and several neighboring peoples to be "Semites"—that is, descendants of one of the heroes of the tale, "Shem" (Gen. 10:22 ff.), who, in the various linguistic forms of his name, is also called "Sem" and "Sam." About 1200 B.C., their nomadic ancestors from the eastern desert had invaded the "land of Canaan," an invasion that coincided with one from the west by the Sea People, or Philistines. By the tenth century the Canaanites, the presumed descendants of the individual upon whom the curse had fallen, had been conquered, and many of them had been annihilated or enslaved. Yet it had become apparent that the victorious Hebrews would have to share the land and the Canaanites with those

² For efforts of critical scholars to solve some of the riddles posed by the story, see *The Anchor Bible: Genesis*, introduction, translation, and notes by E. A. Speiser (Garden City, N.Y., 1964), s.v. "Noah and His Sons," 60-63; *Encyclopedia of Islam*, s.v. "Ham," by A. Vajda; "Nuh [Noah]," by Bernard Heller. For the possible sexual implications of the Hebrew word *'erwat* (nakedness) see F. W. Basset, "Noah's Nakedness and the Curse of Canaan: A Case of Incest?" *Vetus Testamentum*, 21 (1971): 232-39. Interpretations of this kind go back at least to the rabbinical literature of the fourth to the sixth centuries. See *b. Sanhedrin*, 70a (Isidore Epstein, ed., *The Babylonian Talmud*, 35 vols. [London, 1935-52], 28: 477); English-language editions are cited in parentheses following the standard rabbinical reference. For historical interpretations, see note 5, below.

³ *International Encyclopedia of the Social Sciences*, s.v. "Myth and Symbol," by Victor W. Turner, 577.

⁴ Meyer Fortes, *The Dynamics of Clanship among the Tallensi* (New York, 1945), 26. For the relationship between myth and social tension, also see B. Malinowski, *Myth in Primitive Psychology* (New York, 1926), 58-59; and E. R. Leach, *Political Systems of Highland Burma: A Study of Kachin Social Structure* (Cambridge, Mass., 1954), 277-78.

other conquerors, the Philistines, who thus become in myth the sons of Japheth, dwelling "in the tents of Shem."⁵ If the story, despite its incorporation in the Jewish and Christian canons, seems to lack what many later readers liked to think of as the essential elements of divine justice, it at least has the merit of explaining events of history, which often seem to lack the essential elements of divine justice. The tradition provided a militarily successful people with religious sanction. It also provides an example of how myth translates the complex events of history into simple personal terms.

The story seems to say that all mankind is descended from the household of Noah, the single family that survived the Flood, and that one branch of the family of man is fated to perpetual slavery. In its original form, however, Noah's curse seems to justify the enslavement of a people, but a people who were racially indistinguishable from their conquerors. Indeed, according to the biblical account, the enslavers and the enslaved were descended from brothers. As yet the tradition showed no color bias.⁶ The Hebrews of the first millennium B.C., like other ancient peoples, did not associate slavery with any particular race. But, if ancient slavery was racially nonspecific, it was by no means ethnically so. There were always some peoples considered more appropriate for enslavement than others.⁷ For the purpose of understanding the rise of modern Western racial prejudices, it is important to consider the historical process whereby a people acquires or loses a slavish reputation, whereby slavery acquires or loses an ethnic identification.

The history of ancient Israel brings some light to this process, because the slavish reputation of the conquered Canaanites was the result of the position they occupied within the newly created Hebrew kingdom. David, Solomon, and their successors ruled an ethnically stratified society in which the ethnic distribution of power was celebrated by the Hamitic myth. The practice of slavery was by no means new to the Israelites during the period of the monarchy, nor had their modest conquests created vast armies of slaves such as those that flowed into the late Roman Republic. Yet the increase in slaveholding was sufficient to influence their lives and their thinking in a number of ways. "Canaanite slavery," for example, became the most debased form of exploitation that they knew, transmitting to each successive generation the meaning of the conquest and underscoring a difference in status between persons descended from the conqueror and those descended from the vanquished.

⁵ Gerhard von Rad, *Genesis: A Commentary*, trans. John Marks (rev. ed., London, 1972), 134-35. For a summary of scholarly opinions on the dating of the story, see Claus Westermann, *Genesis*, vol. 1: *Genesis, 1-11* (Neukirchen-Vluyn, Germany, 1974), 656-57.

⁶ See Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro, 1550-1812* (Chapel Hill, 1968), 18; and Ephraim Isaac, "Biblical and Rabbinic Understanding of the Curse of Noah," *Sidic: Journal of the Service international de documentation judéo-chrétienne*, 11 (1978): 25. For racial perceptions in the biblical and Greco-Roman worlds, see R. A. Bennett, Jr., "Africa and the Biblical Period," *Harvard Theological Review*, 64 (1971): 489-90; and Frank M. Snowden, Jr., *Blacks in Antiquity: Ethiopians in the Greco-Roman Experience* (Cambridge, Mass., 1970).

⁷ Eduard Meyer, *Die Sklaverei im Altertum* (Dresden, 1898), 10-11; Henri Levy-Bruhl, "Esquisse d'une théorie sociologique de l'esclavage à Rome," in *Slavery in Classical Antiquity: Views and Controversies*, ed. M. I. Finley (Cambridge, 1960), 152-55; and M. I. Finley, "Was Greek Civilisation Based on Slave Labour?" in *ibid.*, 60-61.

Furthermore, Canaanite slavery changed power relationships within the Israelite community, increasing the hierarchical character of society. Previously, the Israelites had had neither true kings nor state slavery, which involved the harshest use of captives. Ironically, state slavery not only further degraded the reputation of the vanquished but also enabled the kings to function with a certain despotic independence that would not have been possible for the earlier leaders, whose authority had been derived from ties of religion and kinship with their followers. Royal captives worked in state enterprises such as the copper mines and refineries of 'Arabah, which gave the king an income for which he did not have to answer to his subjects. This enabled him to hire foreign mercenary troops, making him less dependent upon the popular levy (1 Chron. 18:11; 2 Sam. 20:23).⁸ There were also the *netinim*, the temple slaves, descendants of war captives, "hewers of wood and drawers of water for the congregation" (Josh. 9:21–27; also see Num. 31:26–47). The *netinim* functioned to enhance the power and prestige of the state religion.

Had the reputation of the Canaanites depended upon state and temple slavery, it probably would not have survived the downfall of the monarchy and the destruction of the first Temple. But many more Canaanites served in Hebrew homes, and household slavery continued to be a feature of Jewish communities for more than two thousand years. Household slaves probably received considerably better treatment than could be expected in the royal slave gangs. Their treatment, furthermore, unlike that of either state or temple slaves, was governed by biblical injunctions. These regulations reflected the ethnic distribution of power established by the conquest of Canaan. A type of servitude existed even for Hebrews. Because of poverty, or because he had been convicted of certain crimes, a Hebrew might lose his freedom. But this did not make him an *eved Kanaani*, a Canaanite slave. The laws and customs of the conquerors protected even their least successful or most delinquent members. Legally, he could not be sold against his will to a non-Hebrew. He could not be required to do certain types of work that were considered degrading (Deut. 25:39–40). He had legal recourse if mistreated. He had the right to be freed at the end of a six-year term (Deut. 15:12–18). Though these servants were *eved Ivri*, sometimes translated as "Hebrew slaves," their actual status more closely resembled that of the indentured servants of the seventeenth-century English colonies. The real slaves held by Hebrew masters were the "Canaanite slaves," whose legal enslavement was compounded by ethnic debasement (Lev. 25:29–46).⁹

⁸ Nelson Gluck, "The Boundaries of Edom," *Hebrew Union College Annual*, 11 (1936): 148–49; Isaac Mendelsohn, *Slavery in the Ancient Near East: A Comparative Study of Slavery in Babylonia, Assyria, Syria, and Palestine from the Middle of the Third Millennium to the End of the First Millennium* (New York, 1949), 96; and Roland de Vaux, *Ancient Israel: Its Life and Institutions*, trans. John McHugh, (New York, 1961), 89, 95, 123, 129.

⁹ E. E. Urbach, "The Laws regarding Slavery as a Source for Social History of the Period of the Second Temple, the Mishnah, and Talmud," in Institute of Jewish Studies, University of London, *Papers of the Institute*, 1, ed. J. G. Weiss (Jerusalem, 1964): 26–27; de Vaux, *Ancient Israel*, 80–83; David Farbstein, *Das Recht der unfreien und der freien Arbeiter nach jüdisch-talmudischem Recht* (Bern, 1896), 9–11; Zadoc Kahn, *L'esclavage selon la Bible et le Talmud* (Paris, 1867), 198–99; Simon Rubin, "Ein Kapitel aus der Sklaverei im talmudischen und römischen Rechte," in S. Krauss, ed., *Festschrift für Adolf Schwartz zum siebenzigsten Geburtstag, 15 Juli 1916* (Berlin, 1917), 212; and Mendelsohn, *Slavery in the Ancient Near East*, 105–06, 152.

In most societies it is unnecessary to explain the use of a double standard in the treatment of people of one's own kind and members of some out-group. Outsiders are treated differently because everything about them seems different. Furthermore, they are often perceived as inferior because, since most societies are ethnocentric, outsiders are judged by the only standards that one knows, that is, by the appearance, the speech, the customs of one's own group; and one's eyes and ears tell how badly an outsider falls short of the mark. As long as captives are ethnically distinct from their captors, their discriminatory treatment calls for no further apology.

Yet, as has been characteristic of other slave systems, Hebrew slavery functioned in conflict with its own justifying ideology. On the one hand, because the Canaanites were different, they could be required to do demeaning work, and they could be held to lifelong and hereditary servitude. But, on the other, the daily operation of the slavery system, with intimate contact between slaves and owners, inevitably made the Canaanites less different, as they assimilated the religion, the customs, and speech habits of the dominant group. Inevitably, ties of religion, friendship, and kinship arose between bond and free. Slaveholders occasionally wrote wills freeing their slaves who were sometimes their kinsmen or fellow believers, thereby denying heritable servants to their legitimate children.¹⁰ By one means or another, a number of slaves in each generation ceased to be slaves.

Yet slavery did not disappear, nor did the marginal status of the conquered population disappear. Legalists continued to distinguish between "Canaanite" and "Hebrew" slaves, though these two groups shared the same religion and culture. Throughout the centuries of the monarchs, the descendants of the vanquished remained *gerim*, "sojourners" in the land of their birth. After the Babylonian Captivity they became "proselytes." Their name had changed but their marginal status had not.¹¹ For a conservative process was functioning within Hebrew and, later, Jewish society that inhibited the total assimilation of converts to Jewish religion and culture, a process that maintained ancient distinctions established by distant wars of conquest. To understand how this process functioned we must look beyond the experience of the Jews and take a broader view of slave-holding societies.

THE PROCESS WHEREBY SLAVERY, especially household slavery, becomes ameliorated finds expression on the plane of ideas as paternalism. As the name implies, "paternalism" is associated with the patriarch, the head of a large, hierarchical household. The introduction of slaves into such a household enormously increases the authority of the patriarch in a way that is analogous to the manner

¹⁰ J. Pedersen, *Israel, Its Life and Culture*, trans. Aslang Møller, 1 (London, 1926): 40-41; Jean Juster, *Les Juifs dans L'Empire romain, leur condition juridique, économique, et sociale*, 2 (New York, 1914): 81; Salo Wittmayer Baron, *A Social and Religious History of the Jews*, 16 vols. (1937; 2d ed., rev. and enl., New York, 1952-), 1: 267-70; Urbach, "The Laws regarding Slavery as a Source for Social History," 48, *passim*; and David Farbstein, *Die Stellung der Juden zur Rassen- und Fremdenfrage* (Zurich, 1939), 21-22, 24, 27-28, 33.

¹¹ Pedersen, *Israel, Its Life and Culture*, 40-41; and de Vaux, *Ancient Israel*, 74-76.

in which the acquisition of state slaves by a king enhances his despotic independence. Because slaves lighten household chores, the "real" members of the family are likely to welcome the addition of these fictitious members up until that critical moment when the patriarch awards to a slave something that a free person wants for himself: a particularly competent slave is given a responsibility that a son covets; an adored bastard stands closer to the power of the patriarch than a legitimate child; or a young slave sleeps in a bed that a legal wife thinks is her rightful domain. In brief, the patriarch has created what Marc Henri Piault has called the "scandalous paradox": a person who possesses neither legal rights nor social status has been given preference over one who possesses both.¹² In creating such tensions, a patriarch may merely be indulging his preferences. But, if he chooses, he can always favor a servile over a free member of the household, and this option serves to bring legitimate members of his family under tighter control.

Invariably, the free but dependent members of the patriarchal family, who may be called "legitimists," react to the scandalous paradox. Sometimes the legitimist backlash comes with fearful violence, and the recipients of paternalistic favoritism discover the fragility of such benevolence. In Hebrew tradition the slave woman, Hagar, having become pregnant by Abraham, suffered harsh treatment at the hands of the patriarch's legitimate wife and fled into the wilderness. Abraham did not lift a finger to help her (Gen. 16:1-6). In a more modern slave society there are reports of young concubines having their eyes gouged out by the jealous wives of plantation owners.¹³ Status anxieties, unleashed by the scandalous paradox, may trigger pogroms, lynchings, and race riots. But there is a less dramatic, yet highly important, way that legitimists defend their privileges: it is primarily they who perpetuate tradition, who educate children, who shape status-sustaining ideology. As individuals they may not be as powerful as the patriarch, particularly during his years of greatest vigor. But they outnumber him. The household does not function well without their cooperation. Their ideas tend to be regarded as the more respectable ones of slaveholding societies, while those of the paternalist, if the ideas go beyond empty words, are looked on as eccentric if not subversive.¹⁴ So to any status confusion that the patriarch in his senility may be generating, they reply with the clear-cut claims of the legitimate family. The pretensions of all others are the consequence of sin and abomination. In brief, slave-holding societies are character-

¹² Piault, "Captifs du pouvoir et pouvoir des captifs," in Claude Meillassoux, *L'esclavage en Afrique précoloniale* (Paris, 1975), 333, *passim*.

¹³ Gilberto Freyre, *The Masters and the Slaves: A Study in the Development of Brazilian Civilization*, trans. Samuel Putnam (2d Eng. ed., New York, 1956), 351. Also see Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), 333. In slave and former slave societies, furthermore, the traditionally free population, especially highly insecure groups such as immigrants, finding themselves in a position analogous to free but dependent persons in a servile household, have often embraced the myths and cruelties of legitimacy with all the passion of neophytes.

¹⁴ Thus in 1776, when Henry Laurens of South Carolina considered freeing some of his slaves, he anticipated that he would encounter the opposition of his children and that their views would be sustained by the community. Robert Carter of Virginia did, indeed, arouse the opposition of his sons and neighbors in 1791 by freeing five hundred slaves. See Clement Eaton, *A History of the Old South: The Emergence of a Reluctant Nation* (New York, 1975), 368, 370.

ized by a tension between two *elite*-class, ideological tendencies: paternalism, associated with the patriarch, which functions toward the elimination of slaves as a distinct status group within the hierarchy of the patriarchal family; and legitimacy, associated with the free, dependent members of the household, which incubates traditions, myths, and sexual taboos serving to protect old distinctions of status and privilege.

THIS TENSION BETWEEN PATERNALISM AND LEGITIMACY may explain why Jewish society, despite its strong paternalistic impulse, for centuries held at a distance the *gerim* and proselytes: marginal status groups that shared its faith and culture. In order for free-born but dependent legitimists to maintain a sharp distinction between themselves and such socially marginal groups as slaves, they either had to cultivate some new defensive ideology, as the original ethnocentric justification was reduced to fiction, as Canaanites became assimilated to the "chosen people," or find new sources of slaves to replace those assimilated and emancipated. Both of these circumstances were present in the Hebrew kingdoms of the first millennium B.C. and in the later Jewish communities of the ancient and medieval Near East.

As to the first of these circumstances, the Hebrew legitimists, if they were to defend the line of social stratification defining their influence and privileges, had to cultivate some distance-creating idea that denied the legitimacy of the social ties that inevitably arose between bond and free. In the ancient and medieval Near East the necessary distance was created by the Hamitic myth, which always made possible a shift in the interpretation of the lowly status of the Canaanites from grounds of ethnic distance to grounds of birth. As Canaanites increasingly adopted Hebrew religion and customs and as their degradation became more difficult to explain by the inferiority of their culture, it could still be explained by Noah's curse on Canaan—that is, by the inferiority of their birth. Differences in ancestry, however, are not always as obvious to the eye and ear as those of language and custom. People making castelike distinctions, therefore, often appeal to a justifying myth. American scholars have sometimes thought that there is no way as effective in marking a status frontier as a cluster of ideas associated with skin color. Yet a myth also serves fairly well: two thousand years after the conquest of Canaan, the Jewish philosopher Maimonides, in compiling a slave code, was still observing a distinction between "Canaanite" and "Hebrew" slaves.¹⁵

Undoubtedly, the general acceptance of this Curse of Noah slowed down the process of assimilation and emancipation. Nevertheless, even as early as the beginning of the Christian era, there were no longer enough bona fide Sons of Ham available to meet the demand for slaves. Since it proved to be easier to stretch the definition of "Canaanite" than it was to find new sources of slaves

¹⁵ Maimonides, *Mishnah Torah*, Book Twelve: Acquisitions, Treatise V: Laws Concerning Slaves (Isaac Klein, trans., *The Code of Maimonides*, Yale Judaica Series, ed. Julian Obermann, 20 vols. [New Haven, 1951], 5: 245–81).

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locally, the custom arose of importing "Canaanites" into the ancient land of Canaan. During the first Christian centuries most "Canaanites" were in fact either Syrians or *Kushim* from black Africa.¹⁶ In the early Middle Ages the concept was stretched even further. In the ninth and tenth centuries, for example, as a result of intertribal wars in the area of modern Yugoslavia, thousands of captives from that region flooded the medieval slave markets. Some of these were sold as "Canaanites" in the Jewish communities of the Near East.¹⁷ The subtlety of the theologian and the craft of the lawyer could transform blue-eyed Slavs and black-skinned *Kushim* alike into fellow descendants of Canaan, both laboring under the same devastating curse, the validity of which had grown no weaker over the centuries than had the demand for slaves.

The "sons of Ham" had thus begun their journey that would lead them along the tedious and twisted paths of Jewish, Muslim, and Christian theology and jurisprudence and would bring them one day to the shores of Guinea and beyond. But, before considering the historical pressures that darkened their faces and changed the way their hair grew, the problem of the justification of slavery in Europe needs to be examined. After all, slavery played a less prominent part in the life of the early Jews than it did in the Greco-Roman lands to the west.

MORE WIDELY ACCEPTED than the Hamitic myth during antiquity was a "sambo" perception of slaves. Roman, Greek, and Hebrew masters all referred to the fully adult men and women they owned by the linguistical equivalents of "boy" and "girl." European slaveholders, moreover, would have found little to disagree with in the views of ancient Talmudists, who believed that slaves had a predilection for lying, laziness, theft, drunkenness, and lewdness.¹⁸ Furthermore, the Europeans, like the peoples of the Near East, engaged in the stereotyping of slaves. But beyond the "sambo" perception, which all shared, the stereotypes that emerged in the two regions were different. In Europe for a time, Scythians were regarded as a slavish people. Then Thrace became an important source of supply for the slave trade and Thracian characteristics, such as red hair, came to be regarded as slave characteristics. An actor on the Roman stage wore a red wig as a signal to the audience that he was playing the role of a slave. The Thra-

¹⁶ J. Klausner, "The Economy of Judea in the Period of the Second Temple," in Michael Avi-yonah *et al.*, eds., *The Herodian Period*, vol. 7 of *The World History of the Jewish People*, ed. Cecil Roth (New Brunswick, N.J., 1975), 194; and Baron, *Social and Religious History of the Jews*, 2: 259-60.

¹⁷ Cecil Roth, "Economic Life and Population Movements," in Cecil Roth, ed., *The Dark Ages: The Jews in Christian Europe, 711-1096*, vol. 11 of *World History of the Jewish People*, 27-28; Ivan Hrbek, "Die Sklaven im Dienste der Fatimiden," *Archiv Orientali*, 21 (1953): 549-50; and Johannes Hoffmann, "Die östliche Adriaküste als Hauptnachschubbasis für den venezianischen Sklavenhandel bis zum Ausgang des elften Jahrhunderts," *Vierteljahresschrift für Sozial- und Wirtschaftsgeschichte*, 55 (1968): 172, 180.

¹⁸ Baron, *Social and Religious History of the Jews*, 1: 269-70; and Henri Wallon, *Histoire de l'esclavage dans l'antiquité*, 2 (Paris, 1847): 8. For the same tradition among the Arabs, see Adam Mez, *The Renaissance of Islam*, trans. S. H. Bakhsh (Patna, India, 1967), 162. "Sambo," the Hausa word for "second son," in American usage has come to designate a stereotype of the American black, who has been depicted as "docile but irresponsible, loyal but lazy, humble but chronically given to lying and stealing; his behavior was full of infantile silliness and his talk inflated with childish exaggeration"; Stanley M. Elkins, *Slavery: A Problem in American Institutional Life* (3d ed., Chicago, 1976), 82. This work is a much-disputed interpretation of the "sambo" stereotype.

cian name, "Rufus," came to be regarded as a typical slave name regardless of the slave's nationality.¹⁹

In response to patterns of political disorder or military conquest, however, the principal source of European slaves kept shifting, and early ethnic stereotypes lost all credibility. Various Western European nationalities at times became important sources of slaves, although not for a long enough period for any of them to acquire a slavish reputation. Gaul, for example, was an important source immediately after the Roman conquest in the first century B.C. And the political and ethnic fragmentation of Britain, following the collapse of Roman power, briefly created a surplus of slaves for export in that country.²⁰ In the later Middle Ages, as stable governments emerged in the West, the source of European slaves became confined increasingly to the lands around the Black Sea, areas that continued to feed the Mediterranean slave trade from remote antiquity down to the beginning of modern times. Because every major ethnic stereotype of European slaves has been drawn from this region, a glance at its history may shed some light on how certain peoples acquire a slavish reputation.

Since prehistoric times the rich black soil country north and west of the Black Sea has attracted settlers. But this territory has been the scene of a repetitive disaster. Periodically, nomadic invaders from Central Asia have overrun the western steppes, annihilating the inhabitants or selling them into slavery. Ironically, those escaping the invaders and taking refuge in the Caucasus Mountains to the east of the Black Sea helped create still another source for the slave trade. The remnants of many peoples have intruded into the territory of earlier refugees where they competed for the resources of a region in which much land is non-productive or marginal. Modern Caucasia is one of the most ethnically complex regions of the world. More than fifty languages are spoken in an area slightly larger than California.²¹ In ancient times the ethnic fragmentation may have been even greater. Strabo reported that seventy languages were used in a trading town located near modern Sukhumi, and Pliny the Elder says the Romans in the same area used one hundred and thirty interpreters "for the purpose of transacting business."²² From antiquity until modern times the poverty and the ethnic conflict of the region delivered a continuing stream of Caucasians to the

¹⁹ M. L. Gordon, "The Nationality of Slaves under the Early Roman Empire," in Finley, *Slavery in Classical Antiquity*, 174.

²⁰ But a measure of reunification took place within a hundred years, although some young Englishmen were still being shipped out of Bristol as late as the eleventh century. See Marc Bloch, "Comment et pourquoi finit l'esclavage antique," in Finley, *Slavery in Classical Antiquity*, 224-26; and Charles Verlinden, *L'esclavage dans l'Europe médiévale*, vol. 1: *Péninsule Ibérique-France* (Brugge, 1955), vol. 2: *Italie, colonies italiennes du Levant, Levant latin, Empire byzantin* (Ghent, 1977), 1: 701.

²¹ Moses I. Finley, "The Black Sea and the Danubian Regions and the Slave Trade in Antiquity," *Klio: Beiträge zur alten Geschichte*, 40 (1962): 51-55; *Great Soviet Encyclopedia*, translation of the 1973 edition, s.v. [Caucasus] "Historical Survey" by S. A. Zaleskii; Bernard Geiger et al., *Peoples and Languages of the Caucasus: A Synopsis* (The Hague, 1959); and William H. McNeill, *Europe's Steppe Frontier, 1500-1800* (Chicago, 1964).

²² Strabo, *Geography*, 11: 2, 16; and Pliny, *Natural History*, 6: 5. Some relationship appears to exist between this ethnic diversity and the political fragmentation that has characterized Caucasia and other slave-exporting regions. During the eleventh century, for example, in the one-hundred-and-ninety-mile stretch between Lake Van and the Black Sea coast, there are reported to have been some dozen rival "kingdoms." W. E. D. Allen, *A History of the Georgian People from the Beginning down to the Russian Conquest in the Nineteenth Century* (London, 1932), 85.

slave traders. During the later Middle Ages certain Caucasian peoples such as the Circassians, the Abkhaz, and the Mingrelians were scarcely known outside of their native region except as slaves. Ibn Botlan, an eleventh-century Arabic-speaking Christian physician of Baghdad, described another Caucasian people in distinctly slavish terms:

The Armenian is the worst of the white, as the negro is of the black. . . . Chastity is unknown and theft is rampant among them. But they know not avarice. Coarse is their nature and coarse is their speech. Let an Armenian slave be an hour without work and he will get into mischief. He only works under the threat of the cane or the stress of fear. When you find him lazy—it is simply because he delights in laziness and not because he does not feel equal to work. You must then take to the cane, chastise him and make him do what you want.²³

Long debased by powerlessness, poverty, and slavery, Caucasian peoples have also been burdened with a "sambo"-like reputation that persisted a long time indeed. Even in the late nineteenth century an influential encyclopedia could write of a group of nationalities who lived in what is now the Soviet Republic of Georgia that "the one trait of these generally very poor populations is their care-free attitude. Living from day to day, these peoples think only of adorning themselves in brightly colored clothes that bring out their physical good looks. Singing and dancing are their favorite pleasures. Despite their mediocre intelligence these Caucasians know how to capture the sympathy of foreigners by their grand hospitality and gay friendly manner."²⁴

During the late Middle Ages, however, the most prominent stereotypes of European slaves were not derived from the steady trickle of humanity flowing from Caucasian lands but from the larger disasters and more numerous peoples to the north and west of the Caucasians. In the late fourteenth century, for example, when Tamerlane's armies overran the Golden Horde, the slave markets of Italy were flooded with Tartars—a situation that created for a time a Tartar stereotype for slaves in that country.²⁵ The Slavic peoples of the area suffered a similar fate. Indeed, far more important than the brief Tartar stereotype was the Slavic one, which predominated during the latter half of the Middle Ages in many countries. The Slavs inhabited, in addition to the western steppes, other areas that made them vulnerable. Many lived on the Balkan peninsula, which, like Caucasia, was an ethnically and politically fragmented mountain refuge for defeated peoples of the steppes. For centuries Balkan Slavs were victims of the Venetian slave traders. Also victims were the Central European Slavs, who lived in the path of the German medieval eastward expansion, the *Drang nach Osten*. In

²³ Ibn Botlan, "Introduction to the Art of Making Good Purchases of Slaves," as quoted in Mez, *The Renaissance of Islam*, 162. In certain contexts, especially when comparing themselves to more northerly peoples, Arabs of this period thought of themselves as "black"; see Karl Vollers, "Über Rassenfarben in der arabischen Literatur," in G. Pittrè et al., eds., *Centenario della nascita di Michele Amari* (Palermo, 1910), 87; and Wolf Dietrich Fischer, *Farb- und Formbezeichnungen in der Sprache der altarabischen Dichtung* (Wiesbaden, 1965), 338.

²⁴ *La Grande Encyclopédie* [1885–92 ed.], s.v. "Caucase," by P. Lemosof.

²⁵ Domenico Gioffrè, *Il mercato degli schiavi a Genova nel secolo XV* (Genoa, 1971), 13–14; and *Great Soviet Encyclopedia*, s.v. "Golden Horde," by V. I. Buganov.

virtually every language in Western Europe, the word for slave derives from this politically fragmented and perpetually vulnerable people.²⁶

WHILE SHIFTING ETHNIC STEREOTYPES FOR SLAVES emerged in ancient and medieval Europe, a relatively stable, *racial* stereotype for slaves emerged at the same time in the Near East. Just as most European slaves, and all of the major stereotypes of them, were drawn from the perpetually vulnerable peoples around the Black Sea, so the peoples of black Africa played a corresponding role in the slave trade of the Near East. As Near Eastern slaves more and more came from below the Sahara, the people of the Near East increasingly equated "slaves" with "Negroes." Crucial though this development is for understanding the evolution of the Hamitic myth and the origins of modern racial prejudice in general, the various stages of the process cannot be dated with absolute certainty. Its broad outlines, however, are known. The New Testament contains no hint of any identification of "slave" with "Negro." To be sure, these books from the later first and early second centuries are in Hellenistic Greek and leave the imprint of Hellenism on certain traditions of the Semitic-speaking peoples. In the Babylonian Talmud and certain Jewish midrashim there is a suggestion, albeit tenuous, of such identification. These works, which scholars have ascribed to the fourth through the sixth centuries A.D. (or C.E.), seem to betray little European influence. The equation of "slave" with "Negro" is missing, however, from the Quran (Koran), a highly indigenous Arabian work of the early seventh century. Only from the eighth century on, in several types of Muslim literature, does the fusion of the notions of "black" and "debasement" appear unmistakably and continually.

The differences between European and Near Eastern stereotypes for slaves resulted from the differing nature of the slave trade in the two regions. In Europe, until very late in the Middle Ages, not only the African slave trade but also all direct trade with sub-Saharan Africa was of minor importance. The world's greatest desert posed a formidable barrier between the two regions. Not until the second half of the fifteenth century was the Sahara bypassed on the west and relatively easy maritime contact established between Europe and black Africa. But some three thousand years earlier the Sahara was bypassed on the east. Even as early as the Middle Kingdom in the second millennium B.C., the Egyptians constructed a Nile-Red Sea canal and established Red Sea ports serving

²⁶ Meyer, *Die Sklaverei im Altertum*, 11; Hrbek, "Sklaven im Dienste der Fatimiden," 547, 552; Hoffmann, "Die östliche Adriaküste als Hauptnachschubbasis für den venezianischen Sklavenhandel," 170, 172, 179, *passim*; and Antonio Teja, "Aspetti della vita economica di Zara dal 1289 al 1409," pt. 2: "La schiavitù domestica ed il traffico degli schiavi," *La Rivista Dalmatica*, 22 (1941): 29-30. For the derivation of the word slave in various languages, see Verlinden, *L'Esclavage dans l'Europe médiévale*, 2: 999-1010. Another study yields still further evidence for the slavish reputation of medieval Slavs. The Arabic term for "eunuch" is derived from the ethnic root *Sakhab* (Slav), while the Catalanian term *esclavó* (castrated goat) and the Spanish *eslabón* (link of chain) are likewise taken from the name of East European captives who were sometimes castrated or chained. See Henry Kahane and Renée Kahane, "Notes on the Linguistic History of Sclavus," in *Studi in onore di Ettore Lo Gatto e Giovanni Maurer* (Florence, 1962), 346, 350-51, 357.

their trade with black Africa. Early in the first millennium B.C., the first Hebrew kingdom also had such a port (1 Kings 9:26–28). Furthermore, the Nile valley, fertile and densely settled, led like a road from the Mediterranean to black Africa.²⁷

From remote antiquity the Near East imported slaves from black Africa. Egyptian paintings depict Negro captives. For a very long time this trade was relatively small though growing. Probably as late as the seventh century A.D. more local natives than blacks were sold as slaves in the Near East. Before the establishment of Islam, this ancient battleground of nations, itself, supplied considerable numbers of captives to the slave trade. It may have been as late as A.D. 1000 before the institution of slavery in the Near East and Muslim lands became predominantly Negro slavery.²⁸ But long before a majority of slaves were black, perhaps several centuries before, enough Africans were sold in the Near East to form the basis for a racial stereotype for the region comparable to the Thracian or the later Slavic stereotypes farther west. While still a minority of the servile population, blacks could be recognized as members of the lowest status group, foreign slaves, those sold beyond reach of kin and hope of ransom. This bottom stratum of the society, which included only a small minority of the white population, accounted for the overwhelming majority of the blacks. It was they who became the instantly visible “Canaanites,” the archetypal “sons of Ham.”²⁹

More than ten centuries separate the appearance of the story of Ham in the book of Genesis from the elaboration and explanation of the tale that occurs in rabbinic literature. During these centuries the face of servitude had darkened in the Near East. In retelling the story, one rabbi correspondingly darkened the face of Ham and made the curse of Noah read, “Your seed will be ugly and dark-skinned.” If this writer magnified the curse, another likewise increased the offense for which it was a punishment: “Ham and the dog copulated in the Ark, therefore Ham came forth black-skinned, while the dog publicly exposes its copulation.”³⁰ Still another commentator further vilified the delinquent ancestor of slaves by suggesting that Ham had not only looked upon his father’s nakedness

²⁷ José Antonio Saco, *Historia de la esclavitud desde los tiempos mas remotos hasta nuestros dias*, 1 (2d ed., Havana, 1936): 23–25; and Levi Herzfeld, *Handelsgeschichte der Juden des Altertums* (Braunschweig, 1879), 128, 221–22, 233. For a discussion of caravan trade and the Sahara as a barrier, see E. W. Bovill, *The Golden Trade of the Moors* (New York, 1958). For the navigational problems that delayed the development of a sea route around the western Sahara, see Raymond Mauny, *Les navigations médiévales sur les côtes sahariennes antérieures à la découverte portugaise* (Lisbon, 1960). For evidence of a black slave trade in Pharaonic times, see Wallon, *Histoire de l’esclavage dans l’antiquité*, 1: 27; for the trade in the first century A.D., Raymond Mauny, trans., *Péripie de la mer Erythrée* (Paris, 1968), 25–26; and, for the early Islamic period, Y. F. Husan, *The Arabs and the Sudan from the Seventh to the Early Sixteenth Century* (Edinburgh, 1967), 42–50.

²⁸ That slaves were predominantly black in Egypt during the period ca. 950–1250 is evident from the Geniza documents. See S. D. Goiten, *A Mediterranean Society*, vol. 1: *Economic Foundations* (Berkeley and Los Angeles, 1967), 130–47. Also see Mez, *The Renaissance of Islam*, 157–58; and Gernot Rotter, “Die Stellung des Negers in der islamisch-arabischen Gesellschaft bis XVI Jahrhundert” (Ph.D. dissertation, Rheinische Friedrich-Wilhelms-Universität, Bonn, 1967), 52. Rotter’s fine doctoral dissertation has been my chief authority on Muslim race relations and racial attitudes.

²⁹ Bernard Lewis, *Race and Color in Islam* (New York, 1971), 27–28.

³⁰ *Bereshith Rabbah*, 36. 7 (H. Freedman and Maurice Simon, trans. and eds. *Midrash Rabbah*, 10 vols. [London, 1959], 1: 293). Also see *b. Sanhedrin*, 108b (Epstein, *Babylonian Talmud*, 20: 745).

but had castrated him as well.³¹ Clearly, a person who believed these words and accepted the biblical tradition of hereditary sin had to conclude that the treatment slaves were getting was about what they deserved. And, indeed, such conclusions, along with fragments of the rabbinic commentary, have been echoed down over the centuries, from Babylon to New Orleans, often by men who have been generally innocent of any other bits of Hebrew learning.

The racial stereotyping of slaves by Jews does not appear to have been elaborated beyond the rabbinic expressions of the period from the fourth to the sixth century. During the Middle Ages the Jews became to a larger extent a European as well as a Near Eastern people, and they came to share the stereotypes of both regions. Blacks continued to be "Canaanites"; but at the same time, by some logic best understood by theologians and lawyers, the Slavonic language, ancestor of the modern Slavic languages, became the "language of Canaan."³² After the sixth century, it was those less Europeanized sons of Shem, the Arabs, who further developed the tradition of Ham.

SIXTH-CENTURY, PRE-ISLAMIC ARABS may have already begun to equate blacks with slavery and debasement, although the evidence is questionable.³³ Such an equation is not to be found in the Quran or in the early Islamic movement of the next century. All races were welcomed into the faith, and the Quran lashes out against pride in family lineage, adding that the "most honoured . . . in the sight of God is he who is the most righteous." And the Quran views the linguistic and racial differences of man without any Arab ethnocentrism but sees them as a manifestation of a divine miracle.³⁴ Furthermore, in Muslim society, as in Jewish society, slavery never became the predominant labor system. The Quran possibly reaffirms the Semitic tradition concerning the descendants of Ham. It is free, however, of the color prejudice that seems to have influenced slightly earlier rabbinic versions of the tradition; and there is no condemnation to slavery. Rather than a curse, it contains a vaguely directed threat coming not from Noah but from God himself: when the waters of the Great Flood had subsided, God commanded Noah to "come down from the Ark with peace from us, and blessings on thee and on *some* of the peoples who will spring from those with thee: but there will be other peoples to whom we shall grant their pleasures for a

³¹ *Wayyikra Rabbah*, 17.5 (Freedman and Simon, *Midrash Rabbah*, 4: 219).

³² A twelfth-century Jewish scholar, arriving in Prague from Germany, wrote, "Here begins Slavonia, called by the Jews who inhabit it *Kh'na'an*, because the inhabitants sell their children to all nations, which is also applicable to Russia"; *The Itinerary of Rabbi Benjamin of Tudela*, trans. A. Asher (New York, n.d.), 164. For Slavonic as the "language of Canaan," see Jacob Winter, *Die Stellung der Sklaven bei den Juden in rechtlicher und gesellschaftlicher Beziehung* (Halle, 1886), 5-6, n. 5.

³³ One Islamic scholar has given some weight to this evidence; see Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 25, 75-78. Another has held that antiblack statements attributed to sixth-century poets "almost certainly belong to later periods and reflect later problems . . ."; see Lewis, *Race and Color in Islam*, 9.

³⁴ Quran, 49: 13, 30: 22 (Abdullah Yusuf Ali trans. [Mecca, 1956]). But, since Arabic is the sacred language of Islam, an element of ethnocentrism was inescapable.

time, but in the end will a grievous penalty reach them from us."³⁵ In years to come Muslim slaveholders would make considerable use of the Hamitic myth, but the Quranic version would not prove to be their most effective weapon.

In view of the broad-minded ethnic and social attitudes of the Prophet as well as the noblest of his followers, it is ironic that the lands of Islam became the cradle of modern racial stratification and of many of the ideas that are still used to justify special privileges defined by skin color and other racial characteristics. Muslims aspired to a universal brotherhood of believers. But prominent among their actual achievements was the forging of new links between blackness and debasement. It was under the Muslims that slavery became largely a racial institution.

An important reason why Muslim slavery became Negro slavery was because the rise of Islam eliminated from the Mediterranean slave trade an important source of light-skinned slaves. Previously, the military and political disorder of the Near East had filled the slave markets with captives; as long as these conditions persisted, most slaves remained racially indistinguishable from their masters.³⁶ But the Muslim ideal of brotherhood united the Bedouins, redirected their fierce fighting qualities outward, and assimilated their energies into a dramatic campaign of conquest. When the frontiers of Islam became comparatively stabilized, one hundred years after the death of the Prophet, the movement had established Muslim hegemony over a vast area extending from the Iberian peninsula to the borders of China. The political reality of unity found an idealized expression in the Sunna, or Islamic law, which held that no freeborn Muslim could be sold into slavery. Nor could a "dhimmi"—that is, a person who remained a Jew or a Christian but who lived under the protection of a Muslim government—be legally enslaved.³⁷ If the highly assimilationist Muslim system of slavery was to endure, new sources of captives had to be found outside of the *Dar al-Islam*, the countries living under the peace of Islam.

Lawful captives had to be taken either north of the Mediterranean or south of the Sahara; and, indeed, until well into the modern period, Muslims acquired slaves from each of these regions, though in highly unequal numbers. During the later Middle Ages a number of European states developed, with sophisticated military organizations that could answer the challenge of Islam blow for blow. European rulers, furthermore, could often recover their captured subjects by prisoner exchange or ransom.³⁸ But, basically, few European slaves were available for purchase during the later Middle Ages because of the more orderly political conditions in France, England, the German Empire, and other countries. As these states ceased producing a surplus of slaves for export, the supply

³⁵ Quran, 11: 48; italics added. But, since slaves did not have "their pleasures for a time," the threat may not be directed against the sons of Ham, as is sometimes supposed.

³⁶ Verlinden, *L'esclavage dans l'Europe médiévale*, 1: 228.

³⁷ Laura Veccia Vaglieri, "The Patriarchal and Umayyad Caliphates," in P. M. Holt *et al.*, eds., *The Cambridge History of Islam*, 1: 57-103; and *Encyclopaedia of Islam*, new ed., s.v. "Dhimma," by Claude Cahen.

³⁸ Verlinden, *L'esclavage dans l'Europe médiévale*, 1: 541-43; and Iris Origo, "The Domestic Enemy: The Eastern Slaves in Tuscany in the Fourteenth and Fifteenth Centuries," *Speculum*, 30 (1955): 328. Also see Hans Müller, "Sklaven," in B. Spuler, ed., *Handbuch der Orientalistik*, pt. 1: *Der Nahe und der Mittlere Osten*, 6, sec. 6 (Leiden, 1977): 68-72.

from Europe was reduced to a minimum, and those came mainly from the Slavic lands.³⁹ Both in Europe and in black Africa the slave trade was governed by similar considerations. But, after about the eleventh century, political conditions south of the Sahara were more favorable to slave traders. To be sure, there were strong states in black Africa, such as those of the western Sudan, that protected their inhabitants from slave raids; but most black Africans lived in ethnically fragmented, often mutually antagonistic, societies that could offer little resistance to raids from the Sudanese or other Muslim states. It is no accident, therefore, that in late medieval Arabic poetry and popular literature *Banu Ham*, the "sons of Ham," is a synonym for *sudan*, the Negroes.⁴⁰

Not only did slavery become largely a racial institution in a broad belt of countries extending from Andalusia to the Indian Ocean, but a related development also took place in these countries that forged even tighter links between blackness and debasement, links that have endured into the modern era. As early as the ninth century, racial stratification began to appear in *both* the servile and the free populations.

A line of racial stratification dividing the servile population can be seen in the evolution of two Arabic words. *'Abd* (plural *'abid*), the classical term for slave, continued to be used in its original sense in legal and other formal literature. But in popular literature the term was no longer applied to a white, regardless of legal status. A European slave was now most often referred to as a *mamluk* (owned). *Mamluks* were rarer—and, hence, more expensive—than the *'abid*.⁴¹ European slaves, furthermore, had special uses that also enhanced their value—in prisoner exchanges, for example, to recover family members captured by Christians in the religious wars or to obtain substantial ransoms from wealthy families for Christian knights. Even those of a lower class origin were a potential source of ransom; for Europe, unlike black Africa, had well-organized institutions, such as *El Orden de la Merced*, that specialized in raising money to ransom Christian captives.⁴² To be a white slave was to be a valuable slave, and in the history of servitude relative value, more than any other factor, influenced the treatment that an individual could expect.

³⁹ Mez, *The Renaissance of Islam*, 158. The Mediterranean slave trade was actually more complex than the above description suggests. Some slaves came from more distant lands such as Central Asia and even India. Also the Ottoman Turks in the fifteenth and sixteenth centuries brought about a vigorous revival of white slavery. But until 1453 the Christians controlled the Straits and, hence, access to the Black Sea source of slaves. See Albert Howe Lybyer, *The Government of the Ottoman Empire in the Time of Suleiman the Magnificent* (Cambridge, Mass., 1913), 45–61.

⁴⁰ Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 22, 141. *Sudan* as a geographical term is derived from *Bilad al-Sudan*, "the land of the blacks." For the relationship between state power and the slave trade in this region, see Marian Malowist, "Le Commerce d'or et d'esclaves au Soudan occidental," *Africana Bulletin*, 4 (1966): 61, *passim*.

⁴¹ *Encyclopaedia of Islam*, new ed., s.v. "'Abd" by R. Brunschvig; and Eliyahu Ashtor, *Histoire des prix et des salaires dans l'orient médiéval* (Paris, 1969), 58–59, 361–63, 437–38. In Baghdad in the ninth and tenth centuries a "good-looking, but untrained white slave-girl fetched 1,000 dinars or more," while in Oman, near the mouth of the Persian Gulf, blacks were selling for the equivalent of 25–30 dinars; Mez, *The Renaissance of Islam*, 157–58. For the racial connotation that the word *'abd* assumed, see Leon Carl Brown, "Color in North Africa," in John Hope Franklin, ed., *Color and Race* (Boston, 1968), 193.

⁴² Other such institutions included the Order of the Trinitaries, Santa Maria della Mercede, and Riscatto Schiavi. See Verlinden, *L'esclavage dans l'Europe médiévale*, 1: 541–43; Origo, "Domestic Enemy," 328, 330; and Gioffrè, *Mercato degli schiavi a Genova*, 51–52 n. 115.

The favored treatment of *mamluks* is especially important because of the direction that the institution of slavery took in Islamic countries beginning roughly in the eighth century. The legitimist impulse all but collapsed.⁴³ Muslim patriarchs were, therefore, inhibited by few legal restrictions or social taboos in their paternalistic management of slaves, a development that resulted in hundreds of thousands of people, made captives by the wars of conquest, passing completely through the institution in about three generations to become fully integrated into Islamic society. Thus, Muslim slavery had few parallels in the degree of its paternalism. The "scandalous paradox" was everywhere in evidence as sons of slave mothers became sultans who governed through slave administrators, whose courts were protected by elite guards, entertained by highly trained musicians and dancers, all of whom were slaves. Though their servile status was more than a legal fiction, these slave elites sometimes exercised considerable power and enjoyed a splendid life style.⁴⁴ Under the Fatimid caliphate of the tenth and eleventh centuries, for example, *mamluks* of mostly Turkic or Slavic origin administered Egypt and other countries of the Near East and North Africa.

Black slaves, or *'abid*, however, were less expensive and consequently more expendable. Tens of thousands of black Africans, for example, labored on land reclamation projects in Iraq. Blacks were also used in the copper and salt mines of the Sahara. Wherever the work was demanding and the conditions harsh, black slaves were likely to be found.⁴⁵ Furthermore, when slaves were emancipated, the old line of racial stratification that divided the servile population into *mamluks* and *'abid* continued to affect their status as freedmen. Because of the almost unparalleled paternalism of Muslim slavery, patriarchs showed a marked tendency to assimilate fresh captives to Islamic culture, convert them, and later emancipate them.⁴⁶ Although creating a demand for freshly captured pagans, this process also produced a continuous growth of the freedman population. But

⁴³ Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 76-77, 131-35. Nevertheless, the legitimist tendency did not disappear, especially in the area of law. Muslim law, and, indeed, the legal systems of all slave-holding societies, have functioned conservatively—that is, toward maintaining as much of the original servile character of the society as possible. Thus, despite the towering legal authority of Quran 24: 30 ("And to those of your slaves who desire a deed of manumission, execute it for them, if ye know good in them, and give them a portion of the wealth of God which He hath given you"), the courts responded more sympathetically to the self-interests of heirs who were in their prime than they did to the piety of patriarchs who were aged or deceased. Later Muslim courts ruled that an owner could not by will alienate more than one-third of his estate. Mez, *The Renaissance of Islam*, 169.

⁴⁴ For a study of highly trained servile entertainers, see Albert Wesselski, "Die gelehrten Sklavinnen des Islams und ihr byzantinischen Vorbilder," *Archiv Orientalni*, 9 (1937): 353-78. For revolts of free subjects against servile bureaucrats, see A. N. Poliak, "Les révoltes populaires en Egypte à l'époque des Mamelouks et leurs causes économiques," *Revue des Études Islamiques*, 8 (1934): 251-73. For examples of the conspicuously privileged slave in other cultural settings, see Wallon, *Histoire de l'esclavage dans l'antiquité*, 1: 189; and Mendelsohn, *Slavery in the Ancient Near East*, 71. In slave and former slave societies with strong legitimist traditions, however, a much less dramatic advancement of a low-status person is required to create the scandalous paradox. To produce such a reaction in the United States, for example, it has often been sufficient for a Negro to appear driving a new Cadillac.

⁴⁵ For the use of black slaves in Saharan salt and copper mines, see Ibn Battuta, *Voyages d'Ibn Battuta*, trans. and ed. Defremery C. Sanguinetti, 4 (Paris, 1879): 377, 440; and, in gold mines, see Hasan, *Arabs and the Sudan*, 50-63. For a revolt involving thousands of black slaves used in land reclamation projects in southern Iraq, see Alexandre Popovic, *La Révolte des esclaves en Iraq au III^e/IX^e siècles* (Paris, 1976).

⁴⁶ Samuel Sheridan Haas, "The Contributions of Slaves to and Their Influence upon the Culture of Early Islam" (Ph.D. dissertation, Princeton University, 1942), 10-11, 39-52, *passim*; and Michael Brett, ed., *Northern Africa: Islam and Modernization* (London, 1973), 2-3.

blacks fared more poorly than whites as freedmen just as they had as slaves. Even the term *'abīd* still clung to them in freedom, linking them to the lowest stratum of the servile population. Behind this linguistic tie lay the reality that they had moved from the lowest stratum of the servile population to a corresponding position in free society. And *'abīd* no longer denoted their legal status; instead it identified their race. By the ninth century, therefore, the process of enslaving, assimilating, converting, and freeing Negroes had, in a series of Muslim cities from Andalusia to Persia, created a class of blacks who, though legally free, still worked as butchers, bath attendants, and the like, who still toiled in lowly occupations similar to those they had pursued as slaves.⁴⁷ Emancipation did not dramatically change what one saw blacks doing; in popular usage they were still *'abīd*, "slaves."

Despite the general polarization of Muslim society into low-status blacks and high-status whites, no clearly defined color bar emerged. Muslim countries have, therefore, produced some notable examples of the overlapping of racial status groups. Tenth-century Egypt, for instance, had a *de facto* black ruler. In the seventeenth and eighteenth centuries, an elite guard of blacks held the balance of power in Morocco; two centuries later it was ruled by Sultan Mulay Hasan, whose mother was black.⁴⁸ One of the most spectacular examples of black upward mobility is that of Kızlar Agası Beshir (1650-1746), an Abyssinian eunuch purchased for thirty piasters, the price of a first-rate donkey, but who prospered unbelievably during his long tenure as Ottoman secretary of the treasury. By the time of his death, he had amassed a vast fortune and had founded the Mosque of Aga in Istanbul as well as a number of schools and a public library.⁴⁹

Muslim racial attitudes reflect the ambivalence of the system of color stratification in Islamic society, with its inconsistency, at times its seeming lack of color prejudice. Significantly in this respect, a body of Muslim literature emerged that treated blacks sympathetically or defended them against their detractors.⁵⁰ Nevertheless, Muslims lived in a racially stratified society. If they, unlike people in later English-speaking stratified societies, had no clearly defined color bar, if they were correspondingly less disturbed by the occasional appearance of a high-status black, such violations of the prevailing pigmentocracy, or light-skinned dominance, did not happen often enough to discredit the assumptions most people made about skin color and status: light meant superiority, dark meant debasement.⁵¹

Muslim attitudes toward blacks were mixed, but amid their ambivalence one

⁴⁷ Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 114.

⁴⁸ Brown, "Color in North Africa," 191-92.

⁴⁹ Joseph Freiherr von Hammer-Purgstall, *Geschichte des osmanischen Reiches*, 10 vols. (Pest, Austria-Hungary, 1827-35), 8: 70-71.

⁵⁰ For a review of this literature, see Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 10-20. Unfortunately, most of these writers do not defend blacks in general but limit their defense to certain nationalities, especially Abyssinians.

⁵¹ Levy, *Social Structure of Islam*, 61-64; and Gustave E. von Grünebaum, *Medieval Islam: A Study in Cultural Orientation* (2d ed., Chicago, 1954), 190, 109-10. For the development of the idea of "pigmentocracy" as a theory of Latin American race relations, see Alejandro Lipschütz, *El Indioamericanismo y el problema racial en las Américas* (Santiago, 1944).

can detect here and there most of those notions making up that cluster of ideas we recognize as modern Western racial prejudice. As Negroes came to occupy the bottom strata of both free and servile society and as the term *'abid* came in popular usage to identify a race rather than a legal class, Muslims came to attach to blacks those ideas that Old World peoples had traditionally attached to slaves regardless of their origin. Negroes were thus stereotyped as lazy, lecherous, and prone to lie and steal. And, when humans are treated as domesticated animals, they are sometimes regarded as animallike. Thus, just as the ancients occasionally denied the humanity of slaves, so could Ibn Khaldun, a historian of western Islam, write that "the only people who accept slavery are the Negroes, owing to their low degree of humanity and their proximity to the animal stage."⁵² But not very intelligent animals, a Persian writer thought: "Many have seen that the ape is more capable of being trained than the Negro, and more intelligent." A thirteenth-century Moroccan asserted that the blacks had another quality that seemed to make them especially suited to the debased status that his society generally accorded them. They were "the most stinking of mankind in the armpits and sweat."⁵³ And, according to a popular work on slave buying, blacks were "fickle and careless. Dancing and beating time are engrained in their nature. They say: were the negro to fall from heaven to the earth he would beat time falling."⁵⁴ Thus the most important ideas justifying white dominance had been current in the racially stratified lands of the Mediterranean for several centuries before the northwest Europeans bought their first Negro slaves.

Though Muslims held many disconnected ideas and value judgments that lent stability to a previously evolved pattern of pigmentocracy, the closest they had to a theory of race relations remained the ancient Hamitic myth. During the first century of Islam, the "sons of Ham" had begun their migration to *Bilad al-Sudan*, the land of the blacks; but within their ranks were still a number of light-skinned peoples. Thus, the myth justified slavery more than white superiority, as it had done since early in the first millenium B.C.

The evolution of this tradition is reflected in the genealogies that often follow an account of the curse of Noah. These genealogies purport to show how various nationalities descended from the single family that survived the Flood. They therefore reveal beliefs about which nations have been condemned to perpetual slavery.⁵⁵ At the time of the Muslim conquest of North Africa, most genealogies

⁵² Meyer, *Die Sklaverei im Altertum*, 47-48; and Ibn Khaldun, *An Arab Philosophy of History: Selections from the Prolegomena of Ibn Khaldun of Tunis (1332-1406)*, trans. Charles Issawi (London, 1955), 98.

⁵³ Nasir al-Dīn Tūsī, *Tasawwūrāt* (Leiden, 1950), as quoted in A. J. Arberry, *Classical Persian Literature* (London, 1958), 255; and as-Suhaylī, "Er-Roud el-Unif [sic]," as quoted in Edward William Lane, *An Arabic-English Lexicon*, 2 (London, 1872): 1462, s.v. "Sudan."

⁵⁴ Ibn Botlan, "Introduction to the Art of Making Good Purchases of Slaves," as quoted in Mez, *The Renaissance of Islam*, 161.

⁵⁵ Gerhard von Rad has warned against reading traditional Hebrew genealogies literally as expressions of actual kinship between peoples. Rather the language of kinship is used to express the social and political relationships of tenth- to ninth-century Palestine. See von Rad, *Genesis*, 140. In her study of the Tiv people of Nigeria, Laura Bohannon also found that traditional genealogies had little correspondence to actual kinship. Rather, they were constantly being revised to explain changes in then-existing power relationships. See Bohannon, "A Genealogical Charter," *Africa*, 22 (1952): 140, *passim*.

showed the Egyptians, the Berbers, and other conquered peoples as "sons of Ham." But history did not bear out the truth of the myth in this form. As the conquest swept into southwestern Europe, many of the earlier conquered peoples adopted Islam and distinguished themselves as soldiers and administrators. These nationalities did not look or behave like slaves. Actually they were the products of cultures more urbane and sophisticated than that of their conquerors. By the tenth century, however, the myth had been adjusted to the new reality: Egyptians and Berbers were exempted from the curse. Their reprieve was accomplished by one of two methods. Either the genealogy was revised so that North Africans, though still "sons of Ham," were descended through some son other than Canaan upon whom the curse had fallen; or the story itself was re-told in such a way that these particular sons of Ham were forgiven for the sin committed by their ancestor.⁵⁶

The blacks fared less well in the real world and thus in the world of myth. In sub-Saharan Africa most of them were protected neither by the peace of Islam nor by powerful indigenous states. Furthermore, most blacks passing through the institution of Muslim slavery did so after the frontiers of Islam had become stabilized and opportunities were more limited than they had been in the years of rapid expansion. Just as the blacks, free and slave, remained more or less permanently clustered at the bottom of the status ladder, so the status-defining myth assumed a relatively fixed and unmistakably racial form. A recounting of the story in the tenth century by the Persian historian, Tabari, is typical of the form it had assumed by the later Middle Ages: "Ham begot all blacks and people with crinkly hair. Yafit [Japheth] all who have broad faces and small eyes (that is, the Turkic peoples) and Sam [also called "Shem" or "Sem," the mythical ancestor of the "Semites"] all who have beautiful faces and beautiful hair (that is, the Arabs and Persians); Noah put a curse on Ham, according to which the hair of his descendants would not extend over their ears and they would be enslaved wherever they were encountered."⁵⁷

Popular genealogies often reveal more about existing power relationships than they do about the actual origins and kinship of peoples.⁵⁸ Three centuries before Tabari, for example, the Persians, after their conquest by Muslim armies, had sometimes been sold into slavery and classified as "sons of Ham."⁵⁹ But then a political upheaval brought the Abbassid caliphs to power and elevated Persians to positions of authority and influence throughout the vast territories of the caliphate.⁶⁰ As a result, by the tenth century Ta-

⁵⁶ Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 147-49; and *Encyclopaedia of Islam*, s.v. "Ham," by G. Vajda. Also see Mohammed Tabari, *Chronique . . .*, trans. Louis Dubeux (Paris, 1836), 107-08.

⁵⁷ Abū-Ga'far Muhammed b. Garir a-Tabarī, *Ta'rih ar-rusul wa'l-mulūk*, ed. J. DeGoeje, 1 (Leiden, 1879): 223, as quoted in Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 147. Though Persian, Tabarī wrote in Arabic and, indeed, is the author of what is regarded as the major Arabic historical work of the Middle Ages.

⁵⁸ Ignaz Goldziher, *Mythology among the Hebrews and Its Historical Development*, trans. Russell Martineau (New York, 1967), 357-59. Also see note 53, above.

⁵⁹ Haas, "Contributions of Slaves to Early Islam," 62; and Abd al-Husain Zarrinkuls, "The Arab Conquest of Iran and Its Aftermath," in *Cambridge History of Iran*, 2 (Cambridge, 1968): 29, 42, *passim*.

⁶⁰ *Encyclopaedia of Islam*, new ed., s.v. "Abbasids," by Bernard Lewis.

barī could perceive the Persians, though they were linguistically unrelated to either the Jews or the Arabs, as “Semites,” a people basking in Noah’s blessing and destined to be served by the now definitely sub-Saharan *Banu Ham*.

UNTIL NEARLY THE END OF THE MIDDLE AGES Christians showed no particular interest in Jewish and Muslim revisions of the Ham story. Their version, to the extent they related the story at all, was still the one that appears in Genesis, sometimes used to justify slavery, but one that remained innocent of racial overtones.⁶¹ In Christian Europe “slave” still meant “Slav,” not “Negro.” But already a development was taking place in the Christian lands of the Mediterranean that would have an enormous impact on the habits of Europeans, on their economy, and on the way they perceived blacks. In the eastern Mediterranean during the First Crusade, Europeans had learned to make sugar, a product that one chronicler described as an “unsuspected and inestimable present from Heaven.”⁶² Sugar production was as labor intensive as it was profitable, creating a strong demand for unskilled, closely supervised, work gangs. Initially, growers were able to answer their needs by investing in the labor supplied by the religious wars, captives sold into slavery, or religious refugees who could be employed under conditions approaching slavery. After the Crusaders were expelled from the Near East, their sugar industry was transferred westward to new areas, especially Cyprus, Crete, Sicily, and parts of Spain. The industry was extended further westward, mostly by Italians, to the Madeira Islands about 1432 and to the Canaries about 1480. It was also Italian merchants who supplied sugar planters with much of their labor, selling them “Slavs” from the Black Sea region.⁶³ Political developments in the Near East greatly affected the sugar industry. On the one hand, the disorder created by a new round of wars in the area and by ruinous Turkish commercial policies, helped Christian planters by eliminating their Muslim rivals.⁶⁴ But, on the other, these same policies threatened trade between

⁶¹ A late thirteenth-century English legal writer, for example, noted that “serfage, according to some, comes from the curse which Noah pronounced against Canaan, the son of his son Ham, and against his issue”; [Andrew Horn] *The Mirror of Justices*, ed. William Joseph Whittaker, Introduction by F. W. Maitland (London, 1895), 77. Also see Piero A. Milani, *La schiavitù nel pensiero politico dai Greci al basso Medio Evo* (Milan, 1972), 244, 292, 300, 309, 316, 355–57, 376–77, 377 n. 16.

⁶² Charles Verlinden, *Les origines de la civilisation atlantique de la Renaissance à l'Âge des Lumières* (Paris, 1966), 167.

⁶³ Origo, “The Domestic Enemy: The Eastern Slaves in Tuscany,” 361 n. 44; Charles Verlinden, “Aspects de l’esclavage dans les colonies médiévales italiennes,” *Hommage à Lucien Febvre*, 2 (Paris, 1953): 103; and Noel Deerr, *The History of Sugar*, 1 (London, 1949): chap. 8 *passim*, esp. 74, 76–80, 100, 115. Some sugar had been produced in Spain, mainly in the Costa del Sol region, since the Muslims had introduced it in the tenth century. It had flourished in conjunction with a secondary industry, the manufacture of various kinds of jams and concentrates for soft drinks made from local fruits. Although with the Reconquest and the re-establishment of Christianity many people favored wine over Muslim-type soft drinks, certain of these preparations such as grenadine and sasparrilla continued to be popular. Knowledge of the techniques for making jams and soft-drink concentrates continued to be regarded as a desirable accomplishment for slave women. Edmund O. von Lippmann, *Geschichte des Zuckers seit den ältesten Zeiten bis zum Beginn der Rubenzucker-Fabrikation* (Berlin, 1929), 241; and Ruth Pike, *Aristocrats and Traders: Sevillian Society in the Sixteenth Century* (Ithaca, N.Y., 1972), 177–78.

⁶⁴ W. Heyd, *Histoire du commerce du Levant au moyen âge*, 2 (Leipzig, 1923): 276 ff.; Poliak, “Révoltes populaires en Egypte,” 253–254.

the Black Sea and the Mediterranean and, hence, the labor supply of Christian planters. The price of sugar climbed, but so did the price of slaves.⁶⁵

In 1444 Diniz Dias, his caravel racing before the constant north wind, bypassed the fearful Saharan inferno and dropped anchor before the green land of Guinea. There the Portuguese found peoples of many languages and customs cultivating fields of rice, millet, and yams. Guinea, or more strictly Upper Guinea, is a relatively narrow strip of tropical rain forest extending along the southern and southwestern coast of the large western bulge of Africa. Even before the coming of the Europeans these peoples had suffered slave raids. For the great Sudanese savanna to the north, separating the rain forest from the desert and forming an east-west axis across the continent, may have played a role in the history of Guinea similar to that played by the Eurasian steppe in the history of Caucasia.

Higher and less humid than Guinea, Sudan sustained a healthier population that enjoyed a greater prosperity due to better conditions for agriculture and stock raising. Like the Eurasian steppe, medieval Sudan had been the scene of great empire building. These empires, however, had never extended into the relatively inaccessible areas of the rain forest with its comparatively poor and isolated village populations. Large-scale cavalry warfare, which built and overthrew the empires of the savanna, was ineffective in the forest lowlands. Furthermore, the lowlands were infested with the sleeping sickness-bearing tsetse fly, which is dangerous to man but deadly to horses.⁶⁶ Nevertheless, small-scale raids into the rain forests, especially slave raids, were feasible, since the raiders could usually be confident that they would face not armies but peasant communities that, because of the local nature of their political institutions, could not call on outside help.⁶⁷

Moreover, the conversion of Sudanese governing elites to Islam had results similar to those produced by the emergence of Christian governments in medieval Europe. On the one hand, the support of a well-organized, literate religion strengthened the position of kings, enabling them to offer greater protection to their own subjects. But, on the other, religion provided a rationale for slave

⁶⁵ Ashtor, *Histoire des prix et des salaires dans l'orient médiéval*, 460-61, 498-504; and Karl Schneider, "Der Sklavenhandel im mittelalterlichen Italien," *Zeitschrift für Sozialwissenschaft*, 20 (1907): 238.

⁶⁶ Jack Goody, "Introduction," in Jack Goody and Kwame Arhin, eds., *Ashanti and the Northwest* (Legon, Ghana, 1955), 82-83.

⁶⁷ Bolanle Awe, "Empires of the Western Sudan: Ghana, Mali, Songhai," in J. F. A. Ajayi and Ian Espie, eds., *A Thousand Years of West African History* (Ibadan, Nigeria, 1965), 57-59; and Christopher Fyfe, "Peoples of the Windward Coast, A.D. 1000-1800," in *ibid.*, 149-50, 158-59. Several scholars have held that the inevitable human disasters connected with building the medieval Sudanese empires caused a southward drift of population into the less favorable environment of the rain forests and thus contributed substantially to the cultural diversity of Guinea. See Akin Mabogunje, "The Land and Peoples of West Africa," in J. F. A. Ajayi and Michael Crowder, eds., *History of West Africa*, 1 (New York, 1972): 28; Jacques Richard-Molard, *Afrique occidentale française* (Paris, 1949), 108. Walter Rodney, who based his own work on that of Richard-Molard and Antonio Mendes Correia, the anthropologist, reached similar conclusions. See Rodney, *A History of the Upper Guinea Coast, 1545-1800* (Oxford, 1970), 5-6. Raymond Mauny, however, has had the "impression" that most of the peoples of Guinea had lived a very long time in those locations where they are first noted in Arabic and Portuguese sources. See Mauny, *Tableau géographique de l'ouest africain au moyen âge d'après les sources écrites, la tradition et l'archéologie* (Dakar, 1961), 448.

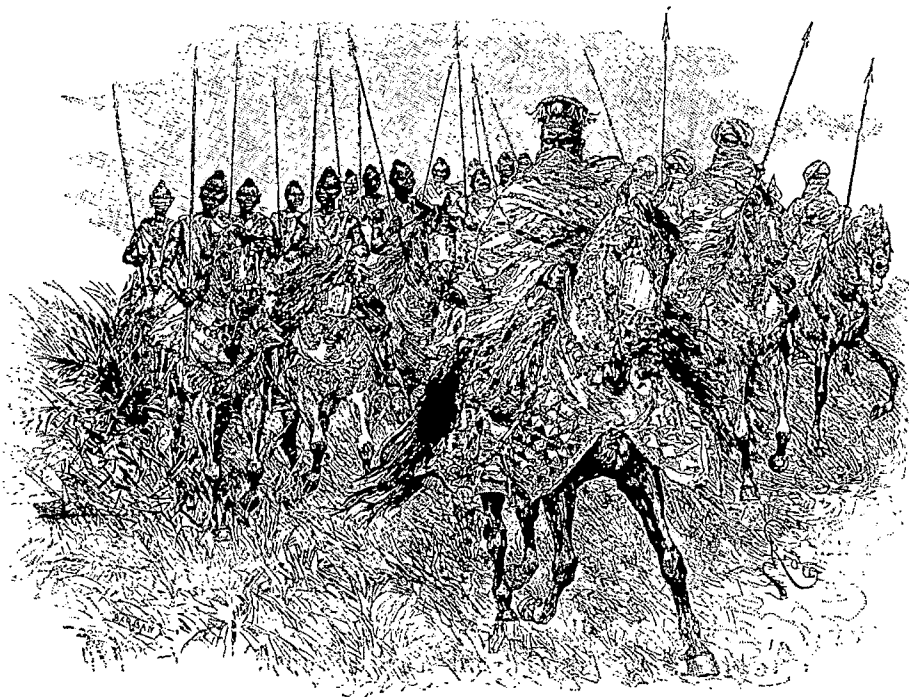


Figure 1: The Mossi prince, Boukary, later ruler of the kingdom, and his horsemen on a slave-raiding expedition. Photograph from a wood engraving, taken from Captain L. G. Binger's description of his two years in Africa; Binger, *Du Niger au Golfe de Guinée par le pays de Kong et le Mossi*, 1 (Paris, 1892): 455.

raids. With the growing volume of the trans-Saharan slave trade and, especially, with the opening of the Atlantic trade, Sudanese rulers showed an increasing tendency to proclaim a *jihad*, or holy war, against *Kaffirs* (unbelievers) as well as against Muslim heretics. Like similar movements in Europe, such as the *Drang nach Osten*, the *Reconquista*, or the Crusades, these campaigns delivered thousands of victims to the slave traders.⁶⁸

With the opening of South Atlantic navigation, the relative isolation of the peoples of Guinea ended. They found themselves between the hammer of Portuguese maritime power and the anvil of the Muslim states. Furthermore, Guinea, like the Caucasus region and the slave export areas of the Balkans, was a kaleidoscope of local cultures in which slave traders had no trouble finding ethnic antagonism from which they could profit.⁶⁹ Just as the Italian traders

⁶⁸ J. Spencer Trimingham, *Islam in West Africa* (Oxford, 1959), 28–30; Mauny, *Tableau géographique de l'ouest africain au moyen âge*, 541–42; and Rodney, *Upper Guinea Coast, 1545–1800*, 236. The Sudanese poet, Mohammed Bello, writing Hausa verse in Arabic script, voiced sentiments that would not have been unusual among the Crusaders, Junkers, or conquistadores of medieval Europe: "Brethren we thank God; / We perform acts of faith and prayer / Even holy war for Thee the Exalted One: / We slew the breed of dogs, . . . / We have slain the heathen." See J. H. Greenberg, "Hausa Verse Prosody," in *Journal of the American Oriental Society*, 69 (1949): 127.

⁶⁹ An official for the Royal Africa Company in 1726, noting the variety of languages along the Gambia such that the people on one side of the river could not understand those on the other, concluded that this situation was "no small Happiness to the Europeans who go thither to trade for slaves." Since Gambians, who were "naturally very lazy, abhorre slavery," the business could be quite dangerous. But, by "having some of every sort on board," there was "no more likelihood of their succeeding in a plot than finishing the Tower of Babel." William Smith, *A New Voyage to Guinea* (London, 1744), 28. Another observer noted that there were seven or eight languages spoken along a sixty-mile stretch of the Gold Coast and that three or four of these were not mutually comprehensible. William Bosman, *A New and Accurate Description of the Coast of Guinea . . .* (1704; 4th English ed., London, 1967), 130.

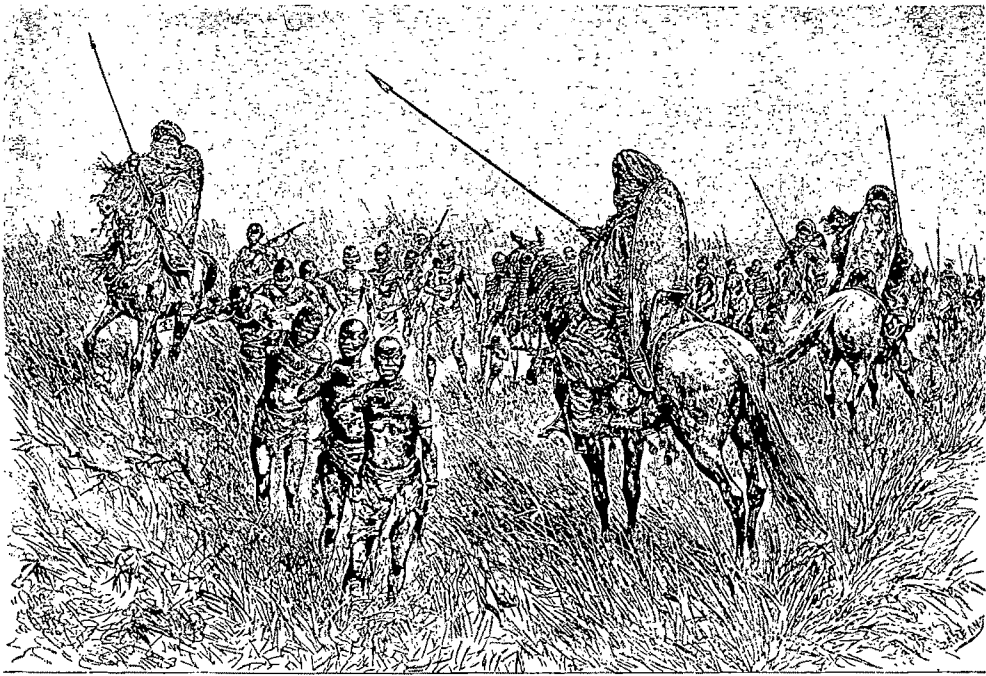


Figure 2: The Mossi horsemen returning with their captive slaves taken during a raid on noncentralized African peoples. During his exploration of Africa, Binger met Boukary in 1888 and described two such raiding parties conducted by the prince. Photograph from a wood engraving, taken from Binger, *Du Niger au Golfe de Guinée*, 471.

were furnishing arms to the Crimean Tartars in exchange for the captives taken in the raids against the Slavic and Caucasian peoples, so the Portuguese established a similar relationship with the Bijagos (or Bissagos) of Guinea.⁷⁰ The Europeans had thus discovered a vast source of black “Slavs”—a source that lay, as it turned out, not far from the major trade-wind, sailing route between Europe and the Americas.

At the same time, the Black Sea trade, the principal source of white slaves, became increasingly constricted. Nine years after the Portuguese opened up the maritime route to Guinea, Constantinople fell to the Turks, eliminating the last relic of Christian control over the Straits. To be sure, the Black Sea trade continued for a time but only on terms laid down by the Turks.⁷¹ Turkish restrictions on the buying and selling of Muslims not only sharply reduced one important source of light-skinned slaves but also, insofar as some Italian merchants were concerned, stood the religious justification for slavery on its head. Cautious about dealing in Muslims, Genoese and Venetian traders furnished the victorious Ottomans with ready cash for their surplus Christians, including Christian monks. These payments were not made for the pious medieval purpose of ransoming brothers in Christ. Inspired by a more modern spirit of enterprise, Ital-

⁷⁰ Rodney, *Upper Guinea Coast, 1545–1800*, 104. As early as 1460–62, according to a contemporary report, the Portuguese procured slaves through Muslim middlemen. See G. R. Crone, ed., *The Voyages of Cadamosto and Other Documents*, Hakluyt Society Publications, 2d ser., no. 80 (London, 1937), 18. Also see G. I. Bratianu, *Recherches sur le commerce génois dans la Mer Noire* (Paris, 1929), 289; and Verlinden, “Aspects de l’esclavage dans les colonies médiévales italiennes,” 94–95, *passim*.

⁷¹ A. H. Lybyer, “The Ottoman Turks and the Routes of Oriental Trade,” *English Historical Review*, 30 (1915): 580–84.

ian merchants snapped up these victims of Turkish expansion for use on the sugar plantations of their island colonies in the eastern Mediterranean or shipped them to the western Mediterranean where they were sold in competition with blacks whom the Portuguese had begun to bring up from Guinea.⁷² In the long run, however, the commerce of the Italian cities did not prosper in the lands controlled by the Turks. In the second quarter of the fifteenth century 91 percent of the slaves in Genoa were of Black Sea origin; by the last quarter of the century this figure had fallen to 26 percent.⁷³

Not only was the flow of slaves from the Black Sea area to the western Mediterranean becoming increasingly constricted, but also the largest single source of white slaves in the mid-fifteenth century, the Russians, virtually dried up. In 1462 Ivan the Great became grand duke of Moscow. His accession marked the end of a long period of political fragmentation as well as the emergence of a viable state, which, like the Ottoman state, could offer its subjects some protection against slave raids.⁷⁴ Of the Black Sea slaves being sold at Genoa, the proportion that was Russian fell from 41 percent in the second quarter of the fifteenth century to 18 percent in the last quarter. Furthermore, the rise in the median age of Russian slaves being sold in the last quarter suggests that many were resales who had been in Italy for some time and that among the freshly taken captives the proportion of Russians was even lower.⁷⁵

With this turn of events the reputation of Slavs began to improve. At the beginning of the fifteenth century the Tunisian historian, Ibn Khaldun, could still write that Negroes had character traits "close to those of dumb animals. It has even been reported that most of the Negroes of the first zone [the tropics] dwell in caves and thickets, eat herbs, live in savage isolation and do not congregate, and eat each other. The same applies to the Slavs."⁷⁶ But, before the century was over, as a victorious Slavic nation swept out onto the steppes and began the conquest of the Eurasian heartland, the image of Slavs began to change. Sugar production was expanding rapidly in the Atlantic islands, and the cane fields were being filled with "Slavs" who came from Guinea instead of the Crimea. The term "slave" was losing all of its psychological connection to Eastern Europe, just as centuries before the term "Canaanite" had lost all connection with Canaan.

As historical events redirected the slave trade, as European slavery entered what the leading authority on medieval slavery has called its "Negro" period,⁷⁷

⁷² I. Sukasov, "Documents récemment découverts datant de la fin du XIV^e siècle et concernant les Bulgares de la Macédoine vendus comme esclaves," *Makedonski pregled*, 7 (1932): 236; Charles Verlinden, "La Crète, débouché et plaque tournante de la traite des esclaves aux XIV^e et XV^e siècles, in *Studi in onore di Amintore Fanfani*, 3 (Milan, 1962): 630, *passim*; and communication from Charles Verlinden, June 15, 1979.

⁷³ For these estimates, see Gioffrè, *Il mercato degli schiavi a Genova nel secolo XV*, 61.

⁷⁴ George Vernadsky, *Russia at the Dawn of the Modern Age*, vol. 4 of George Vernadsky and Michael Karpovich, eds., *A History of Russia* (New Haven, 1959), 1-12, 96-101.

⁷⁵ Gioffrè, *Il mercato degli schiavi a Genova nel secolo XV*, 19.

⁷⁶ Ibn Khaldun, *The Muqaddimah: An Introduction to History*, trans. Franz Rosenthal, 1 (New York, 1958): 168-69.

⁷⁷ Charles Verlinden, "Schiavitù ed economia nel Mezzogiorno agli inizi dell'età moderna," *Annali del Mezzogiorno*, 3 (1963): 37. Also see Charles Verlinden, "Esclavage noir en France méridionale et courants de traite en Afrique," *Annales du Midi*, 78 (1966): 335-443; and Vicenta Cortes, *La esclavitud en Valencia durante el reinado de los Reyes Católicos, 1479-1516* (Valencia, 1964), 16.

Christians began to look at blacks in ways that had been characteristic of racially stratified Muslim countries for some seven centuries. Perhaps the bare beginnings of this change appear in a report that a mid-fifteenth-century Portuguese chronicler made to Prince Henry the Navigator. He wrote that a "noble Moor" captured on the Saharan coast had proposed through the Arabic interpreter that, if he were allowed to return home, his ransom would be paid in "black Moors." "Here you must note," the chronicler told the prince, "that these blacks were Moors like the others, though their slaves, in accordance with ancient custom, which I believe to have been because of the curse which, after the Deluge, Noah laid upon his son Cain [*sic*], cursing him in this way—that his race should be subject to all the other races of the world." But, when it turned out that the ten blacks that were actually delivered in payment of his ransom were not in fact Muslims, the writer piously added: "For though they were black, yet had they souls like the others, and all the more as these blacks were not of the lineage of the Moors but were Gentiles, and so the better to bring into the path of salvation."⁷⁸ Blacks were still "Gentiles." In time, Europeans would call them by other names, names less connected with religion.

IN THE TWO CENTURIES AFTER COLUMBUS, race relations in some of the recently discovered lands took a new turn. To the predominantly Negro, but racially mixed, slavery of the Christian and Muslim Old World was added the exclusively Negro slavery of the Americas. In their quest for an explanation for this development, a number of scholars have stressed the importance of the meaning that Old World peoples attributed to color. To these peoples angels were white, the devil was black. They associated white or lightness with such positive values as virtue, chastity, and purity and linked black or darkness with ignorance, filth, death, and sorrow. It has been suggested that such color values may have influenced early settlers of the American colonies so that, when confronted with a racially mixed labor force, colonists tended to favor whites over blacks, establishing a pattern of discrimination that resulted in the emergence of a system of slavery that was exclusively Negro.⁷⁹

But this hypothesis fails to account for the stable relationship that had long existed in the Old World between racially mixed slavery on the one hand and color prejudice on the other. Furthermore, after the brief "Negro period" of slavery in Europe in the second half of the fifteenth century, white slavery revived there—in the face of essentially the same color prejudice.⁸⁰ While any

⁷⁸ Gomes Eannes de Azurara, *The Chronicle of the Discovery and Conquest of Guinea*, Hakluyt Society Publications, 1st ser., no. 95 (London, 1895), 54–55.

⁷⁹ Philip Mason, *Prospero's Magic: Some Thoughts on Class and Race* (1962; 2d ed., Westport, Conn., 1975), 58, 77, *passim*; Kenneth J. Gergen, "The Significance of Skin Color in Human Relations," in Franklin, *Color and Race*, 114, 120; Talcott Parsons, "The Problem of Polarization on the Axis of Color," in *ibid.*, 358; Jordan, *White over Black*, ix–x, 9, 41, *passim*; Carl N. Degler, *Neither Black nor White: Slavery and Race Relations in Brazil and the United States* (New York, 1971), 208–12; and Joel Kovel, *White Racism: A Psychohistory* (New York, 1970), 232.

⁸⁰ Lybyer, *Ottoman Empire in the Time of Suleiman the Magnificent*, 45–61, *passim*; Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Phillip II*, trans. Siân Reynolds, 2 (New York, 1973): 754–55, 841–42; Antonio Domínguez Ortiz, "La esclavitud en Castilla durante la edad moderna," *Estudios de historia social de España*, 2 (1962): 380–81, 381 n. 24; von Hammer-Purgstall, *Geschichte des osmanischen Reiches*, 3: 167, 5: 321, 6: 154; and Paul W. Bamford, "Slaves for the Gallies of France, 1665–1700," in James Ford Bell, ed., *Mer-*

simple explanation for a complex development is risky, a more promising explanation of the rise of exclusively black slavery in the New World appears to be the differences in mortality rates of the various racial populations of the early modern world. According to Philip D. Curtin, "statistics now available suggest" that the death rate of blacks in the New World was "about one-third as high as that for European newcomers."⁸¹ Investment in a black slave was, therefore, far safer and more profitable in the Americas than investment in a white one. Thus, in the New World, during the sixteenth and seventeenth centuries, black slavery replaced the various forms of white servitude. In the Mediterranean world, however, where conditions were healthier for whites, slave dealers could continue to buy and sell blacks and whites with egalitarian evenhandedness, seemingly oblivious to the fact that their world of religious ideas had been populated by white angels and a black devil for thousands of years.

During these same two centuries when slavery in the New World was becoming exclusively black, several patterns of race relations emerged. On the one hand, there was a discernible legitimist impulse. The Iberian nations, beginning in the Portuguese colonies in India (and perhaps influenced by the caste system of that country), established a system of racial "castas," in which pure-blooded whites were the most privileged group. But this system failed throughout the Portuguese and Spanish empires, where the European settlers were overwhelmingly males. Even when white women finally began arriving in the colonies in greater numbers, they found themselves confronted by a strongly established paternalistic culture, one of the characteristics of which is a marked sexual division of labor: women strive to become specialists in virtue, men in sin. The Portuguese crown and benevolent societies, for example, strove mightily to establish the legitimate white family, even to the extent of subsidizing the dowries of chaste white girls. But, while male colonists showed considerable appreciation for the ideal that these young ladies reflected, as evidenced by the popularity of the Marian cult, they also showed a definite reluctance to give up their black and brown concubines: As a result, many splendid young white women, unable to find husbands, entered convents or returned to Portugal, taking their virtue with them.⁸²

chants and Scholars: Essays in the History of Exploration and Trade (Minneapolis, 1965), 171-91. Furthermore, the racially mixed slavery of the Mediterranean continued for at least a generation after the disappearance of black slavery from the Americas. According to Reuben Levy, Circassians and blacks were still being sold on the slave block in Istanbul in 1908; Levy, *The Social Structure of Islam* (Cambridge, 1957), 88. And, in 1948, the United Nations Declaration of Human Rights noted the persistence of this institution in the Near East; see Müller, "Sklaven," 77.

⁸¹ Curtin, "The Atlantic Slave Trade, 1600-1800," in Ajayi and Crowder, *History of West Africa*, 253. How little color values contributed to the shaping of American labor systems is further suggested by the simultaneous decline in Indian slavery in the same lands where there was a rise of exclusively black slavery and a decline of white servitude. See Almon Wheeler Lauber, *Indian Slavery in Colonial Times within the Present Limits of the United States* (New York, 1913). Also see Bamford, "Slaves for the Galleys of France," 185, 190.

⁸² A. J. R. Russell-Wood, *Hidalgos and Philanthropists: The Santa Casa da Misericórdia of Bahia, 1550-1755* (Berkeley and Los Angeles, 1968), 32-33, 179-83. Brazil's paternalistic culture, moreover, may have inhibited the bourgeois process of capital accumulation. Because aging patriarchs could count more sins than legitimate heirs, they sometimes willed property—property that in other societies would have perpetuated or increased the economic power of a family—to trust funds for financing masses for the dead. João de Mattos de Aguiar, who died in 1700, furnishes an extreme example of this tendency. His will provided for eleven thousand

The legitimate white family was numerically weak in the Iberian colonies, and its advocates suffered setbacks, as bastards—the brown-skinned sons of concubines and slaves—filled the ranks of the militia and sometimes served as officers and bureaucrats as well. What has replaced “castas” in the Iberian empires is a *café con leche* society, consisting of a racial continuum in which whites are clustered toward the top both socially and economically while blacks are clustered toward the bottom, but a society without clearly defined racial frontiers.⁸³ This pattern has long existed in many Muslim countries and, prior to the great discoveries, apparently had already been established in those parts of Portugal and Castile that had long remained under Islamic influence.⁸⁴ The Muslim-Iberian system involves a subtle interplay between the resources of each individual and the white racial prejudices of society that rewards the light-skinned and penalizes the dark. Its flexibility enables an occasional Negro to rise high and allows more than an occasional white to remain poor.

In the seventeenth century, as the system of “castas” began to disintegrate in the Iberian colonies, it was picked up by the northern European powers and applied more successfully in their colonies, where the sex ratio among white settlers was more balanced.⁸⁵ a white, colored, black, three-status system in the West Indies, and a biracial, two-status system in North America. The status systems established by the north European empires incorporated several features similar to the caste system of India, including status group endogamy or in-group marriage, the association of each status group with a particular kind of economic activity, and a corresponding degree of access to political power.⁸⁶ Yet the racial status hierarchies of these societies have never become as firmly established as has the caste system of India. Despite the most valiant efforts of that great school of status-sustaining mythology and taboo, the legitimate white family, the quasi-caste systems of the New World and South Africa have never been upheld by such an elaborate ideology as that which lends a certain stability to the caste system of India. Thus, low-status people of these societies do not to the same degree internalize caste values. Nor have the quasi-caste systems ever become self-policing. The perpetuation of patterns of caste behavior in these coun-

masses annually, at a cost of two hundred réis each, to be said for his soul and those of his parents and grandparents. *Ibid.*, 167–68. For the failure of legitimacy in other parts of the empire, see Charles Ralph Boxer, *Race Relations in the Portuguese Empire* (Oxford, 1963), 49–53, 116–17.

⁸³ Boxer, *Race Relations in the Portuguese Empire*, 50, 57, 73–76, 83–84. For the breakdown of the system of “castas” but the persistence of color prejudice, see Florestan Fernandes, *The Negro in Brazilian Society*, trans. Jacqueline D. Skiles *et al.* (New York, 1969), 12, 135, 168, 451 n. 20; Gonzalo Aguirre Beltrán, *La población negra de México* (Mexico City, 1972), 85, 93, 154, 198, 224–33, 267–80; and Franklin W. Knight, “Cuba,” in David W. Cohen and Jack P. Greene, eds., *Neither Slave nor Free: The Freedmen of African Descent in the Slave Societies of the New World* (Baltimore, 1972), 280. For an interpretation that stresses the differences between the American racial status groups and the East Indian caste system, see Magnus Mörner, *Race Mixture in the History of Latin America* (Boston, 1967), 53–56.

⁸⁴ E. Levi-Provençal, *Histoire de l'Espagne musulmane*, 3 (Paris, 1953): 186, 208; Domínguez Ortiz, “La esclavitud en Castilla durante la edad moderna,” 372.

⁸⁵ Herbert Moller, “Sex Composition and Correlated Cultural Patterns of Colonial America,” *William and Mary Quarterly*, 2 (1945): 131–37.

⁸⁶ H. Hoetink, *Caribbean Race Relations: A Study of Two Variants*, trans. Eva M. Hooykaas (New York, 1967); and Degler, *Neither Black nor White*. These studies provide an excellent comparison of the two basic patterns of New World race relations, though I disagree with some of the conclusions reached by each author.

tries, unlike in India, has continued to depend upon the heavy-handed intervention of a strong state, ultimately upon the pistols and sabers of militia companies manned by the white-skinned sons of the "uptight" legitimate family.

Despite the New World's history of exclusive black slavery, patterns of race relations have arisen there that share an underlying common feature with those of the Muslim world of the Near East and North Africa: each of these societies is a pigmentocracy, ruled by people with light skin. This racial distribution of power, established centuries ago by the clash of arms, is maintained by a system of color values that permits a governing elite to define eligibility to power and privilege in its own image. Such color values are expressed in a cluster of distance-creating ideas, ideas that attach characteristics to the Negro that were once attached to slaves of any origin and that make a caricature of the physical and cultural traits of sub-Saharan Africans. Such caricatures inevitably imply that the debasement of blacks is the result of their race and culture rather than their history of captivity and exploitation. These perceptions, which emerged in the racially stratified countries of the Muslim Mediterranean during the centuries immediately prior to the great discoveries, created neither racial stratification in the Old World nor exclusively black slavery in the New. Historically, they arose as an attempt to explain and justify already existing social relations, social relations that were the result of the physical processes of history. Their function is thus conservative rather than creative. That is, they function to conserve old social relationships rather than to create new ones.

But, while the distance-creating perceptions of blacks were by no means new, most of them going back at least to the eleventh century, these old ideas were now operating within a new social environment, within the ethos of emerging capitalism. This new environment made an important difference. Some of the world's greatest fortunes were being won from the extraction of precious metals and from the production of plantation commodities, particularly sugar. Although these industries were not confined to the New World, the Americas presented entrepreneurs not only with their greatest opportunities but also with their most acute problem: how to persuade people to work for bare subsistence in mines and on plantations located in a country of "open resources,"⁸⁷ where there was an abundance of land often free for the taking? How could entrepreneurs control other people's impulse to "get ahead" in a land of boundless opportunity? Slavery helped. But so have the various "color bars" and patterns of color prejudice that restrict upward mobility and, hence, function to maintain an adequate supply of cheap labor, making possible higher profits.

To be sure, in those parts of the Americas where the white man continued to deal with the black man as his, or somebody's, household servant, the more typical pattern of Old World racial prejudice still persisted—with all of its inconsistencies and its admixture of paternalism. But, wherever color prejudice has been

⁸⁷ For the theory that, "leaving militarism and other secondary causes out of consideration," slavery flourishes only in situations of "open resources" or frontier conditions, see H. J. Nieboer, *Slavery as an Industrial System* (The Hague, 1900), 306, 347, *passim*. The "Nieboer thesis" is further developed by W. Kloosterboer; see her *Involuntary Labour since the Abolition of Slavery* (Leiden, 1960).

reinforced by the profit motive, wherever relations with the blacks have been conditioned not only by face to face contact but also by the pressure of distant and impersonal markets, white prejudices have taken on such an immediacy, a consistency, a neurotic intensity that a number of scholars have understandably mistaken this attitude for an "irrational" psychological phenomenon rather than a discrete historical one.⁸⁸ In certain advanced capitalist countries, furthermore, these ideas about blacks have been transmitted to each successive generation by means of a legitimate white family, which, by uniting an appeal to immediate self-interest with moral zeal, has elevated an ancient cluster of perceptions and myths to the level of an impassioned ideology.

This investigation, thus, does not bear out the belief that patterns of race relations were shaped by pre-existing prejudices of whites. Rather, it supports the proposition that patterns of both race relations and prejudice are determined by power relationships. Inevitably, power relationships change, often for reasons that have little to do with what people think about status; and, as they change, old patterns of thought give way to new ones. Myths become transformed. Sometimes they are even discarded.

⁸⁸ Especially see H. Hoetink, *Slavery and Race Relations in the Americas: Comparative Notes on Their Nature and Nexus* (New York, 1973), 51, *passim*. Also see Kovel, *White Racism: A Psychohistory*, 95, *passim*; Degler, *Neither Black nor White*, 211, *passim*; Jordan, *White over Black*, vii, x, 9, *passim*.

Time, Space, and the Evolution of Afro-American Society on British Mainland North America

IRA BERLIN

TIME AND SPACE ARE THE USUAL BOUNDARIES of historical inquiry. The last generation of slavery studies in the United States has largely ignored these critical dimensions but has, instead, been preoccupied with defining the nature of American slavery, especially as compared with racial bondage elsewhere in the Americas. These studies have been extraordinarily valuable not only in revealing much about slave society but also in telling a good deal about free society. They have been essential to the development of a new understanding of American life centered on social transformation: the emergence of bourgeois society in the North with an upward-striving middle class and an increasingly self-conscious working class and the development of a plantocracy in the South with a segmented social order and ideals of interdependence, stability, and hierarchy. But viewing Southern slavery from the point of maturity, dissecting it into component parts, comparing it to other slave societies, and juxtaposing it to free society have produced an essentially static vision of slave culture. This has been especially evident in the studies of Afro-American life. From Stanley M. Elkins's *Sambo* to John W. Blassingame's Nat-Sambo-Jack typology, scholars of all persuasions have held time constant and ignored the influence of place. Even the most comprehensive recent interpretation of slave life, Eugene D. Genovese's *Roll, Jordan, Roll*, has been more concerned with explicating the dynamic of the patriarchal ideal in the making of Afro-American culture than in explaining its development in time and space. None of the histories written since World War II has equaled the temporal and spatial specificity of U. B. Phillips's *American Negro Slavery*.¹

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¹ Stanley Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959); John W. Blassingame, *The Slave Community: Plantation Life in the Antebellum South* (New York, 1972); Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974); and Ulrich B. Phillips, *American Negro Slavery* (New York, 1918). For a historical perspective on post-World War II scholarship on slavery, see David Brion

Recent interest in the beginnings of slavery on the mainland of British North America, however, has revealed a striking diversity in Afro-American life. During the seventeenth and eighteenth centuries, three distinct slave systems evolved: a Northern nonplantation system and two Southern plantation systems, one around Chesapeake Bay and the other in the Carolina and Georgia lowcountry. Slavery took shape differently in each with important consequences for the growth of black culture and society. The development of these slave societies depended upon the nature of the slave trade and the demographic configurations of blacks and whites as well as upon the diverse character of colonial economy. Thus, while cultural differences between newly arrived Africans and second and third generation Afro-Americans or creoles² everywhere provided the basis for social stratification within black society, African-creole differences emerged at different times with different force and even different meaning in the North, the Chesapeake region, and the lowcountry.³ A careful examination of the diverse development of Afro-American culture in the colonial era yields important clues for an understanding of the full complexity of black society in the centuries that followed.

THE NATURE OF SLAVERY AND THE DEMOGRAPHIC BALANCE of whites and blacks during the seventeenth and first decades of the eighteenth centuries tended to incorporate Northern blacks into the emerging Euro-American culture, even as

Davis, "Slavery and the Post-World War II Historians," *Daedalus*, 103 (1974): 1-16; and, on the importance of temporal change, see Herbert G. Gutman, "Slave Culture and Slave Family and Kin Network: The Importance of Time," *South Atlantic Urban Studies*, 2 (1978): 73-88.

² I have used these terms synonymously. Both are mined with difficulties. "Afro-American" has recently come into common usage as a synonym for "black" and "Negro" in referring to people of African descent in the United States. Although "creole" generally refers to native-born peoples, it has also been applied to people of partly European, but mixed racial and national, origins in various European colonies. In the United States, "creole" has also been specifically applied to people of mixed but usually non-African origins in Louisiana. Staying within the bounds of the broadest definition of "creole" and the literal definition of "Afro-American," I have used both terms to refer to black people of native American birth.

³ As used in this essay, the concept of acculturation or creolization does not mean the liquidation of a culture, only its transformation. African culture transported to the New World was not lost or destroyed but transformed. The transformation of Africans to Afro-Americans entailed the joining together of a variety of distinctive African cultures as well as the compounding of those cultures with various European and native American ones to create a new cultural type: the Afro-American. Scholars have only begun to study the making of Afro-American culture; therefore, any judgment about its nature and the process of its creation must be tentative and incomplete. I would emphasize that "Africans" and "creoles" as used here do not represent autonomous categories, if for no other reason than African and creole people were connected by ties of blood and kinship. Instead, these categories are used as two poles within a range of an historical experience that was varied and overlapping. The process of creolization was not always synchronized with generational change. Beginning with Melville J. Herskovits's *The Myth of the Negro Past* (New York, 1941), scholars have produced a wide-ranging theoretical literature on the question of cultural transformation of African people in the New World. For some that have been most useful for this essay, see Sidney W. Mintz and Richard Price, *An Anthropological Approach to the Caribbean Past* (Philadelphia, 1976); Melville J. Herskovits, "Problem, Method, and Theory in Afro-American Studies," *Phylon*, 7 (1946): 337-54; M. G. Smith, *The Plural Society in the British West Indies* (Berkeley and Los Angeles, 1965), and "The African Heritage in the Caribbean," in Vera Rubin, ed., *Caribbean Studies* (Seattle, 1960), 34-45; H. Orlando Patterson, "Slavery, Acculturation, and Social Change: The Jamaican Case," *British Journal of Sociology*, 17 (1966): 151-64; and Edward Brathwaite, *The Development of Creole Society in Jamaica, 1770-1820* (London, 1971), and "Caliban, Ariel, and Unprospero in the Conflict of Creolization: A Study of the Slave Revolt in Jamaica in 1831-32," in Vera Rubin and Arthur Tuden, eds., *Comparative Perspectives on Slavery in New World Plantation Societies*, Annals of the New York Academy of Sciences, no. 292 (New York, 1977), 41-62.

whites denied them a place in Northern society.⁴ But changes in the character of the slave trade during the middle third of the eighteenth century gave new impetus to African culture and institutions in the Northern colonies. By the American Revolution, Afro-American culture had been integrated into the larger Euro-American one, but black people remained acutely conscious of their African inheritance and freely drew on it in shaping their lives.

Throughout the colonial years, blacks composed a small fraction of the population of New England and the Middle Colonies. Only in New York and Rhode Island did they reach 15 percent of the population. In most Northern colonies the proportion was considerably smaller. At its height, the black population totaled 8 percent of the population of New Jersey and less than 4 percent in Massachusetts and Connecticut. But these colony-wide enumerations dilute the presence of blacks and underestimate the importance of slave labor. In some of the most productive agricultural regions and in the cities, blacks composed a larger share of the population, sometimes constituting as much as one-third of the whole and perhaps one-half of the work force.⁵ Although many Northern whites never saw a black slave, others had daily, intimate contact with them. And, although some blacks found it difficult to join together with their former countrymen, others lived in close contact.

The vast majority of Northern blacks lived and worked in the countryside. A few labored in highly capitalized rural industries—tanneries, salt works, and iron furnaces—where they often composed the bulk of the work force, skilled and unskilled. Iron masters, the largest employers of industrial slaves, also were often the largest slaveholders in the North. Pennsylvania iron masters manifested their dependence on slave labor when, in 1727, they petitioned for a reduction in the tariff on slaves so they might keep their furnaces in operation. Bloomeries and forges in other colonies similarly relied on slave labor.⁶ But in an overwhelmingly agrarian society only a small proportion of the slave population engaged in industrial labor.

Like most rural whites, most rural blacks toiled as agricultural workers. In southern New England, on Long Island, and in northern New Jersey, which contained the North's densest black populations, slaves tended stock and raised crops for export to the sugar islands. Farmers engaged in provisioning the West Indies with draft animals and foodstuffs were familiar with slavery and had easy

⁴ In the discussion of the Chesapeake region and the lowcountry, scholars have employed the term "Anglo-American" to refer to the culture of white people. Because of the greater diversity of origins of white peoples in the Middle Colonies, the term "Euro-American" seems more applicable to white culture in the North.

⁵ For a collection of the relevant censuses, see William S. Rossiter, *A Century of Population Growth* (Washington, 1909), 149–84. Also see Robert V. Wells, *The Population of the British Colonies in America before 1776: A Survey of Census Data* (Princeton, 1975), 69–143, and Wells's correction of the 1731 enumeration, "The New York Census of 1731," *New York Historical Society Quarterly*, 57 (1973): 255–59. For estimates of the Northern black population predating these censuses, see U.S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957* (Washington, 1960), 756.

⁶ Edgar J. McManus, *Black Bondage in the North* (Syracuse, N.Y., 1973), 42–43; Charles S. Boyer, *Early Forges and Furnaces in New Jersey* (Philadelphia, 1963), 30–31, 149, 166, 194–99, 239; Frances D. Pingeon, "Slavery in New Jersey on the Eve of the Revolution," in Williams C. Wright, ed., *New Jersey in the American Revolution* (rev. ed., Trenton, N.J., 1974), 51–52, 57; Darold D. Wax, "The Demand for Slave Labor in Colonial Pennsylvania," *Pennsylvania History*, 34 (1967): 334–35; and William Binning, *Pennsylvania Iron Manufacture in the Eighteenth Century* (Harrisburg, Pa., 1931), 122–25.

access to slaves. Some, like the Barbadian émigrés in northern New Jersey, had migrated from the sugar islands. Others, particularly those around Narragansett Bay, styled themselves planters in the West Indian manner. They built great houses, bred race horses, and accumulated slaves, sometimes holding twenty or more bondsmen. But, whatever the aspirations of this commercial gentry, the provisioning trade could not support a plantation regime. Most slaves lived on farms (not plantations), worked at a variety of tasks, and never labored in large gangs. No one in the North suggested that agricultural labor could be done only by black people, a common assertion in the sugar islands and the Carolina low-country. In northern New England, the Hudson Valley, and Pennsylvania, the seasonal demands of cereal farming undermined the viability of slavery. For most wheat farmers, as Peter Kalm shrewdly observed, "a Negro or black slave requires too much money at one time," and they relied instead on white indentured servants and free workers to supplement their own labor. Throughout the North's bread basket, even those members of the gentry who could afford the larger capital investment and the concomitant risk that slave ownership entailed generally depended on the labor of indentured servants more than on that of slaves. Fully two-thirds of the bond servants held by the wealthiest farmers in Lancaster and Chester counties, Pennsylvania, were indentured whites rather than chattel blacks. These farmers tended to view their slaves more as status symbols than as agricultural workers. While slaves labored in the fields part of the year, as did nearly everyone, they also spent a large portion of their time working in and around their masters' houses as domestic servants, stable keepers, and gardeners. Significantly, the wills and inventories of Northern slaveholders listed their slaves with other high status objects like clocks and carriages rather than with land or agricultural implements.⁷

The distinct demands of Northern agriculture shaped black life in the countryside. Where the provisioning trade predominated, black men worked as stock minders and herdsmen while black women labored as dairy maids as well as domestics of various kinds. The large number of slaves demanded by the provisioning trade and the ready access to horses and mules it allowed placed black companionship within easy reach of most bondsmen. Such was not always true in the cereal region. Living scattered throughout the countryside on the largest farms and working in the house as often as in the field, blacks enjoyed neither the mobility nor the autonomy of slaves employed in the provisioning trade. But, if the demands of Northern agriculture affected black life in different ways, almost all rural blacks lived and worked in close proximity to whites. Slaves

⁷ Kalm, *Peter Kalm's Travels in North America*, ed. and trans. A. B. Benson, 1 (New York, 1937): 205, as quoted in Alan Tully, "Patterns of Slaveholding in Colonial Pennsylvania: Chester and Lancaster Counties, 1729-1758," *Journal of Social History*, 6 (1973): 286; Lorenzo J. Greene, *The Negro in Colonial New England* (New York, 1942), 103-12; McManus, *Black Bondage in the North*, 40-41; Pigeon, "Slavery in New Jersey," 51; William D. Miller, "The Narragansett Planters," *American Antiquarian Society Proceedings*, 43 (1933): 67-71; Tully, "Patterns of Slaveholding in Colonial Pennsylvania," 284-303; Steven B. Frankt, "Patterns of Slave-Holding in Somerset County, N.J.," seminar paper, 1967, in Special Collections, Rutgers University Library, New Brunswick, N.J.; Wax, "The Demand for Slave Labor in Colonial Pennsylvania," 332-40; and Jerome H. Woods, Jr., "The Negro in Early Pennsylvania: The Lancaster Experience, 1730-1790," in Elinor Miller and Eugene D. Genovese, eds., *Plantation, Town, and County: Essays on the Local History of American Slave Society* (Urbana, Ill., 1974), 447-48.

quickly learned the rudiments of the English language, the Christian religion, the white man's ways. In the North, few rural blacks remained untouched by the larger forces of Euro-American life.

Northern slaves were also disproportionately urban. During the eighteenth century, a fifth to a quarter of the blacks in New York lived in New York City. Portsmouth and Boston contained fully a third of the blacks in New Hampshire and Massachusetts, and nearly half of Rhode Island's black population resided in Newport. Ownership of slaves was almost universal among the urban elite and commonplace among the middling classes as well. On the eve of the Revolution, nearly three-fourths of Boston's wealthiest quartile of propertyholders ranked in the slaveholding class. Fragmentary evidence from earlier in the century suggests that urban slave-ownership had been even more widespread but contracted with the growth of a free working class. Viewed from the top of colonial society, the observation of one visitor that there was "not a house in Boston" that did "not have one or two" slaves might be applied to every Northern city with but slight exaggeration.⁸

Urban slaves generally worked as house servants—cooking, cleaning, tending gardens and stables, and running errands. They lived in back rooms, lofts, closets, and, occasionally, makeshift alley shacks. Under these cramped conditions, few masters held more than one or two slaves. However they might cherish a large retinue of retainers, urban slaveholders rarely had the room to lodge them. Because of the general shortage of space, masters discouraged their slaves from establishing families in the cities. Women with reputations for fecundity found few buyers, and some slaveholders sold their domestics at the first sign of pregnancy. A New York master candidly announced the sale of his cook "because she breeds too fast for her owners to put up with such inconvenience," and others gave away children because they were an unwarranted expense. As a result, black women had few children, and their fertility ratio was generally lower than that of whites. The inability or unwillingness of urban masters to support large households placed a severe strain on black family life.⁹ But it also encouraged masters to allow their slaves to live out, hire their own time, and thereby gain a measure of independence and freedom.

Slave hirelings along with those bondsmen owned by merchants, warehouse

⁸ N. B. Shurtleff et al., eds., *Records of the Governor and Company of Massachusetts Bay in New England (1628-1698)*, 1 (Boston, 1853): 79, as quoted in Carl Bridenbaugh, *Cities in the Wilderness, 1625-1742* (New York, 1938), 49; Rossiter, *A Century of Population Growth*, 149-84; Greene, *The Negro in Colonial New England*, 78, 81-82, 84-88, 92-93; Gary B. Nash, "Slaves and Slaveowners in Colonial Philadelphia," *William and Mary Quarterly*, 3d ser., 30 (1973): 226-52; and Thomas Archdeacon, *New York City, 1664-1710: Conquest and Change* (Ithaca, N.Y., 1976), 46-47.

⁹ New York *Weekly Post-Boy*, May 17, 1756, as quoted in McManus, *Black Bondage in the North*, 38; Carl Bridenbaugh, *Cities in Revolt, 1743-1776* (New York, 1955), 88, 285-86, and *Cities in the Wilderness*, 153, 200-01; Nash, "Slaves and Slaveowners in Colonial Pennsylvania," 243-44; Archdeacon, *New York City*, 89-90; Rossiter, *A Century of Population Growth*, 170-80; Edgar J. McManus, *A History of Slavery in New York* (Syracuse, N.Y., 1966), 44-45, and *Black Bondage in the North*, 37-39; and Wells, *The Population in the British Colonies of America before 1776*, 116-23. The low ratio of women to children may have been the result of high child mortality as well as low fertility. In 1788, J. P. Brissot de Warville observed, "Married Negroes certainly have as many children as whites, but it has been observed that in the cities the death rate of Negro children is higher"; Brissot de Warville, *New Travels in the United States of America, 1788*, ed. Durand Echeverria (Cambridge, Mass., 1964), 232n.

keepers, and ship chandlers kept Northern cities moving. Working outside their masters' houses, these bondsmen found employment as teamsters, wagoners, and stockmen on the docks and drays and in the warehouses and shops that composed the essential core of the mercantile economy. In addition, many slaves labored in the maritime trades not only as sailors on coasting vessels, but also in the rope walks, shipyards, and sail factories that supported the colonial maritime industry. Generally, the importance of these slaves to the growth of Northern cities increased during the eighteenth century. Urban slavery moved steadily away from the household to the docks, warehouses, and shops, as demonstrated by the growing disproportion of slave men in the urban North. Aside from those skills associated with the maritime trades, however, few slaves entered artisan work. Only a handful could be found in the carriage trades that enjoyed higher status and that offered greater opportunity for an independent livelihood and perhaps the chance to buy freedom.¹⁰

In the cities as in the countryside, blacks tended to live and work in close proximity to whites. Northern slaves not only gained first-hand knowledge of their masters' world, but they also rubbed elbows with lower-class whites in taverns, cock fights, and fairs where poor people of varying status mingled.¹¹ If urban life allowed slaves to meet more frequently and enjoy a larger degree of social autonomy than did slavery in the countryside, the cosmopolitan nature of cities speeded the transformation of Africans to Afro-Americans. Acculturation in the cities of the North was a matter of years, not generations.

For many blacks, the process of cultural transformation was well under way before they stepped off the boat. During the first century of American settlement, few blacks arrived in the North directly from Africa. Although American slavers generally originated in the North, few gave priority to Northern ports. The markets to the south were simply too large and too lucrative. Slaves dribbled into the Northern colonies from the West Indies or the mainland South singly, in twos and threes, or by the score but rarely by the boatload. Some came on special order from merchants or farmers with connections to the West Indian trade. Others arrived on consignment, since few Northern merchants specialized in selling slaves. Many of these were the unsalable "refuse" (as traders contemptuously called them) of larger shipments. Northern slaveholders generally disliked these scourgings of the transatlantic trade who, the governor of Massachusetts observed, were "usually the worst servants they have"; they feared that the West Indian re-exports had records of recalcitrance and criminality as well as physical defects. In time, some masters may have come to prefer seasoned slaves because of their knowledge of English, familiarity with work routines, or resistance to New World diseases. But, whatever their preference, Northern colonies could not compete with the wealthier staple-producing colonies for prime African field hands. Before the 1740s, Africans appear to have

¹⁰ Nash, "Slaves and Slaveowners in Colonial Philadelphia," 248-52; Archdeacon, *New York City*, 89-90, esp. 89 n. 16; Greene, *The Negro in Colonial New England*, 111-18; and Bridenbaugh, *Cities in Revolt*, 88, 274, 285-86.

¹¹ Eric Foner, *Tom Paine and Revolutionary America* (New York, 1976), 48-56.

arrived in the North only when a temporary glut made sale impossible in the West Indies and the mainland South. Even then they did not always remain in the North. When conditions in the plantation colonies changed, merchants re-exported them for a quick profit. The absence of direct importation during the early years and the slow, random, haphazard entry of West Indian creoles shaped the development of black culture in the Northern colonies.¹² While the nature of the slave trade prevented the survival of tribal or even shipboard ties that figured so prominently in Afro-American life in the West Indies and the Lower South, it better prepared blacks to take advantage of the special circumstances of their captivity.

Newly arrived blacks, most already experienced in the New World and familiar with their proscribed status, turned Northern bondage to their advantage where they could. They quickly established a stable family life and, unlike newly imported Africans elsewhere on the continent, increased their numbers by natural means during the first generation. By 1708, the governor of Rhode Island observed that the colony's slaves were "supplied by the offspring of those they have already, which increase daily. . . ." The transplanted creoles also seized the opportunities provided by the complex Northern economy, the relatively close ties of master and slave, and, for many, the independence afforded by urban life. In New Amsterdam, for example, the diverse needs of the Dutch mercantile economy induced the West India Company, the largest slaveholder in the colony, to allow its slaves to live out and work on their own in return for a stipulated amount of labor and an annual tribute. "Half-freedom," as this system came to be called, enlarged black opportunities and allowed for the development of a strong black community. When the West India Company refused to make these privileges hereditary, "half-free" slaves organized and protested, demanding that they be allowed to pass their rights to their children. Failing that, New Amsterdam slaves pressed their masters in other ways to elevate their children's status. Some, hearing rumors that baptism meant freedom, tried to gain church membership. A Dutch prelate complained that these blacks "wanted nothing else than to deliver their children from bodily slavery, without striving for piety and Christian virtues." Even after the conquering English abolished "half-freedom" and instituted a more rigorous system of racial servitude, blacks continued to use the leverage gained by their prominent role in the city's economy to set standards of treatment well above those in the plantation colonies. Into the eighteenth century, New York slaves informally enjoyed the rights of an earlier era, including the right to hold property of their own. "The

¹² W. N. Sainsbury et al., eds., *Calendar of State Papers, Colonial Series, 1708-1709*, 110, as quoted in Greene, *The Negro in Colonial New England*, 35; McManus, *Black Bondage in the North*, 18-25, and *Slavery in New York*, 23-39; James G. Lydon, "New York and the Slave Trade, 1700 to 1774," *William and Mary Quarterly*, 3d ser., 35 (1978): 275-79, 381-90; Greene, *The Negro in Colonial New England*, 15-45; and Darold D. Wax, "Negro Imports into Pennsylvania, 1720-1766," *Pennsylvania History*, 32 (1965): 254-87, and "Preferences for Slaves in Colonial America," *Journal of Negro History*, 58 (1973): 374-76, 379-87. So many of the slaves entering the North were re-exports from other parts of the Americas that Philip D. Curtin has not included the North in his calculation of the African population transported to the New World; see *The Atlantic Slave Trade: A Census* (Madison, Wisc., 1969), 143.

Custom of this Country," bristled a frustrated New York master to a West Indian friend, "will not allow us to use our Negroes as you doe in Barbados."¹³

Throughout the North, the same factors that mitigated the harshest features of bondage in New York strengthened the position of slaves in dealing with their masters. Small holdings, close living conditions, and the absence of gang labor drew masters and slaves together. A visitor to Connecticut noted in disgust that slaveowners were "too Indulgent (especially the farmers) to their Slaves, suffering too great a familiarity from them, permitting them to sit at Table and eat with them (as they say to save time) and into the dish goes the black hoof as freely as the white hand." Slaves used knowledge gained at their masters' tables to press for additional privileges: the right to visit friends, live with their families, or hire their own time. One slaveholder reluctantly cancelled the sale of his slaves because of "an invariable indulgence here to permit Slaves of any kind of worth or Character who must change Masters, to choose those Masters," and he could not persuade his slaves "to leave their Country (if I may call it so), their acquaintances & friends."¹⁴ Such indulgences originated not only in the ability of slaves to manipulate their masters to their own benefit, but also from the confidence of slaveholders in their own hegemony. Surety of white dominance, derived from white numerical superiority, complemented the blacks' understanding of how best to bend bondage to their own advantage and to maximize black opportunities within slavery.

DURING the middle decades of the eighteenth century, the nature of Northern slavery changed dramatically. Growing demand for labor, especially when European wars limited the supply of white indentured servants and when depression sent free workers west in search of new opportunities, increased the importance of slaves in the work force. Between 1732 and 1754, blacks composed fully a third of the immigrants (forced and voluntary) arriving in New York. The new importance of slave labor changed the nature of the slave trade. Merchants who previously took black slaves only on consignment now began to import them directly from Africa, often in large numbers. Before 1741, for example, 70 percent of the slaves arriving in New York originated in the West Indies and other mainland sources and only 30 percent came directly from Africa. After that date, the proportions were reversed. Specializing in the slave trade, African slavers carried many times more slaves than did West Indian traders. Whereas slaves had earlier arrived in small parcels rarely numbering more than a half-

¹³ Governor Samuel Cranston to the Board of Trade, December 5, 1708, in J. R. Bartlett, ed., *Records of the Colony of Rhode Island and Providence Plantations*, 4 (1860): 55, as quoted in Miller, "Narragansett Planters," 68 n. 2; and Cadwallader Colden to Mr. Jordan, March 26, 1717, in *Letters and Papers of Cadwallader Colden*, 1 (New York, 1917): 39, as quoted in Arthur Zilversmit, *The First Emancipation: The Abolition of Negro Slavery in the North* (Chicago, 1967), 22. Joyce D. Goodfriend, "Burghers and Blacks: The Evolution of a Slave Society at New Amsterdam," *New York History*, 59 (1978): 125-44; McManus, *Slavery in New York*, 2-22; and Gerald F. DeJong, "The Dutch Reformed Church and Negro Slavery in Colonial America," *Church History*, 40 (1971): 430.

¹⁴ Sara Kemble Knight, as quoted in Ralph F. Weld, *Slavery in Connecticut* (New Haven, 1935), 8-9; John Watts, *Letterbook of John Watts*, New York Historical Society Collections, no. 61 (New York, 1938), 151; and McManus, *Black Bondage in the North*, *passim*.

dozen, direct shipments from Africa at times now totaled over a hundred and, occasionally, several times that. Slaves increasingly replaced white indentured servants as the chief source of unfree labor not only in the areas that had produced for the provisioning trade, where their pre-eminence had been established earlier in the century, but in the cities as well. In the 1760s, when slave importation into Pennsylvania peaked, blacks composed more than three-quarters of Philadelphia's servant population.¹⁵

Northern whites generally viewed this new wave of slaves as substitutes for indentured labor. White indentured servants had come as young men without families, and slaves were now imported in much the same way. "For this market they must be young, the younger the better if not quite children," declared a New York merchant. "Males are best." As a result, the sex ratio of the black population, which earlier in the century had been roughly balanced, suddenly swung heavily in favor of men. In Massachusetts, black men outnumbered black women nearly two to one. Elsewhere sex ratios of 130 or more became commonplace.¹⁶ Such sexual imbalance and the proscription of interracial marriage made it increasingly difficult for blacks to enjoy normal family lives. As the birth rate slipped, mortality rates soared, especially in the cities where newly arrived blacks appeared to be concentrated. Since most slaves came without any previous exposure to New World diseases, the harsh Northern winters took an ever higher toll. Blacks died by the score; the crude death rate of Philadelphia and Boston blacks in the 1750s and 1760s was well over sixty per thousand, almost double that of whites.¹⁷ In its demographic outline, Northern slavery at mid-century often bore a closer resemblance to the horrors of the West Indies during the height of a sugar boom than to the relatively benign bondage of the earlier years.

Whites easily recovered from this demographic disaster by again switching to European indentured servants and then to free labor as supplies became available, and, as the influx of slaves subsided, black life also regained its balance. But the transformation of Northern slavery had a lasting influence on the development of Afro-American culture. Although the Northern black population remained predominantly Afro-American after nearly a century of slow importation from the West Indies and steady natural increase, the direct entry of Africans into Northern society reoriented black culture.

Even before the redirection of the Northern slave trade, those few Africans in the Northern colonies often stood apart from the creole majority. While Afro-American slaves established precedents and customs, which they then drew upon to improve their condition, Africans tended to stake all to recapture the

¹⁵ Nash, "Slaves and Slaveowners in Colonial Philadelphia," 226-37; Lydon, "New York and the Slave Trade," 387-88; and Darold D. Wax, "Quaker Merchants and the Slave Trade in Colonial Pennsylvania," *Pennsylvania Magazine of History and Biography*, 86 (1962): 145, and "Negro Imports into Pennsylvania," 256-57, 280-87.

¹⁶ Watts, *Letterbook of John Watts*, 31; McManus, *Black Bondage in the North*, 38-39; Wax, "Preferences for Slaves in Colonial America," 400-01; Rossiter, *A Century of Population Growth*, 149-84; and Greene, *The Negro in Colonial New England*, 93-96.

¹⁷ Nash, "Slaves and Slaveowners in Colonial Philadelphia," 232-41, esp. n. 46.

world they had lost. Significantly, Africans, many of whom did not yet speak English and still carried tribal names, composed the majority of the participants in the New York slave insurrection of 1712, even though most of the city's blacks were creoles.¹⁸ The division between Africans and Afro-Americans became more visible as the number of Africans increased after mid-century. Not only did creoles and Africans evince different aspirations, but their life-chances—as reflected in their resistance to disease and their likelihood of establishing a family—also diverged sharply. Greater visibility may have sharpened differences between creoles and Africans, but Africans were too few in number to stand apart for long. Whatever conflicts different life-chances and beliefs created, whites paid such distinctions little heed in incorporating the African minority into their slaveholdings. The propensity of Northern whites to lump blacks together mitigated intraracial differences. Rather than permanently dividing blacks, the entry of Africans into Northern society gave a new direction to Afro-American culture.¹⁹

Newly arrived Africans reawakened Afro-Americans to their African past by providing direct knowledge of West African society. Creole blacks began to combine their African inheritance into their own evolving culture. In some measure, the easy confidence of Northern whites in their own dominance speeded the syncretization of African and creole culture by allowing blacks to act far more openly than slaves in the plantation colonies. Northern blacks incorporated African culture into their own Afro-American culture not only in the common-place and unconscious way that generally characterizes the transit of culture but also with a high degree of consciousness and deliberateness. They designated their churches "African," and they called themselves "Sons of Africa."²⁰ They adopted African forms to maximize their freedom, to choose their leaders, and, in general, to give shape to their lives. This new African influence was manifested most fully in Negro election day, a ritual festival of role reversal common throughout West Africa and celebrated openly by blacks in New England and a scattering of places in the Middle Colonies.

The celebration of Negro election day took a variety of forms, but everywhere it was a day of great merrymaking that drew blacks from all over the countryside. "All the various languages of Africa, mixed with broken and ludicrous English, filled the air, accompanied with the music of the fiddle, tambourine, the banjo, [and] drum," recalled an observer of the festival in Newport. Negro election day culminated with the selection of black kings, governors, and judges. These officials sometimes held symbolic power over the whole community and real power over the black community. While the black governors held court, ad-

¹⁸ Kenneth Scott, "The Slave Insurrection in New York in 1712," *New York Historical Society Quarterly*, 45 (1961): 43-74, esp. 62-67.

¹⁹ The shortage of African women and a sexual balance among Indians and, to a lesser extent, whites that favored women encouraged black men to marry Indian and, occasionally, white women, especially in New England; Winthrop D. Jordan, "American Chiaroscuro: The Status and Definition of Mulattoes in the British Colonies," *William and Mary Quarterly*, 3c ser., 19 (1962): 197-98, esp. n. 28.

²⁰ For petitions by blacks, see Robert C. Twombly, "Black Resistance to Slavery in Massachusetts," in William L. O'Neill, ed., *Insights and Parallels* (Minneapolis, 1973), 13-16; and, for various association names, see Dorothy Porter, ed., *Early Negro Writings, 1760-1837* (Boston, 1971).

judicating minor disputes, the blacks paraded and partied, dressed in their masters' clothes and mounted on their masters' horses. Such role reversal, like similar status inversions in Africa and elsewhere, confirmed rather than challenged the existing order, but it also gave blacks an opportunity to express themselves more fully than the narrow boundaries of slavery ordinarily allowed. Negro election day permitted a seeming release from bondage, and it also provided a mechanism for blacks to recognize and honor their own notables. Most important, it established a framework for the development of black politics. In the places where Negro election day survived into the nineteenth century, its politics shaped the politics within the black community and merged with partisan divisions of American society. Slaves elsewhere in the New World also celebrated this holiday, but whites in the plantation colonies found the implications of role reversal too frightening to allow even symbolically. Northern whites, on the other hand, not only aided election day materially but sometimes joined in themselves. Still, white cooperation was an important but not the crucial element in the rise of Negro election day. Its origin in the 1740s and 1750s suggests how the entry of Africans reoriented Afro-American culture at a formative point in its development.²¹

African acculturation in the Northern colonies at once incorporated blacks into American society and sharpened the memory of their African past and their desire to preserve it. While small numbers and close proximity to whites forced blacks to conform to the forms of the dominant Euro-American culture, the confidence of whites in their own hegemony allowed black slaves a good measure of autonomy. In this context it is not surprising that a black New England sea captain established the first back-to-Africa movement in mainland North America.²²

UNLIKE AFRICAN ACCULTURATION IN THE NORTHERN COLONIES, the transformation of Africans into Afro-Americans in the Carolina and Georgia lowcountry was a slow, halting process whose effects resonated differently within black society. While creolization created a unified Afro-American population in the North, it left lowcountry blacks deeply divided. A minority lived and worked in close proximity to whites in the cities that lined the rice coast, fully conversant with the most cosmopolitan sector of lowland society. A portion of this urban elite, increasingly light-skinned, pressed for further incorporation into white society, confident they could compete as equals. The mass of black people, however, remained physically separated and psychologically estranged from the Anglo-American world and culturally closer to Africa than any other blacks on continental North America.

²¹ Henry Bull, "Memoir of Rhode Island," Newport *Rhode-Island Republican*, April 19, 1837, as quoted in William D. Pierson, "Afro-American Culture in Eighteenth-Century New England" (Ph.D. dissertation, Indiana University, 1975), 181; Joseph P. Reidy, "'Negro Election Day' and Black Community Life in New England, 1750-1860," *Marxist Perspectives*, 1 (1978): 102-17; Alice M. Earle, *Colonial Days in Old New York* (5th ed., New York, 1922); Woods, "The Negro in Early Pennsylvania," 451; and Pierson, "Afro-American Culture in Eighteenth-Century New England," 181-313.

²² Peter Williams, *A Discourse, Delivered in the Death of Capt. Paul Cuffee* (New York, 1817).

The sharp division was not immediately apparent. At first it seemed that African acculturation in the Lower South would follow the Northern pattern. The first blacks arrived in the lowcountry in small groups from the West Indies. Often they accompanied their owners and, like them, frequently immigrated in small family groups. Many had already spent considerable time on the sugar islands, and some had doubtless been born there. Most spoke English, understood European customs and manners, and, as their language skills and family ties suggest, had made the difficult adjustment to the conditions of black life in the New World.

As in the Northern colonies, whites dominated the population of the pioneer Carolina settlement. Until the end of the seventeenth century, they composed better than two-thirds of the settlers. During this period and into the first years of the eighteenth century, most white slaveholders engaged in mixed farming and stock raising for export to the West Indian islands where they had originated. Generally, they lived on small farms, held few slaves, and worked closely with their bond servants. Even when they hated and feared blacks and yearned for the prerogatives of West Indian slave masters, the demands of the primitive, labor-scarce economy frequently placed master and slave face-to-face on opposite sides of a sawbuck.²³ Such direct, equalitarian confrontations tempered white domination and curbed slavery's harshest features.

White dependence on blacks to defend their valuable lowland beachhead reinforced this "sawbuck equality." The threat of invasion by the Spanish and French to the south and Indians to the west hung ominously over the lowcountry during its formative years. To bolster colonial defenses, officials not only drafted slaves in time of war but also regularly enlisted them into the militia. In 1710 Thomas Nairne, a knowledgeable Carolina Indian agent, observed that "enrolled in our Militia [are] a considerable Number of active, able, Negro Slaves; and Law gives every one of those his freedom, who in Time of an Invasion kills an Enemy." Between the settlement of the Carolinas and the conclusion of the Yamasee War almost fifty years later, black soldiers helped fend off every military threat to the colony. Although only a handful of slaves won their freedom through military service, the continued presence of armed, militarily experienced slaves weighed heavily on whites. During the Yamasee War, when the governor of Virginia demanded one Negro woman in return for each Virginia soldier sent to defend South Carolina, the beleaguered Carolinians rejected the offer, observing that it was "impracticable to Send Negro Women in their Roomes by reason of the Discontent such Usage would have given their husbands to have their wives taken from them which might have occasioned a Revolt."²⁴

²³ Peter H. Wood, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974), 13-24, 94-97. The image is derived from an account of a French refugee living near the Santee River who reported in 1697 that "he worked many days with a Negro man at the Whip saw"; Alexander S. Salley, ed., "Journal of General Peter Horry," *South Carolina Historical Magazine*, 38 (1937): 51-52, as quoted in *ibid.*, 97.

²⁴ Memorial of Joseph Boone and Richard Beresford to the Lord Commissioners of Trade and Plantations, December 6, 1716, Public Record Office, London, as quoted in Clarence L. Ver Steeg, *Origins of a Southern Mosaic: Studies of Early Carolina and Georgia* (Athens, Ga., 1975), 106; Wood, *Black Majority: Negroes in Colonial*

The unsettled conditions that made the lowcountry vulnerable to external enemies strengthened the slave's hand in other ways. Confronted by an overbearing master or a particularly onerous assignment, many blacks took to the woods. Truancy was an easy alternative in the thinly settled, heavily forested lowcountry. Forest dangers generally sent truant slaves back to their owners, but the possibility of another flight induced slaveholders to accept them with few questions asked. Some bondsmen, however, took advantage of these circumstances to escape permanently. Maroon colonies existed throughout the lowland swamps and into the backcountry. Maroons lived a hard life, perhaps more difficult than slaves, and few blacks chose to join these outlaw bands. But the ease of escape and the existence of a maroon alternative made masters chary about abusing their slaves.²⁵

The transplanted African's intimate knowledge of the subtropical lowland environment—especially when compared to the Englishman's dense ignorance—magnified white dependence on blacks and enlarged black opportunities within the slave regime. Since the geography, climate, and topography of the lowcountry more closely resembled the West African than the English countryside, African not European technology and agronomy often guided lowland development. From the first, whites depended on blacks to identify useful flora and fauna and to define the appropriate methods of production. Blacks, adapting African techniques to the circumstances of the Carolina wilderness, shaped the lowland cattle industry and played a central role in the introduction and development of the region's leading staple. In short, transplanted Englishmen learned as much or more from transplanted Africans as did the former Africans from them.²⁶ While whites eventually appropriated this knowledge and turned it against black people to rivet tighter the bonds of servitude, white dependence on African know-how operated during those first years to place blacks in managerial as well as menial positions and thereby permitted blacks to gain a larger share of the fruits of the new land than whites might otherwise allow. In such circumstances, white domination made itself felt, but both whites and blacks incorporated much of West African culture into their new way of life.

The structure of the fledgling lowland economy and the demands of stock raising, with deerskins as the dominant "crop" during the initial years of settlement, allowed blacks to stretch white military and economic dependence into generous grants of autonomy. On the small farms and isolated cowpens (hardly plantations by even the most latitudinous definition), rude frontier conditions permitted only perfunctory supervision and the most elementary division of labor. Most units were simply too small to employ overseers, single out specialists, or benefit from the economies of gang labor. White, red, and black laborers of varying legal status worked shoulder to shoulder, participating in the dullest

South Carolina, 124–30; Ver Steeg, *Origins of a Southern Mosaic*, 105–07; and Verner W. Crane, *The Southern Frontier, 1670–1732* (Durham, N.C., 1928), 162–81.

²⁵ John D. Duncan, "Servitude and Slavery in Colonial South Carolina, 1670–1776" (Ph.D. dissertation, Emory University, 1971), 587–601; and Herbert Aptheker, "Maroons within the Present Limits of the United States," *Journal of Negro History*, 24 (1939): 167–84.

²⁶ Wood, *Black Majority: Negroes in Colonial South Carolina*, 35–62, 119–30.

drudgery as well as the most sophisticated undertakings. Rather than skilled artisans or prime field hands, most blacks could best be characterized as jacks-of-all-trades. Since cattle roamed freely through the woods until fattened for market, moreover, black cowboys—suggestively called “cattle chasers”—moved with equal freedom through the countryside, gaining full familiarity with the terrain.²⁷ The autonomy of the isolated cowpen and the freedom of movement stock raising allowed made a mockery of the total dominance that chattel bondage implied. Slaves set the pace of work, defined standards of workmanship, and divided labor among themselves, doubtless leaving a good measure of time for their own use. The insistence of many hard-pressed frontier slaveowners that their slaves raise their own provisions legitimated this autonomy. By law, slaves had Sunday to themselves. Time allowed for gardening, hunting, and fishing both affirmed slave independence and supplemented the slave diet. It also enabled some industrious blacks to produce a small surplus and to participate in the colony's internal economy, establishing an important precedent for black life in the lowcountry.²⁸

Such independence burdened whites. They complained bitterly and frequently about blacks traveling unsupervised through the countryside, congregating in the woods, and visiting Charles Town to carouse, conspire, or worse. Yet knowledge of the countryside and a willingness to take the initiative in hunting down cattle or standing up to Spaniards were precisely the characteristics that whites valued in their slaves. They complained but they accepted. Indeed, to resolve internal disputes within their own community, whites sometimes promoted black participation in the affairs of the colony far beyond the bounds later permitted slaves or even black freemen. “For this last election,” grumbled several petitioners in 1706, “Jews, Strangers, Sailors, Servants, Negroes, & almost every French Man in Craven & Berkly County came down to elect, & their votes were taken.”²⁹ Such breaches of what became an iron law of Southern racial policy suggest how the circumstances of the pioneer lowcountry life shrank the social as well as the cultural distance between transplanted Africans and the *mélange* of European settlers. During the first generations of settlement, Afro-American and Anglo-American culture and society developed along parallel lines with a large degree of overlap.

²⁷ *Ibid.*, 28–34; Converse D. Clowse, *Economic Beginnings of Colonial South Carolina, 1670–1730* (Columbia, S.C., 1971), 61; Crane, *The Southern Frontier, 1670–1732*, 91, 120, 163, 184–85; Ver Steeg *Origins of a Southern Mosaic*, 114–16; Gary S. Dunbar, “Colonial Carolina Cowpens,” *Agricultural History*, 35 (1961): 125–30; and David L. Coon, “The Development of Market Agriculture in South Carolina, 1670–1785” (Ph.D. dissertation, University of Illinois, Urbana-Champaign, 1972), 113–14, 134–37. Georgia developed later than South Carolina; a description of an isolated cowpen in the Georgia countryside in 1765 may, therefore, suggest practices of an earlier era in South Carolina. See Harold E. Davis, *The Fledgling Province: Social and Cultural Life in Colonial Georgia, 1733–1776* (Chapel Hill, N.C., 1976), 67–68.

²⁸ Frank J. Klingberg, *An Appraisal of the Negro in Colonial South Carolina* (Washington, 1941), 6–7; Klaus G. Leowald, Beverly Starika, and Paul S. Taylor, trans. and eds., “Johann Martin Bolzius Answers a Questionnaire on Carolina and Georgia,” *William and Mary Quarterly*, 3d ser., 14 (1957): 235–36, 256; Thomas Cooper and David J. McCord, comps., *The Statutes at Large of South Carolina*, 10 vols. (Columbia, S.C., 1836–41), 7: 404; and Wood, *Black Majority: Negroes in Colonial South Carolina*, 62. For black participation in the internal economy of the sugar islands, see Sidney W. Mintz, *Caribbean Transformations* (Chicago, 1974), esp. chap. 7.

²⁹ “The Representation and Address of Several Members of This Present Assembly,” in William James Rivers, *A Sketch of the History of South Carolina* (Charleston, S.C., 1856), 459, as quoted in Ver Steeg, *Origins of a Southern Mosaic*, 38 (italics removed); and Wood, *Black Majority: Negroes in Colonial South Carolina*, 102–03.

If the distinction between white and black culture remained small in the lowcountry, so too did differences within black society. The absence of direct importation of African slaves prevented the emergence of African-creole differences; and, since few blacks gained their liberty during those years, differences in status within the black community were almost nonexistent. The small radius of settlement and the ease of water transportation, moreover, placed most blacks within easy reach of Charles Town. A "city" of several dozen rude buildings where the colonial legislature met in a tavern could hardly have impressed slaves as radically different from their own primitive quarters. Town slaves, for their part, doubtless had first-hand familiarity with farm work as few masters could afford the luxury of placing their slaves in livery.³⁰

Thus, during the first years of settlement, black life in the lowcountry, like black life in the North, evolved toward a unified Afro-American culture. Although their numbers combined with other circumstances to allow Carolina blacks a larger role in shaping their culture than that enjoyed by blacks in the North, there remained striking similarities in the early development of Afro-American life in both regions. During the last few years of the seventeenth century, however, changes in economy and society undermined these commonalities and set the development of lowcountry Afro-American life on a distinctive course.

THE discovery of exportable staples, first naval stores and then rice and indigo, transformed the lowcountry as surely as the sugar revolution transformed the West Indies. Under the pressure of the riches that staple production provided, planters banished the white yeomanry to the hinterland, consolidated small farms into large plantations, and carved new plantations out of the malaria-ridden swamps. Before long, black slaves began pouring into the region and, sometime during the first decade of the eighteenth century, white numerical superiority gave way to the lowcountry's distinguishing demographic characteristic: the black majority.

Black numerical dominance grew rapidly during the eighteenth century. By the 1720s, blacks outnumbered whites by more than two to one in South Carolina. In the heavily settled plantation parishes surrounding Charles Town, blacks enjoyed a three to one majority. That margin grew steadily until the disruptions of the Revolutionary era, but it again increased thereafter. Georgia, where metropolitan policies reined planter ambition, remained slaveless until mid-century. Once restrictions on slavery were removed, planters imported blacks in large numbers, giving lowland Georgia counties considerable black majorities.³¹

Direct importation of slaves from Africa provided the impetus to the growth

³⁰ Wood, *Black Majority: Negroes in Colonial South Carolina*, 99–103, 157, 159.

³¹ Peter H. Wood, "More like a Negro Country": Demographic Patterns in Colonial South Carolina, 1670–1740," in Stanley L. Engerman and Eugene D. Genovese, eds., *Race and Slavery in the Western Hemisphere: Quantitative Studies* (Princeton, 1975), 131–45; Julian J. Petty, *The Growth and Distribution of Population in South Carolina* (Columbia, S.C., 1943), 15–58, 220–27; Bureau of the Census, *Historical Statistics of the United States*, 756; and *Returns of the Whole Number of Persons within the . . . United States [1790]* (Philadelphia, 1791).

of the black majority. Some West Indian Afro-Americans continued to enter the lowcountry, but they shrank to a small fraction of the whole.³² As African importation increased, Charles Town took its place as the largest mainland slave mart and the center of the lowland slave trade. Almost all of the slaves in Carolina and later in Georgia—indeed, fully 40 percent of all pre-Revolutionary black arrivals in mainland North America—entered at Charles Town. The enormous number of slaves allowed slave masters a wide range of choices. Lowcountry planters developed preferences far beyond the usual demands for healthy adult and adolescent males and concerned themselves with the regional and tribal origins of their purchases. Some planters may have based their choices on long experience and a considered understanding of the physical and social character of various African nations. But, for the most part, these preferences were shallow ethnic stereotypes. Coromantees revolted; Angolans ran away; Iboes destroyed themselves. At other times, lowland planters apparently preferred just those slaves they did not get, perhaps because all Africans made unsatisfactory slaves and the unobtainable ones looked better at a distance. Although lowcountry slave masters desired Gambian people above all others, Angolans composed a far larger proportion of the African arrivals. But, however confused or mistaken in their beliefs, planters held them firmly and, in some measure, put them into practice. "Gold Coast and Gambia's are the best, next to them the Windward Coast are prefer'd to Angola's," observed a Charles Town merchant in describing the most salable mixture. "There must not be a Callabar amongst them."³³ Planter preferences informed lowcountry slave traders and, to a considerable degree, determined the tribal origins of lowland blacks.

Whatever their origins, rice cultivation shaped the destiny of African people arriving at Charles Town. Although the production of pitch and tar played a pivotal role in the early development of the staple-based economy in South Carolina, rice quickly became the dominant plantation crop. Rice cultivation evolved slowly during the late seventeenth and early eighteenth centuries as planters, aided by knowledgeable blacks, mastered the complex techniques necessary for commercial production. During the first half of the eighteenth century, rice culture was limited to the inland swamps, where slave-built dikes controlled the irrigation of low-lying rice fields. But by mid-century planters had discovered how to regulate the tidal floods to irrigate and drain their fields. Rice production moved to the tidal swamps that lined the region's many rivers and expanded greatly. By the beginning of the nineteenth century, the rice coast stretched from Cape Fear in North Carolina to the Satilla River in Georgia.³⁴ Throughout the lowcountry, rice was king.

³² W. Robert Higgins, "Charleston: Terminus and Entrepôt of the Colonial Slave Trade," in Martin L. Kilson and Robert I. Rotberg, eds., *The African Diaspora* (Cambridge, Mass., 1976), 115.

³³ Wood, *Black Majority: Negroes in Colonial South Carolina*, xiv, and "More like a Negro Country," 149-54; Higgins, "Charleston: Terminus and Entrepôt of the Colonial Slave Trade," 118-27; Wax, "Preferences for Slaves in Colonial America," 388-99; Curtin, *The Atlantic Slave Trade*, 143, 156-57; and Henry Laurens, *The Papers of Henry Laurens*, ed. Philip M. Hamer, George C. Rogers, Jr., and David R. Chesnutt, 7 vols. (Columbia, S.C., 1970-), 1: 294-95. For a continuing discussion of slave preferences in the lowcountry, see Laurens, *Papers of Henry Laurens*, esp. vols. 1-3.

³⁴ Clowse, *Economic Beginnings of Colonial South Carolina*, 122-33, 167-71, 220-21, 231-35, 256-58; Wood, *Black*

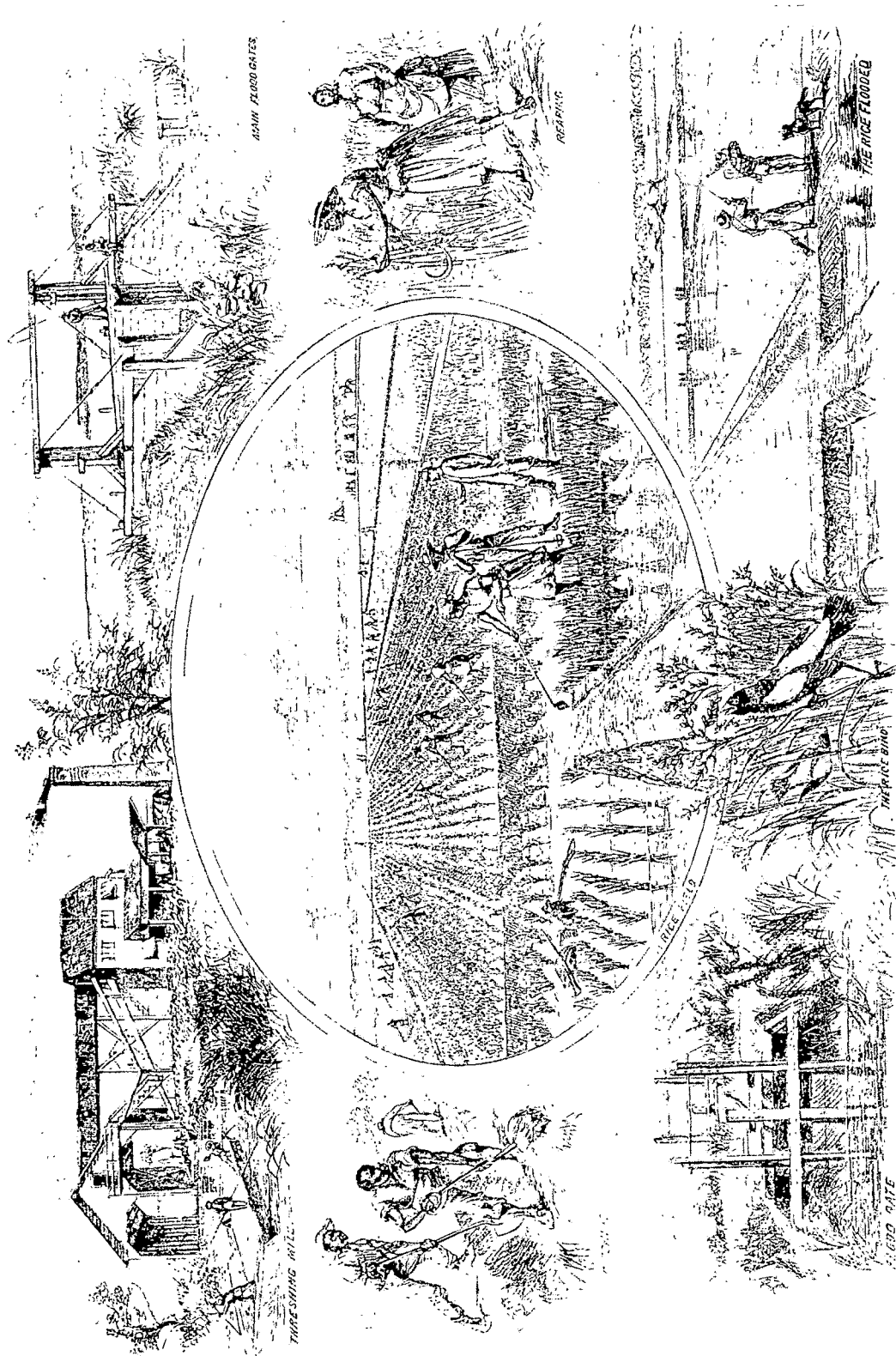


Figure 1: Mid-nineteenth-century representation of the processes of rice production. With the exception of the steam mill in the upper left-hand corner, the processes depicted are traditional. *Illustration from The Illustrated London News, January 5, 1867.*

The relatively mild slave regime of the pioneer years disappeared as rice cultivation expanded. Slaves increasingly lived in large units, and they worked in field gangs rather than at a variety of tasks. The strict requirements of rice production set the course of their work. And rice was a hard master. For a large portion of the year, slaves labored knee deep in brackish muck under the hot tropical sun; and, even after the fields were drained, the crops laid-by, and the grain threshed, there were canals to clear and dams to repair. By mid-century planters had also begun to grow indigo on the upland sections of their estates. Indigo complemented rice in its seasonal requirements, and it made even heavier labor demands.³⁵ The ready availability of African imports compounded the new harsh realities of plantation slavery by cheapening black life in the eyes of many masters. As long as the slave trade remained open, they skimmed on food, clothing, and medical attention for their slaves, knowing full well that substitutes could be easily had. With the planters' reliance on male African imports, slaves found it increasingly difficult to establish and maintain a normal family life. Brutal working conditions, the disease-ridden, lowland environment, and the open slave trade made for a deadly combination. Slave birth rates fell steadily during the middle years of the eighteenth century and mortality rates rose sharply. Between 1730 and 1760, deaths outnumbered births among blacks and only African importation allowed for continued population growth. Not until the eve of the Revolution did the black population begin again to reproduce naturally.³⁶

As the lowcountry plantation system took shape, the great slave masters retreated to the cities of the region; their evacuation of the countryside was but another manifestation of the growing social and cultural distance between them and their slaves. The streets of Charles Town, and, later, of Beaufort, Georgetown, Savannah, Darien, and Wilmington sprouted great new mansions as planters fled the malarial lowlands and the black majority. By the 1740s, urban life in the lowcountry had become attractive enough that men who made their fortunes in rice and slaves no longer returned home to England in the West Indian tradition. Instead, through intermarriage and business connections, they began to weave their disparate social relations into a close-knit ruling class, whose self-consciousness and pride of place became legendary. Charles Town, as the capital of this new elite, grew rapidly. Between 1720 and 1740 its population

Majority: *Negroes in Colonial South Carolina*, 35-62; Lewis C. Gray, *History of Agriculture in the Southern United States to 1860*, 2 vols. (Washington, 1933), 1: 277-89; James M. Clifton, "Golden Grains of White: Rice Planting on the Lower Cape Fear," *North Carolina Historical Review*, 50 (1973): 368-78; Douglas C. Wilms, "The Development of Rice Culture in 18th-Century Georgia," *Southeastern Geographer*, 12 (1972): 45-57; and Coon, "Market Agriculture in South Carolina," 126-27, 168-69, 178-86, 215-68. For the importance of naval stores in the transformation, see Ver Steeg, *Origins of a Southern Mosaic*, 117-32.

³⁵ For excellent descriptions of the process of rice growing and its changing technology, see David Doar, *Rice and Rice Planting in the Carolina Low Country* (Charleston, S.C., 1936), 7-41; and Gray, *Agriculture in the Southern United States*, 1: 290-97.

³⁶ Wood, "More like a Negro Country," 153-64; and Philip D. Morgan, "Afro-American Cultural Change: The Case of Colonial South Carolina Slaves," paper presented at the annual meeting of the Organization of American Historians, held in New Orleans, April 1979, 3-6, esp. tables 1, 4, 7. In the 1760s, as blacks began to increase naturally, slaveholders began to show some concern for their slaves' family life; see Laurens, *Papers of Henry Laurens*, 4: 595-96, 625, 5: 370.

doubled, and it nearly doubled again by the eve of the Revolution to stand at about twelve thousand. With its many fine houses, its great churches, its shops packed with luxury goods, Charles Town's prosperity bespoke the maturation of the lowland plantation system and the rise of the planter class.³⁷

Planters, ensconced in their new urban mansions, their pockets lined with the riches rice produced, ruled their lowcountry domains through a long chain of command: stewards located in the smaller rice ports, overseers stationed near or on their plantations, and plantation-based black drivers. But their removal from the plantation did not breed the callous indifference of West Indian absenteeism. For one thing, they were no more than a day's boat ride away from their estates. Generally, they resided on their plantations during the non-malarial season. Their physical removal from the direct supervision of slave labor and the leisure their urban residences afforded appear to have sharpened their concern for "their people" and bred a paternalist ideology that at once legitimated their rule and informed all social relations.³⁸

The lowcountry plantation system with its urban centers, its black majority, its dependence on "salt-water" slaves transformed black culture and society just as it reshaped the white world. The unified Afro-American culture and society that had evolved during the pioneer years disappeared as rice cultivation spread. In its place a sharp division developed between an increasingly urban creole and a plantation-based African population. The growth of plantation slavery not only set blacks further apart from whites, it also sharply divided blacks.

One branch of black society took shape within the bounds of the region's cities and towns. If planters lived removed from most slaves, they maintained close, intimate relations with some. The masters' great wealth, transient life, and seasonal urban residence placed them in close contact with house servants who kept their estates, boatmen who carried messages and supplies back and forth to their plantations, and urban artisans who made city life not only possible but comfortable. In addition, coastal cities needed large numbers of workers to transport and process the plantation staples, to serve the hundreds of ships that annually visited the lowcountry, and to satisfy the planters' newly acquired taste for luxury goods. Blacks did most of this work. Throughout the eighteenth century they composed more than half the population of Charles Town and other lowcountry ports. Probably nothing arrived or left these cities without some black handling it. Black artisans also played a large role in urban life. Master craftsmen employed them in every variety of work. A visitor to Charles Town

³⁷ George C. Rogers, Jr., *Charleston in the Age of the Pinckneys* (Norman, Okla., 1969); Carl Bridenbaugh, *Myths and Realities* (Baton Rouge, 1952), 59–60, 76–94, and *Cities in Revolt*, 216; and Frederick P. Bowes, *The Culture of Early Charleston* (Chapel Hill, N.C., 1942).

³⁸ Eugene D. Genovese has not made either regional or temporal distinctions in the development of Southern ideology but has leaned heavily on South Carolina for his understanding of Southern paternalism; see his *Roll, Jordan, Roll*, 1–113. For the interplay of quasi-absenteeism and planter ideology in the nineteenth century, see William W. Freehling, *Prelude to Civil War: The Nullification Controversy in South Carolina, 1813–1836* (New York, 1966), 65–70; and Michael P. Johnson, "Planters and Patriarchy: A Family History of Planter Ideology, Charleston, South Carolina," *Journal of Southern History* (forthcoming). The degree of absenteeism and its effect on social relations between planters and slaves has yet to be explored.

found that even barbers "are supported in idleness & ease by their negroes . . . ; & in fact many of the mechaniks bear nothing more of their trade than the name." Although most black artisans labored along the waterfront as shipwrights, ropemakers, and coopers, lowcountry blacks—unlike blacks in Northern cities—also entered the higher trades, working as gold beaters, silversmiths, and cabinetmakers. In addition, black women gained control over much of the marketing in the lowcountry ports, mediating between slave-grown produce in the countryside and urban consumption. White tradesmen and journeymen periodically protested against slave competition, but planters, master craftsmen, and urban consumers who benefited from black labor and services easily brushed aside these objections.³⁹

Mobile, often skilled, and occasionally literate, urban slaves understood the white world. They used their knowledge to improve their position within lowcountry society even while the condition of the mass of black people deteriorated in the wake of the rice revolution. Many urban creoles not only retained the independence of the earlier years but enlarged upon it. They hired their own time, earned wages from "overwork," kept market stalls, and sometimes even opened shops. Some lived apart from their masters and rented houses of their own, paying their owners a portion of their earnings in return for *de facto* freedom. Such liberty enabled a few black people to keep their families intact and perhaps even accumulate property for themselves. The small black communities that developed below the Bluff in Savannah and in Charles Town's Neck confirm the growing independence of urban creoles.⁴⁰

The incongruous prosperity of urban bondsmen jarred whites. By hiring their own time, living apart from their masters, and controlling their own family life, these blacks forcibly and visibly claimed the white man's privileges. Perhaps no aspect of their behavior was as obvious and, hence, as galling as their elaborate dress. While plantation slaves—men and women—worked stripped to the waist wearing no more than loin cloths (thereby confirming the white man's image of savagery), urban slaves appropriated their masters' taste for fine clothes and often the clothes themselves. Lowcountry legislators enacted various sumptuary regulations to restrain the slaves' penchant for dressing above their station. The

³⁹ Joseph W. Barnwell, ed., "The Diary of Timothy Ford," *South Carolina Historical Magazine*, 13 (1914): 142; Alexander Hewatt, *An Historical Account of the Rise and Progress of the Colonies of South Carolina and Georgia*, 2 (London, 1779): 97; Alan Candler, ed., *The Colonial Records of the State of Georgia*, 18 (Atlanta, 1912): 277–82; Charles S. Henry, comp., *A Digest of All the Ordinances of Savannah* (Savannah, Ga., 1854), 94–97; Petition from Charleston Carpenters and Bricklayers, 1783, and Petition from Charleston Coopers, 1793, Legislative Papers, South Carolina Department of Archives and History, Columbia; Cooper and McCord, *Statutes at Large of South Carolina*, 2: 22–23, 7: 385–87, 9: 692–97; Donald R. Lennon and Ida B. Kellam, eds., *The Wilmington Town Book, 1743–1778* (Raleigh, N.C., 1973), 165–66; Petition from Newberne, 1785, North Carolina Legislative Papers, North Carolina State Archives, Raleigh; Carl Bridenbaugh, *Colonial Craftsmen* (New York, 1950) 139–41, and *Cities in Revolt*, 88–89, 244, 274, 285–86; Leila Sellers, *Charleston Business on the Eve of the American Revolution* (Chapel Hill, N.C., 1934), 99–108; Duncan, "Servitude and Slavery in Colonial South Carolina," 439–46; and Kenneth Coleman, *Colonial Georgia, A History* (New York, 1976), 229–30.

⁴⁰ Candler, *Colonial Records of the State of Georgia*, 23–30, 252–62; Henry, *Ordinances of Savannah*, 95–97; Alexander Edwards, comp., *Ordinances of the City Council of Charleston* (Charleston, S.C., 1802), 65–68; Cooper and McCord, *Statutes at Large of South Carolina*, 7: 363, 380–81, 393; Lennon and Kellam, *The Wilmington Town Book*, xxx–xxxi, 165–68, 204–05; Duncan, "Servitude and Slavery in Colonial South Carolina," 467–69, 481–84; and Sellers, *Charleston Business on the Eve of the American Revolution*, 99–102, 106–08.

South Carolina Assembly once even considered prohibiting masters from giving their old clothes to their slaves. But hand-me-downs were clearly not the problem as long as slaves earned wages and had easy access to the urban marketplace. Frustrated by the realities of urban slavery, lawmakers passed and re-passed the old regulations to little effect. On the eve of the Revolution, a Charles Town Grand Jury continued to bemoan the fact that the "Law for preventing the excessive and costly Apparel of Negroes and other Slaves in this province (especially in *Charles Town*) [was] not being put into Force."⁴¹

Most of these privileged bondsmen appear to have been creoles with long experience in the New World. Although some Africans entered urban society, the language skills and the mastery of the complex interpersonal relations needed in the cities gave creoles a clear advantage over Africans in securing elevated positions within the growing urban enclaves. To be sure, their special status was far from "equal." No matter how essential their function or intimate their interaction, their relations with whites no longer smacked of the earlier "sawbuck equality." Instead, these relations might better be characterized as paternal, sometimes literally so.

Increasingly during the eighteenth century, blacks gained privileged positions within lowcountry society as a result of intimate, usually sexual, relations with white slave masters. Like slaveholders everywhere, lowland planters assumed that sexual access to slave women was simply another of the master's prerogatives. Perhaps because their origin was West Indian or perhaps because their dual residence separated them from their white wives part of the year, white men established sexual liaisons with black women frequently and openly. Some white men and black women formed stable, long-lasting unions, legitimate in everything but law. More often than other slaveholders on continental British North America, lowcountry planters recognized and provided for their mulatto offspring, and, occasionally, extended legal freedom. South Carolina's small free Negro population, almost totally confined to Charles Town, was largely the product of such relations. Light-skinned people of color enjoyed special standing in the lowcountry ports, as they did in the West Indies, and whites occasionally looked the other way when such creoles passed into the dominant caste. But even when the planters did not grant legal freedom, they usually assured the elevated standing of their mulatto scions by training them for artisan trades or placing them in household positions. If the countryside was "blackened" by African imports, Charles Town and the other lowcountry ports exhibited a *mélange* of "colored" peoples.⁴²

While one branch of black society stood so close to whites that its members

⁴¹ *South Carolina Gazette*, May 24, 1773, as quoted in Duncan, "Servitude and Slavery in Colonial South Carolina," 234; Leowald, *et al.*, "Bolzius Answers a Questionnaire on Carolina and Georgia," 236; Cooper and McCord, *Statutes at Large of South Carolina*, 7: 396-412; and Duncan, "Servitude and Slavery in Colonial South Carolina," 233-37.

⁴² Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro* (Chapel Hill, N.C., 1968), 144-50, 167-78, and "American Chiaroscuro: The Status and Definition of Mulattoes in the British Colonies," 186-200; Wood, *Black Majority: Negroes in Colonial South Carolina*, 100-03; and General Tax, Receipts and Payments, 1761-69, Records of the Public Treasurers of South Carolina, South Carolina Department of Archives and History, Columbia (I am grateful to Peter H. Wood for telling me about these records). A sample of manumissions taken from the South Carolina records between 1729 and 1776 indicates that two-thirds of the slaves

sometimes disappeared into the white population, most plantation slaves remained alienated from the world of their masters, physically and culturally. Living in large units often numbering in the hundreds on plantations that they had carved out of the malarial swamps and working under the direction of black drivers, the black majority gained only fleeting knowledge of Anglo-American culture. What they knew did not encourage them to learn more. Instead, they strove to widen the distance between themselves and their captors. In doing so, they too built upon the large degree of autonomy black people had earlier enjoyed.

In the pioneer period, many masters required slaves to raise their own provisions. Slaves regularly kept small gardens and tended barnyard fowl to maintain themselves, and they often marketed their surplus. Blacks kept these prerogatives with the development of the plantation system. In fact, the growth of lowcountry towns, the increasing specialization in staple production, and the comparative absence of nonslaveholding whites enlarged the market for slave-grown produce. Planters, of course, disliked the independence truck gardening afforded plantation blacks and the tendency of slaves to confuse their owners' produce with their own, but the ease of water transportation and the absence of white supervision made it difficult to prevent.

To keep their slaves on the plantation, some planters traded directly with their bondsmen, bartering manufactured goods for slave produce. Henry Laurens, a planter who described himself as a "factor" for his slaves, exchanged some "very gay Wastcoats which some of the Negro Men may want" for grain at "10 Bushels per Wastcoat." Later, learning that a plantation under his supervision was short of provisions, he authorized the overseer "to purchase of your own Negroes all that you know Lawfully belongs to themselves at the lowest price they will sell it for." As Laurens's notation suggests, planters found benefits in slave participation in the lowcountry's internal economy, but the small profits gained by bartering with their bondsmen only strengthened the slaves' customary right to their garden and barnyard fowl. Early in the nineteenth century, when Charles C. Pinckney decided to produce his own provisions, he purchased breeding stock from his slaves. By the Civil War, lowland slaves controlled considerable personal property—flocks of ducks, pigs, milch cows, and occasionally horses—often the product of stock that had been in their families for generations.⁴³ For the most part, slave propertyholding remained small during the eighteenth century. But it helped insulate plantation blacks from the harsh con-

freed were female and one-third of the slaves freed were mulattoes at a time when the slave population of South Carolina was disproportionately male and black; Duncan, "Servitude and Slavery in Colonial South Carolina," 395-98.

⁴³ Laurens, *Papers of Henry Laurens*, 4: 616, 5: 20, 41; C. C. Pinckney, *Plantation Journal*, 1812, and George Lucas to Charles Pinckney, January 30, 1745/46, Manuscript Division, Library of Congress, Washington, D.C.; Entries in Memo Book "per self" and "Negro Esquire per self," Cameron Family Papers, Southern Historical Collection, University of North Carolina, Chapel Hill; Charles Town Grand Jury Presentment, January 1772, South Carolina Department of Archives and History; and Depositions from Liberty County, Georgia, Southern Claims Commission, Third Auditor, General Accounting Office, RG 217, National Archives, Washington, D.C. A similar division of labor between master and slave has been found in various nineteenth-century African slave societies. Whether these similar patterns have a common root or are the product of independent development is a subject for future research. See Paul O. Lovejoy, "The Characteristics of Plantations in the Nineteenth-Century Sokoto Caliphate (Islamic West Africa)," *AHR*, 84 (1979): 1283-84. Also see footnote 19, above.

ditions of primitive rice production and provided social distance from their masters' domination.

The task system, a mode of work organization peculiar to the lowcountry, further strengthened black autonomy. Under the task system, a slave's daily routine was sharply defined: so many rows of rice to be sowed, so much grain to be threshed, or so many lines of canal to be cleared. Such a precise definition of work suggests that city-bound planters found it almost impossible to keep their slaves in the fields from sunup to sundown. With little direct white supervision, slaves and their black foremen conspired to preserve a large portion of the day for their own use, while meeting their masters' minimum work requirements. Struggle over the definition of a task doubtless continued throughout the formative years of the lowcountry plantation system and after, but by the end of the century certain lines had been drawn. Slaves generally left the field sometime in the early afternoon, a practice that protected them from the harsh afternoon sun and allowed them time to tend their own gardens and stock.⁴⁴ Like participation in the lowcountry's internal economy, the task system provided slaves with a large measure of control over their own lives.

The autonomy generated by both the task system and truck gardening provided the material basis for lowland black culture. Within the confines of the overwhelmingly black countryside, African culture survived well. The continual arrival of Africans into the lowcountry renewed and refreshed slave knowledge of West African life. In such a setting blacks could hardly lose their past. The distinctive pattern of the lowland slave trade, moreover, heightened the impact of the newly arrived Africans on the evolution of black culture. While slaves dribbled into the North through a multiplicity of ports, they poured into the lowcountry through a single city. The large, uncentered slave trade and the large slaveholding units assured the survival not only of the common denominators of West African culture but also many of its particular tribal and national forms. Planter preferences or perhaps the chance ascendancy of one group sometimes allowed specific African cultures to reconstitute themselves within the plantation setting. To be sure, Africans changed in the lowcountry. Even where blacks enjoyed numerical superiority and a considerable degree of auton-

⁴⁴ By the middle of the nineteenth century, the work required under the task system had been carefully defined. Indeed, for many lowcountry crops, the task had become so standardized that it was often used interchangeably as a unit of land (the amount necessary to grow a task of peas) or even a unit of time (the amount of time it took to plant a task of peas). Nevertheless, the struggle over the definition of the task did not end. Following emancipation, when planters attempted to eliminate the task system, freed people objected, often violently. In 1865, a Union soldier reported from Georgetown that the freedmen "have been accustomed to working by task, which has always given them leisure to cultivate land for themselves, tend their stock, and amuse themselves, and, therefore very correctly, I think, [believe] that with such a change in the march of labor all their privileges will go and their condition will be less to their taste than it was when they were slaves." Lt. Col. A. J. Willard to Capt. George H. Hooker, Georgetown, November 7, 1865, Letters Sent, vol. 156 DS, U.S. Army Commands, RG 393, pt. 2, National Archives, Washington, D.C. Also see Ulrich B. Phillips, ed., *Plantation and Frontier*, 1 (Cleveland, 1909): 115-19; and Frederick Law Olmsted, *The Cotton Kingdom*, ed. Arthur M. Schlesinger, 1 (New York, 1953): 190-94. The origins of the task system and the struggle over the definition of work in the eighteenth century has not yet been investigated, but, for the kinds of disputes that defined the measure of a task, see Josiah Smith to George Austin, July 22, 1773, Josiah Smith Letterbook, Southern Historical Collection, and Richard Hutson to Mr. Croll, "per Caser," August 22, 1767, Charles W. Hutson Papers, Southern Historical Collection, University of North Carolina, Chapel Hill.

omy, they could no more transport their culture unchanged than could their masters. But lowcountry blacks incorporated more of West African culture—as reflected in their language, religion, work patterns, and much else—into their new lives than did other black Americans. Throughout the eighteenth century and into the nineteenth, lowcountry blacks continued to work the land, name their children, and communicate through word and song in a manner that openly combined African traditions with the circumstances of plantation life.⁴⁵

The new pattern of creolization that developed following the rice revolution smashed the emerging homogeneity of black life in the first years of settlement and left lowcountry blacks deeply divided. One branch of black culture evolved in close proximity to whites. Urban, often skilled, well-traveled, and increasingly American-born, creoles knew white society well, and they used their knowledge to better themselves. Some, clearly a well-connected minority, pressed for incorporation into the white world. They urged missionary groups to admit their children to school and later petitioned lawmakers to allow their testimony in court, carefully adding that they did not expect full equality with whites.⁴⁶ Plantation slaves shared few of the assimilationist aspirations of urban creoles. By their dress, language, and work routine, they lived in a world apart. Rather than demand incorporation into white society, they yearned only to be left alone. Within the quarter, aided by their numerical dominance, their plantation-based social hierarchy, and their continued contact with Africa, they developed their own distinctive culture, different not only from that of whites but also from the cosmopolitan world of their Afro-American brethren. To be sure, there were connections between the black majority and the urban creoles. Many—market women, jobbing artisans, and boatmen—moved easily between these two worlds, and most blacks undoubtedly learned something of the other world through chance encounters, occasional visits, and word of mouth.⁴⁷ Common white oppression continually shrank the social distance that the distinctive experience created, but by the eve of the Revolution, deep cultural differences separated those blacks who sought to improve their lives through incorporation into the white world and those who determined to disregard the white man's ways. If the movement from African to creole obliterated cultural differences among Northern blacks, creolization fractured black society in the lowcountry.

CULTURAL DISTINCTIONS BETWEEN AFRICANS AND AFRO-AMERICANS developed in the Chesapeake as well, although the dimension of differences between African and creole tended to be time rather than space. Unlike in the lowcountry, white

⁴⁵ Wood, *Black Majority: Negroes in Colonial South Carolina*, esp. chap. 6; Lorenzo D. Turner, *Africanisms in the Gullah Dialect* (Chicago, 1949); William R. Bascom, "Acculturation among the Gullah Negroes," *American Anthropologist*, 43 (1941): 43–50; Klingberg, *An Appraisal of the Negro in South Carolina*; and Hennig Cohen, "Slave Names in Colonial South Carolina," *American Speech*, 28 (1952): 102–07.

⁴⁶ Klingberg, *An Appraisal of the Negro in Colonial South Carolina*, 116–17; and Petition of John and William Morriss, 1791, and Petition from Camden Negroes, 1793, South Carolina Legislative Papers, South Carolina Department of Archives and History, Columbia.

⁴⁷ For one planter's attempt to keep boatmen from mixing with his plantation hands, see Laurens, *Papers of Henry Laurens*, 4: 319, 633; and Sellers, *Charleston Business on the Eve of the American Revolution*, 108.

planters did not promote the creation of a distinctive group whose origins, function, and physical appearance distinguished them from the mass of plantation slaves and offered them hope, however faint, of eventual incorporation into white society. And, compared to the North, African immigration into the Chesapeake came relatively early in the process of cultural transformation. As a result, African-creole differences disappeared with time and a single, unified Afro-American culture slowly emerged in the Chesapeake.

As in the lowcountry, little distinguished black and white laborers during the early years of settlement. Most of the first blacks brought into the Chesapeake region were West Indian creoles who bore English or Spanish surnames and carried records of baptism. Along the James, as along the Cooper, the demands of pioneer life at times operated to strengthen the slaves' bargaining position. Some blacks set the condition of their labor, secured their family life, participated in the region's internal economy, and occasionally bartered for their liberty. This, of course, did not save most black people from the brutal exploitation that almost all propertyless men and women faced as planters squeezed the last pound of profit from the tobacco economy. The blacks' treatment at the hands of planters differed little from that of white bound labor in large measure because it was difficult to treat people more brutally.⁴⁸ While the advantages of this peculiar brand of equality may have been lost on its beneficiaries, those blacks who were able to complete their terms of servitude quickly joined whites in the mad scramble for land, servants, and status.

Many did well. During the seventeenth century, black freemen could be found throughout the region owning land, holding servants, and occasionally attaining minor offices. Like whites, they accumulated property, sued their neighbors, and passed their estates to their children. In 1651, Anthony Johnson, the best known of these early Negro freemen, received a two-hundred-and-fifty-acre headright for importing five persons into Virginia. John Johnson, a neighbor and probably a relative, did even better, earning five hundred and fifty acres for bringing eleven persons into the colony. Both men owned substantial farms on the Eastern Shore, held servants, and left their heirs sizable estates. As established members of their communities, they enjoyed the rights of citizens. When a servant claiming his freedom fled Anthony Johnson's plantation and took refuge with a nearby white farmer, Johnson took his neighbor to court and won the return of his servant along with damages against the white man.⁴⁹

The class rather than racial basis of early Chesapeake society enabled many black men to compete successfully for that scarcest of all New World commodities: the affection of white women. Bastardy lists indicate that white female servants ignored the strictures against what white lawmakers labeled "shame-

⁴⁸ Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 108-79, 215-49; and Wesley Frank Craven, *White, Red, and Black: The Seventeenth-Century Virginian* (Charlottesville, Va., 1971), 75-99.

⁴⁹ Ross M. Kimmel, "Free Blacks in Seventeenth-Century Maryland," *Maryland Historical Magazine*, 71 (1976): 19-25; John H. Russell, *The Free Negro in Virginia, 1619-1865* (Baltimore, 1913), 24-38, 88, 116, 119-20, 136-37; James H. Brewer, "Negro Property Owners in Seventeenth-Century Virginia," *William and Mary Quarterly*, 3d ser., 12 (1955): 575-80; and Susie M. Ames, *Studies of the Virginia Eastern Shore in the Seventeenth Century* (Richmond, Va., 1940), 99-108.

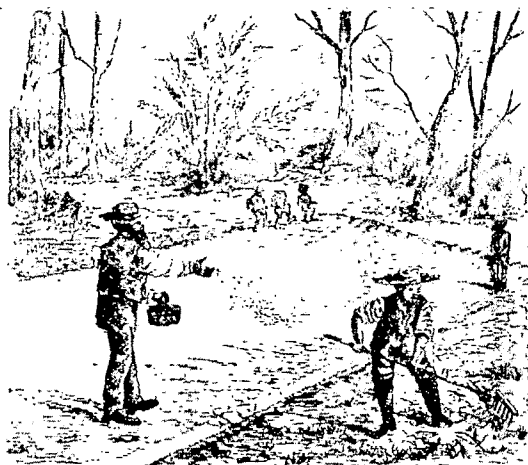
ful" and "unnatural" acts and joined together with men of their own condition regardless of color. Fragmentary evidence from various parts of seventeenth-century Virginia reveals that approximately one-quarter to one-third of the bastard children born to white women were mulattoes. The commonplace nature of these interracial unions might have been the reason why one justice legally sanctified the marriage of Hester, an English servant woman, to James Tate, a black slave. Some successful, property-owning whites and blacks also intermarried. In Virginia's Northampton county, Francis Payne, a Negro freeman, married a white woman, who later remarried a white man after Payne's death. William Greensted, a white attorney who represented Elizabeth Key, a mulatto woman, in her successful suit for her freedom, later married her. In 1691, when the Virginia General Assembly finally ruled against the practice, some propertied whites found the legislation novel and obnoxious enough to muster a protest.⁵⁰

By the middle of the seventeenth century, Negro freemen sharing and fulfilling the same ideals and aspirations that whites held were no anomaly in the Chesapeake region. An Eastern Shore tax list of 1668 counted nearly a third of black tithables free. If most blacks did not escape the tightening noose of enslavement, they continued to live and work under conditions not much different from white servants. Throughout the seventeenth and into the first decades of the eighteenth century, black and white servants ran away together, slept together, and, upon occasion, stood shoulder to shoulder against the weighty champions of established authority. Thus viewed from the first years of settlement—the relatively small number of blacks, their creole origins, and the initial success of some in establishing a place in society—black acculturation in the Chesapeake appeared to be following the nonplantation pattern of the Northern colonies and the pioneer lowcountry.⁵¹

THE emergence of a planter class and its consolidation of power during a series of political crises in the middle years of the seventeenth century transformed black life in the Chesapeake and threatened this pattern of cultural change. Following the legalization of slavery in the 1660s, black slaves slowly but steadily replaced white indentured servants as the main source of plantation labor. By 1700, blacks made up more than half the agricultural work force in Virginia and, since the great planters could best afford to purchase slaves, blacks composed an even larger share of the workers on the largest estates. Increased reliance on slave labor quickly outstripped West Indian supplies. Beginning in the

⁵⁰ Morgan, *American Slavery, American Freedom*, 329–37; Warren M. Billings, "The Cases of Fernando and Elizabeth Key: A Note on the Status of Blacks in the Seventeenth Century," *William and Mary Quarterly*, 3d ser., 30 (1973): 467–74; and Kimmel, "Free Blacks in Seventeenth-Century Maryland," 20–21.

⁵¹ Edmund S. Morgan, "Slavery and Freedom: The American Paradox," *Journal of American History*, 59 (1972): 17–18; and T. H. Breen, "A Changing Labor Force and Race Relations in Virginia, 1660–1710," *Journal of Social History*, 7 (1973): 3–25. The confused, uncertain status of black people generally and of free blacks in particular during the seventeenth century also indicates the unwillingness, inability, or, more probably, lack of interest on the part of whites in firmly fixing the status of blacks. For the farrago of legislation governing free blacks, see Ira Berlin, *Slaves without Masters: The Free Negro in the Antebellum South* (New York, 1974), 7–9; and Jordan, *White over Black*, 136–78. The status of blacks, free or slave, has become something of a historical perennial, with scholars agreeing that before the 1660s at least some blacks were free and some were slave and the precise status of most is simply impossible to determine. For a review of the evidence, see Jordan, *White over Black*, chap. 2.



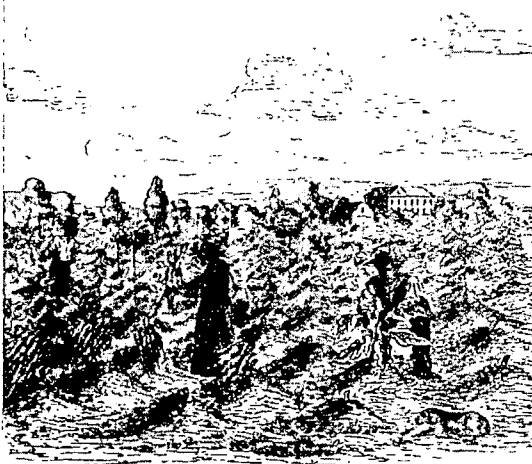
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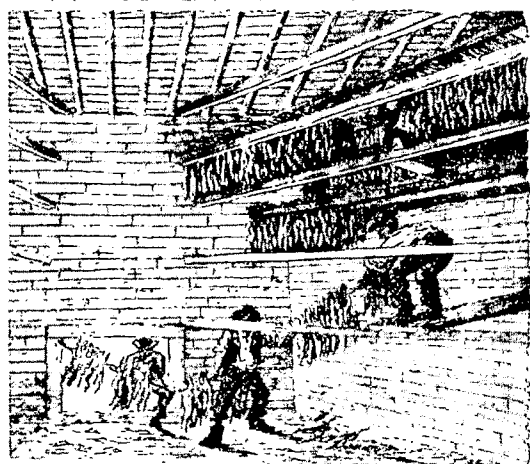
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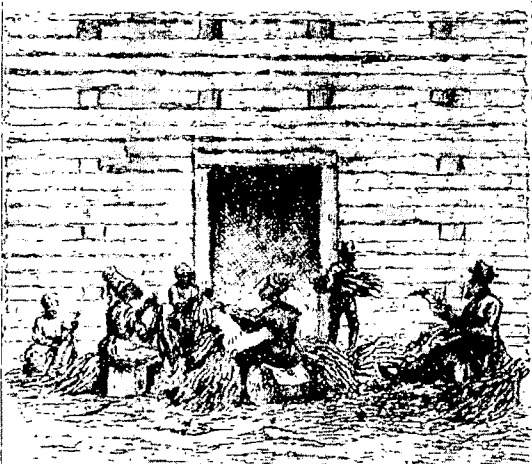
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Figure 2: Late nineteenth-century representation of the stages of prerevolutionary tobacco production. 1: Sowing and hilling. 2: Transplanting. 3: Laying by, topping, worming, and suckering. 4: Cutting and sticking. 5: Housing. 6: Stripping and tying. Photograph taken from Benjamin Butterworth, *The Growth of Industrial Art* (Washington, 1892), 192.

1680s, Africans entered the region in increasingly large numbers. The proportion of blacks born in Africa grew steadily throughout the waning years of the seventeenth century, so that by the first decade of the eighteenth century, Africans composed some three-quarters of the region's blacks.⁵² Unlike the low-country, African imports never threatened the Chesapeake's overall white numerical superiority, but by the beginning of the eighteenth century they dominated black society. Some eighty years after the first blacks arrived at Jamestown and some forty years after the legalization of slavery, African importation profoundly transformed black life.

Slave conditions deteriorated as their numbers increased. With an eye for a quick profit, planters in the Chesapeake imported males disproportionately. Generally men outnumbered women more than two to one on Chesapeake slavers. Wildly imbalanced sex ratios undermined black family life. Physically spent and emotionally drained by the rigors of the Middle Passage, African women had few children. Thus, as in the North and the Carolina lowlands, the black birth rate fell and mortality rate surged upward with the commencement of direct African importation.⁵³

The hard facts of life and death in the Chesapeake region distinguished creoles and Africans at the beginning of the eighteenth century. The demands of the tobacco economy enlarged these differences in several ways. Generally, planters placed little trust in newly arrived Africans with their strange tongues and alien customs. While they assigned creoles to artisanal duties on their plantations and to service within their households, they sent Africans to the distant, upland quarters where the slaves did the dull, backbreaking work of clearing the land and tending tobacco. The small size of these specialized upcountry units, their isolation from the mainstream of Chesapeake life, and their rude frontier conditions made these largely male compounds lonely, unhealthy places that narrowed men's vision. The dynamics of creole life, however, broadened black understanding of life in the New World. Traveling freely through the countryside as artisans, watermen, and domestic servants, creoles gained in confidence as they mastered the terrain, perfected their English, and learned about Christianity and other cultural modes that whites equated with civilization. Knowledge of the white world enabled black creoles to manipulate their masters to their own advantage. If Afro-Americans became increasingly knowledgeable about their circumstances and confident of their ability to deal with them, Africans remained provincials, limited by the narrow alternatives of plantation life.⁵⁴

⁵² Allan Kulikoff, "A 'Prolifick' People: Black Population Growth in the Chesapeake Colonies, 1700-1790," *Southern Studies*, 16 (1977): 391-96, 403-05, and "The Origins of Afro-American Society in Tidewater Maryland and Virginia, 1700 to 1790," *William and Mary Quarterly*, 3d ser., 35 (1978): 229-31; Russell R. Menard, "The Maryland Slave Population, 1658 to 1730: A Demographic Profile of Blacks in Four Counties," *ibid.*, 32 (1975): 30-32; and Craven, *White, Red, and Black*, 89-103. Herbert S. Klein has maintained that West Indian re-exports remained the majority into the first two decades of the eighteenth century; see his "Slaves and Shipping in Eighteenth-Century Virginia," *Journal of Interdisciplinary History*, 5 (1975): 384-85.

⁵³ Kulikoff, "A 'Prolifick' People: Black Population Growth," 392-406; Menard, "The Maryland Slave Population," 30-35, 38-49; and Craven, *White, Red, and Black*, 98-101.

⁵⁴ Gerald W. Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia* (New York, 1972), esp. chaps. 2-3; Menard, "The Maryland Slave Population," 32-54; and Kulikoff, "Origins of Afro-American Society in Tidewater Maryland and Virginia," 236-49.

As in the lowcountry and the Northern colonies, Africans in the Chesapeake strove to escape whites, while creoles used their knowledge of white society for their own benefit. These cultural differences, which were reflected in all aspects of black life, can be seen most clearly in the diverse patterns of resistance. Africans ran away toward the back country and isolated swamps. They generally moved in groups that included women and children, despite the hazards such groups entailed for a successful escape. Their purpose was to recreate the only society they knew free from white domination. In 1727, Governor William Gooch of Virginia reported that about a dozen slaves had left a new plantation near the falls of the James River. They headed west and settled near Lexington, built houses, and planted a crop before being retaken. But Afro-Americans ran away alone, usually with the hope of escaping into American society. Moving toward the areas of heaviest settlement, they found refuge in the thick network of black kinship that covered the countryside and sold their labor to white yeomen with few questions asked. While the possibility of passing as free remained small in the years before the Revolution, the creoles' obvious confidence in their ability to integrate themselves into American society stands in stark contrast to that of Africans, who sought first to flee it.⁵⁵

As reflected in the mode of resistance, place of residence, occupation, and much else, Africans and creoles developed distinctive patterns of behavior and belief. To a degree, whites recognized these differences. They stigmatized Africans as "outlandish" and noted how creoles "affect our language, habits, and customs." They played on African-creole differences to divide blacks from each other, and they utilized creole skills to maximize the benefits of slave labor. But this recognition did not elevate creoles over Africans in any lasting way. Over the course of the century following legal enslavement, it had precisely the opposite effect. Chesapeake planters consolidated their class position by asserting white racial unity. In this context, the entry of large numbers of African—as opposed to creole—blacks into the region enlarged racial differences and helped secure planter domination. Thus, as reliance on black labor increased, the opportunities for any black—no matter how fluent in English or conversant with the countryside—to escape bondage and join the scramble for land, servants, and status diminished steadily.

By the middle of the eighteenth century, the size and character of the free Negro population had been significantly altered. Instead of a large minority of the black population, Negro freemen now composed just a small proportion of all blacks, probably not more than 5 percent. Many were cripples and old folks whom planters discarded when they could no longer wring a profit from their labor. While most were of mixed racial origins, few of these free mulattoes of the Chesapeake, in contrast to those of the lowcountry, traced their ancestry to the planter class. Instead, they descended from white servants, frequently women. These impoverished people had little status to offer their children. Indeed, planter-inspired legislation further compromised their liberty by requiring that

⁵⁵ Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia*, 34–110, esp. table 3 (pp. 108–09); and Kulikoff, "Origins of Afro-American Society in Tidewater Maryland and Virginia," 253–54.

the offspring of white women and black men serve their mother's master for thirty-one years. Those who survived the term could scarcely hope for the opportunities an earlier generation of Negro freemen had enjoyed.⁵⁶ The transformation of the free Negro caste in the century between 1660 and 1760 measured the change in Chesapeake society as its organizing principle changed from class to race.

The free Negro's decline reveals how the racial imperatives of Chesapeake society operated to lump all black people together, free and slave, creole and African. In the Chesapeake, planters dared not grant creoles special status at the expense of Africans. Since the Africans would shortly be creoles and since creoles shared so much with whites, distinctions among blacks threatened the racial division that underlay planter domination. In the lowcountry, where geography, economy, and language separated white and black, those few blacks who spoke, dressed, acted, and looked like whites might be allowed some white prerogatives. But, if lowcountry planters could argue that no white man could do the work required to grow rice commercially, no one in the Chesapeake could reasonably deny that whites could grow tobacco. The fundamental unity of Chesapeake life and the long-term instability of African-creole differences pushed blacks together in the white mind and in fact.

During the middle years of the eighteenth century, changes in the Chesapeake economy and society further diminished differences within black society and created a unified Afro-American culture. The success of the tobacco economy enlarged the area of settlement and allowed planters to increase their holdings. The most successful planters, anxious to protect themselves from the rigors of the world marketplace, strove for plantation self-sufficiency. The great estates of the Chesapeake became self-contained enterprises with slaves taking positions as artisans, tradesmen, wagoners, and, sometimes, managers; the plantation was "like a Town," as a tutor on Robert Carter's estate observed, "but most of the Inhabitants are black." The increased sophistication of the Chesapeake economy propelled many more blacks into artisanal positions and the larger units of production, tighter pattern of settlement, and the greater mobility allowed by the growing network of roads ended the deadening isolation of the upcountry quarter. Bondsmen increasingly lived in large groups, and those who did not could generally find black companionship within a few miles' walk. Finally, better food, clothing, and shelter and, perhaps, the development of immunities to New World diseases enabled blacks to live longer, healthier lives.⁵⁷

⁵⁶ Hugh Jones, *The Present State of Virginia* (1724), ed. Richard L. Morton (Chapel Hill, N.C., 1956), 75; Berlin, *Slaves without Masters*, 3-6; Donald L. Horowitz, "Color Differentiation in the American Systems of Slavery," *Journal of Interdisciplinary History*, 3 (1973): 526-30; and George M. Fredrickson, "Toward a Social Interpretation of the Development of American Racism," in Nathan I. Huggins et al., eds., *Key Issues in the Afro-American Experience*, 1 (New York, 1971): 246-47.

⁵⁷ Philip V. Fithian, *The Journal and Letters of Philip Vickers Fithian, 1773-1774*, ed. Hunter D. Farish (Williamsburg, Va., 1943), 73; Mullin, *Flight and Resistance: Slave Resistance in Eighteenth-Century Virginia*, 19-32; Kulikoff, "Origins of Afro-American Society in Tidewater Maryland and Virginia," 240-42, 246-49; Louis Morton, *Robert Carter of Nomini Hall: A Virginia Tobacco Planter of the Eighteenth Century* (Charlottesville, Va., 1941); Michael Greenberg, "William Byrd II and the World of the Market," *Southern Studies*, 16 (1977): 429-56; and, especially, Landon Carter, *The Diary of Colonel Landon Carter of Sabine Hall, 1752-1778*, ed. Jack P. Greene, 2 vols. (Charlottesville, Va., 1966), *passim*.

As part of their drive for self-sufficiency, Chesapeake slaveholders encouraged the development of an indigenous slave population. Spurred by the proven ability of Africans to survive and reproduce and pressed in the international slave market by the superior resources of West Indian sugar magnates and lowland rice growers, Chesapeake planters strove to correct the sexual imbalance within the black population, perhaps by importing a large proportion of women or lessening the burden of female slaves. Blacks quickly took advantage of this new circumstance and placed their family life on a firmer footing. Husbands and wives petitioned their owners to allow them to reside together on the same quarter and saw to it that their families were fed, beyond their masters' rations. Planters, for their part, were usually receptive to slaves' demands for a secure family life, both because it reflected their own values and because they profited mightily from the addition of slave children. Thomas Jefferson frankly considered "a woman who brings a child every two years as more profitable than the best man on the farm [for] what she produces is an addition to capital, while his labor disappears in mere consumption." Under these circumstances, the black population increased rapidly. Planters relied less and less on African importation and, by the 1740s, most of the growth of the black population came from natural increase. Within a generation, African importation was, for all practical purposes, no longer a significant source of slave labor. In the early 1770s, the period of the greatest importation into the lowcountry, only five hundred of the five thousand slaves added annually to the black population of Virginia derived directly from Africa.⁵⁸

The establishment of the black family marked the re-emergence of Afro-American culture in the Chesapeake. Although Africans continued to enter the region, albeit at a slower pace, the nature of the slave trade minimized their impact on the development of black society in the region. Unlike those in the lowcountry, newly arrived Africans could rarely hope to remain together. Rather than funnel their cargo through a single port, Chesapeake slavers peddled it in small lots at the many tobacco landings that lined the bay's extensive perimeter. Planters rarely bought more than a few slaves at a time, and larger purchasers, usually the great planter-merchants, often acted as jobbers, quickly reselling these slaves to backcountry freeholders.⁵⁹ The resulting fragmentation sent

⁵⁸ Allan Kulikoff, "The Beginnings of the Afro-American Family in Maryland," in Aubrey C. Land *et al.*, eds., *Law, Society, and Politics in Early Maryland* (Baltimore, 1977), 177-96, "A 'Prolifick' People: Black Population Growth," 401-03, 405-14, and "Origins of Afro-American Society in Tidewater Maryland and Virginia," 246-53; Daniel Dulany to Robert Carter, December 18, 1768, Colonial Papers, Maryland Historical Society, Baltimore; Robert Carter to John Pound, March 16, 1779, to Fleet Cox, January 2, 1788, and to George Newman, December 29, 1789, typescript, Robert Carter Papers, Duke University, Durham, N.C.; John C. Fitzpatrick, ed., *The Writings of George Washington*, 39 vols. (Washington, 1931-44), 2: 526, 29: 154, 398; and Edwin M. Betts, ed., *Thomas Jefferson's Farm Book* (New York, 1953), pt. 2: 46, 12-13, 21, 24-26, 42-46. Planters also found a relationship between family stability and social stability. A Maryland planter instructed his overseer about a returned fugitive: "While his wife continues at home, I suppose there will be no danger of his making a second attempt to get off. You may let him know, that his pardon depends upon his good future behavior, that if he behaves well, and endeavours to make amends for his past behavior I will when I return purchase his wife if her master will sell her at a reasonable price." Letter of John Hanson, January 29, 1782, John Hanson Papers, Maryland Historical Society, Baltimore.

⁵⁹ Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia*, 14-16; Kulikoff, "Origins of Afro-American Society in Tidewater Maryland and Virginia," 230-35; Darold D. Wax, "Black Immigrants: The

newly arrived Africans in all directions and prevented the maintenance of tribal or shipboard ties. Chesapeake slaveholders cared little about the origins of their slaves. In their eyes, newly arrived Africans were not Iboes, Coromantees, or Angolans, but "new Negroes." While the unicentered slave trade sustained and strengthened African culture in the lowcountry, the Chesapeake slave trade facilitated the absorption of Africans into the evolving creole society.

Differences between creoles and Africans did not disappear with the creation of a self-sustaining Afro-American population. The creoles' advantages—language skills, familiarity with the countryside, artisanal standing, and knowledge of the plantation routine—continued to propel them into positions of authority within the slave hierarchy. In some ways, the growing complexity of the Chesapeake economy widened the distance between Africans and creoles, at least at first. Most of the skilled and managerial positions within the region's expanding iron industry went to creole blacks as did the artisanal work in flour mills and weaving houses. On some plantations, moreover, artisan and house status became lodged in particular families with parents passing privileged positions on to their children. Increasingly, skilled slaves entered the market economy by selling their own time and earning money from "overwork," thereby gaining a large measure of freedom. For the most part, Africans remained on rude, backwoods plantations tending the broad-leaf weed. Since creole slaves sold at a premium price and most great planters had already established self-sustaining slave forces, small planters purchased nearly all of the newly arrived Africans after mid-century. These upward-striving men generally owned the least developed, most distant farms. Their labor requirements remained primitive compared to the sophisticated division of labor on the self-contained plantation-towns.⁶⁰

Over the long term, however, economic changes sped the integration of Africans into Afro-American society. Under the pressure of a world-wide food shortage, Chesapeake planters turned from the production of tobacco to that of food-stuff, especially wheat. The demands of wheat cultivation transformed the nature of labor in the region. Whereas tobacco farming required season-long labor, wheat farming employed workers steadily only during planting and harvesting. The remainder of the year, laborers had little to do with the crop. At the same time, however, wheat required a larger and more skilled labor force to transport the grain to market and to store it, mill it, and reship it as flour, bread, or bulk grain. Economic changes encouraged masters to teach their slaves skills and to hire them out during the slack season. At first, these opportunities went mostly to creoles, but as the wheat economy grew, spurring urbanization and manufacturing, the demands for artisans and hirelings outstripped the creole

Slave Trade in Colonial Maryland," *Maryland Magazine of History*, 73 (1978): 30-45; and Winthrop D. Jordan, "Planter and Slave Identity Formation: Some Problems in the Comparative Approach," in Rubin and Tuden, *Comparative Perspectives on Slavery in New World Plantation Societies*, 38.

⁶⁰ Kulikoff, "The Beginnings of the Afro-American Family in Maryland," 185-86; Jordan, *White over Black*, 405 n. 7; Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia*, 83-139; "Description of Servants, 1772," Northampton Furnace, Ridgely Account Books, Maryland Historical Society, Baltimore; and Ronald L. Lewis, *Coal, Iron, and Slaves: Industrial Slavery in Maryland and Virginia, 1715-1865* (Westport, Conn., 1979), 82-84, 162-63.

population.⁶¹ An increasing number of Africans were placed in positions previously reserved for creoles. The process of cultural transformation that earlier in the eighteenth century had taken a generation or more was considerably shorter at mid-century. Africans became Afro-Americans with increasing rapidity as the century wore on, eliminating the differences within black society that African importation had created.

Chesapeake blacks enjoyed considerably less autonomy than their lowcountry counterparts. Resident planters, small units of production, and the presence of large numbers of whites meant that most blacks lived and worked in close proximity to whites. While lowcountry planters fled to coastal cities for a large part of the year, the resident planter was a fixture of Chesapeake life. Small freeholders labored alongside slaves, and great planters prided themselves on regulating all aspects of their far-flung estates through a combination of direct personal supervision and plantation-based overseers. The latter were usually white, drawn from the region's white majority. Those few blacks who achieved managerial positions, moreover, enjoyed considerably less authority than lowland drivers. The presence of numerous nonslaveholding whites circumscribed black opportunities in other ways as well. While Chesapeake slaves commonly kept gardens and flocks of barnyard animals, white competitors limited their market and created a variety of social tensions. If lowcountry masters sometimes encouraged their slaves to produce nonstaple garden crops, whites in the Chesapeake—slaveholders and nonslaveholders alike—complained that blacks stole more than they raised and worked to curb the practice. Thus, at every turn, economy and society conspired to constrain black autonomy.

The requirements of tobacco cultivation reinforced the planters' concern about daily work routine. Whereas the task system insulated lowcountry blacks against white intervention and maximized black control over their work, the constant attention demanded by tobacco impelled Chesapeake planters to oversee the tedious process of cultivating, topping, worming, suckering, and curing tobacco. The desire of Chesapeake masters to control their slaves went beyond the supervision of labor. Believing that slaves depended on them "for every necessity of life," they intervened in the most intimate aspects of black life. "I hope you will take care that the Negroes both men and women I sent you up last always go by the names we gave them," Robert "King" Carter reminded his steward. "I am sure we repeated them so often . . . that everyone knew their names & would readily answer to them." Chesapeake planters sought to shape domestic

⁶¹ Carville Earle and Ronald Hoffman, "The Urban South: The First Two Centuries," *Perspectives in American History*, 10 (1976): 26–76; Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia*, 87–88, 124–27; and Gray, *Agriculture in the Southern United States*, 2: 602–17. Although the best study of slave hiring in the Chesapeake region focuses on the post-Revolution years, the forces promoting slave hire after the war suggest that the practice predates the Revolution. See Sarah S. Hughes, "Slaves for Hire: The Allocation of Black Labor in Elizabeth City County, Virginia, 1782 to 1810," *William and Mary Quarterly*, 3d ser., 35 (1978): 260–86. Also see Robert Carter to Warner Lewis, October 16, 1773, and October 20, 1774, to Mrs. Corbin, September 27, 1775, to Griffin Garland, September 29, 1775, and to John Ballantine, July 7, 1777, Carter Papers, typescript, Duke University, Durham, N.C. Allan Kulikoff has estimated that the proportion of blacks working as agricultural laborers dropped from 90 to 82 percent between 1733 and 1776; see his "Tobacco and Slaves: Population, Economy, and Society in Eighteenth-Century Prince George's County, Maryland" (Ph.D. dissertation, Brandeis University, 1976), 235–39.

relations, cure physical maladies, and form personalities. However miserably they failed to ensure black domestic tranquility and reform slave drunkards, paternalism at close quarters in the Chesapeake had a far more potent influence on black life than the distant paternalism that developed in the lowcountry. Chesapeake blacks developed no distinct language and rarely utilized African day names for their children.⁶² Afro-American culture in the Chesapeake evolved parallel with Anglo-American culture and with a considerable measure of congruence.

THE DIVERSE DEVELOPMENT OF AFRO-AMERICAN CULTURE during the seventeenth and eighteenth centuries reveals the importance of time and place in the study of American slavery. Black people in colonial America shared many things: a common African lineage, a common racial oppressor, a common desire to create the richest life possible for themselves and their posterity in the most difficult of circumstances. But these commonalities took different shape and meaning within the diverse circumstances of the North American mainland. The nature of the slave trade, the various demographic configurations of whites and blacks, and the demands of particular staples—to name some of the factors influencing the development of slave society—created at least three distinctive patterns of Afro-American life. Perhaps a finer analysis will reveal still others.

This diversity did not end with the American Revolution. While African-creole differences slowly disappeared as the centerpole of black society with the closing of the slave trade and the steady growth of an Afro-American population, other sources of cohesion and division came to the fore.⁶³ Differences between freemen and bondsmen, urban and rural folk, skilled and unskilled workers, and browns and blacks united and divided black people, and made black society every bit as variable and diverse during the nineteenth century as in the eighteenth. Indeed the diversity of black life increased substantially during the antebellum years as political changes abolished slavery in some places and strengthened it in others, as demographic changes set in motion by the Great Migration across the Lower South took effect, as the introduction of new crops enlarged the South's repertoire of staples, and as the kaleidoscopic movement of the world market sent the American economy in all directions.

⁶² Robert Carter to Robert Jones, Robert "King" Carter Letterbooks, Alderman Library, University of Virginia, Charlottesville (I am grateful to Emory Evans for alerting me to this letter); and Robert Carter to William Carr, March 15, 1785, Carter Papers, typescript, Duke University, Durham, N.C. Also see Carter, *Diary of Colonel Landon Carter of Sabine Hall, passim*; Robert Carter to his various stewards and overseers (Rubin Sanford, Clement Brooke, Newyear Branson), Carter Papers, typescript, Duke University, Durham, N.C.; Fitzpatrick, *The Writings of George Washington*, esp. vols. 32–34; Depositions of James Holland, William Ferguson, and Charles Gardiner, August 23, 1793, Lloyd Family Papers, Maryland Historical Society, Baltimore; and Betts, *Thomas Jefferson's Farm Book*, pt. 2: 16. For the striking difference in naming patterns of Chesapeake and lowcountry bondsmen, compare the slave lists in the Charles Carroll Account Book, Maryland Hall of Records, Annapolis, and the Charles C. Pinckney Plantation Journal, Manuscript Division, Library of Congress, Washington, D.C.

⁶³ For the importance of African-creole differences in understanding black reactions to the revolutionary crises of the last quarter of the eighteenth century, see Michael Mullin, "British Caribbean and North American Slaves in an Era of War and Revolution, 1775–1807," in Jeffrey J. Crow and Larry E. Tise, eds., *The Southern Experience in the American Revolution* (Chapel Hill, N.C., 1978), 235–67.

If slave society during the colonial era can be comprehended only through a careful delineation of temporal and spatial differences among Northern, Chesapeake, and lowcountry colonies, a similar division will be necessary for a full understanding of black life in nineteenth-century America. The actions of black people during the American Revolution, the Civil War, and the long years of bondage between these two cataclysmic events cannot be understood merely as a function of the dynamics of slavery or the possibilities of liberty, but must be viewed within the specific social circumstances and cultural traditions of black people. These varied from time to time and from place to place. Thus no matter how complete recent studies of black life appear, they are limited to the extent that they provide a static and singular vision of a dynamic and complex society.

Reviews of Books

GENERAL

QUENTIN SKINNER. *The Foundations of Modern Political Thought*. Volume 1, *The Renaissance*; volume 2, *The Age of Reformation*. New York: Cambridge University Press. 1978. Pp. xxiv, 305; vi, 405. Cloth \$29.50, paper \$9.50 each.

Quentin Skinner professes three aims in writing his monumental study of the "foundations of modern political thought": he wishes to present a simple outline of the principal texts; he wants to indicate "the process by which the modern concept of the State came to be formed"; and he attempts to exemplify his ideas of how historical texts should be studied, namely, by placing "the leading theorists" in "the more general social and intellectual matrix out of which their works arose." It is fair to say that, with some cavils, he has accomplished these goals. As a matter of observed fact he seems to me to have presented a remarkably detailed summary of almost all major and minor thinkers who contributed something of significance to either their own time's conceptions of politics or ours; moreover, with the exception of a very few of the latest works, he has brought to bear just about every worthwhile interpretive study of the last half-century that may be thought to throw light on his subject (essentially by English-speaking scholars, but including some in French, German, and Italian). If it were not for the fact that, though extremely broadly defined, his subject is yet limited to the history of political thought, one would be tempted to recommend these volumes as an up-to-date interpretive survey of the period as a whole in the best English university style of historical synthesis. His judgments are (from my own point of view, of course) nearly impeccable in the more controversial corners of this broad field. Common sense, wide reading, and methodological sophistication are smoothly and pleasingly blended in an altogether satisfying way for both the author and a doubtlessly wide range of readers.

And yet, despite so many very admirable features of this large and perfectionistic work, and even in

the core of probably its strongest achievement, the worm of discontent still squirms. I like his contextual approach very much. The book is organized into a succession of clusters of schools, sects, and movements, each of which makes an important contribution to the development of political thought and constitutes a subculture of the period as a whole. Within each, a collection of minor or major figures presents a collective ideology that provides the parameters within which rulers and other political agents may act in Skinner's view, although this is not always explicitly demonstrated. Thus it is his contention, and dearly held, that political thought, viewed in this collectivistic, ideological way, attains a general historical importance lacking in isolated, history-of-ideas treatments of famous men or great thinkers. He manages in this method to be both an internalist, emphasizing the particular tradition and interests from which a given thinker emerges, and an externalist, viewing the subculture as having a broad impact on the larger historical situation. Here one might observe a less explicit but always implied concern with the impact of events and structures, in their turn, on ideas and ideologies.

The discontent one feels may well be inherent in the author's compulsion to present so many not very interesting lesser figures who indeed provide the context out of which a Machiavelli, or a Thomas More, or a Jean Bodin arises. My approbation for dealing with intellectual history culturally and collectively in this fashion is necessarily strong, because I have practiced it and constantly advocate it myself. But one must confess to missing the sense of the dramatic that a sharper focusing on and featuring of the truly revolutionary minds would provide. Such thinkers took the givens of their group and even polemicized and ridiculed them in order to bring forth what in the end mattered more in public life and to reveal, perhaps, the true contribution of the boring, routinized thinking of the schools that fathered the more daring ones. It is not that Machiavelli and More were not "true" humanists of their own time and place but that they made the issues that underlay the growling and quarreling of

their day bright and clear to be faced as challenges or, more likely, paid lip service to and embarrassingly ignored.

Skinner deals with Machiavelli's and More's critiques of other humanists in the last chapter of volume one, and he follows Felix Gilbert and J. H. Hexter in his interpretation. Though he intends the reader to see a continuity between the *ragione di stato* he sees in Machiavelli (following Meinecke/Gilbert) and the clarity of Buchanan's secularist affirmation of the objectivity of sovereignty, the issue is somehow obscured by the complexity of his exposition of German, English, French, Italian, and Spanish Reformation and Counter-Reformation controversies. If such continuity, in history, really is the issue!

And yet the demonstrated continuity from the late *ducento* city-state struggle for *libertas*—independence and self-government—to the late sixteenth-century contest between proponents of monarchical and popular sovereignty, as Skinner has presented it, is indeed massive. And his effort to relate it to theological and ecclesiastical issues is never failing. But for this reviewer, and without the least sense of attempting to promote a populist perspective, this concentration—the concentration of the political philosopher—on the status of the state seems genetic-modernizing and retrospective, doing full justice neither to the minds and passions of the major thinkers nor to the religious, moral, familial, and survivalistic concerns of the great mass of the population. Was the condition and destiny of mankind totally determined by divine providence, or did man himself have a role? What kind, how much, under what circumstances? The center of consciousness of those centuries of Europe's history lay in such questions, not in revival of ancient and in the foundation of modern political thought.

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DIETMAR ROTHERMUND. *Europa und Asien im Zeitalter des Merkantilismus*. (Erträge der Forschung, number 80.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1978. Pp. vi, 184.

Prompted by the paradox that during the "Age of Mercantilism (1602–1775)" Europe's trade with Asia displayed an antimercantilist character, Dietmar Rothermund has produced an exceedingly useful overview of that trade. He has drawn together the results of research in several areas and has suggested a provocative synthesis.

In the first place, he stresses the pivotal importance of the Asian trade for European economic development. It was, as Rothermund puts it, an *angel-punkt*. The herring trade and the Baltic grain trade

may have bulked quantitatively larger in the seventeenth-century Dutch economy, but the Dutch East India Company trade became the model for Europe's commercial revolution: a re-export trade that encouraged free-trade practices in Holland, low interest rates, capital accumulation, easy exchange of securities, and a close partnership between merchants and government. The Asian trade produced similar results in Britain. In addition, the importation of large quantities of cheap Indian cotton goods after 1650 stimulated the innovations in the British textile industry associated with the beginnings of the Industrial Revolution. In fact, Rothermund demonstrates that the Asian trade was also pivotal in the development of free-trade theory. Adam Smith's predecessors were men like Thomas Mun, Charles Davenant, Josiah Child, and the anonymous author of "Considerations upon the East Indian Trade," who defended the British East India Company against its mercantilist opponents. The Asian trade, therefore, was crucially important for the commercial, financial, and industrial revolutions in Europe.

The trade produced no such results in Asia, however, because, Rothermund contends, the major Asian empires during the seventeenth and eighteenth centuries—the Ch'ing dynasty in China, Mughal Empire in India, and Tokugawa Japan—were land-based empires for which the trade with Europe was of peripheral importance and thus attracted little government interest beyond its tax potential. In each of these empires the land tax remained the mainstay of the governments and thus prevented government policy from being influenced by commercial considerations. Even the unfavorable balance of payments characteristic of the Asian trade seemed to benefit Europe. The continuously increasing flow of silver into the Asian lands seems to have reduced the value of silver in Asia, kept its value high in Europe, and allowed Europeans to import massive quantities of Asian goods at remarkably stable prices. Rothermund observes that contemporary economic theory, dominated by equilibrium models, seems unable to explain the phenomenon. He suggests that what happened in the Asian states is analogous to a modern currency devaluation.

It is a small book; I wish it were much larger. It is suggestive rather than exhaustive, but it clarifies and synthesizes the state of research on an exceedingly important topic. It should stimulate some critical discussion and more research. For this reason and because it is useful as it stands for all students of early modern history, it should be translated into English. It contains a good annotated bibliography.

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RAYMOND GREW, editor. *Crises of Political Development in Europe and the United States*. (Studies in Political Development, number 9.) Princeton: Princeton University Press. 1978. Pp. xi, 434. Cloth \$27.50, paper \$6.95.

Most historians may not have noticed—why should they?—but during the 1960s a self-conscious group of political scientists sponsored by the Committee on Comparative Politics of the Social Science Research Council developed schemes for analyzing the “political development” of nations old and new. Broad analytic dimensions of political functioning were elaborated in an attempt to encompass Third World and Western “political systems” within a single universally applicable theoretical framework. All such systems, it was hypothesized, would “modernize” (or had done so) by developing greater political equality, governmental capacity, and differentiation of specialized political institutions. What is more, a set of five basic “crises” was pinpointed, through which all modernizing polities might be supposed to pass: (1) a crisis of identity, or the development of a sense of shared nationality; (2) a crisis of legitimacy, in which the government comes to be accepted as morally valid; (3) a crisis of participation, referring to the involvement of increasing proportions of the citizenry in political affairs; (4) a crisis of penetration, or governmental ability to exert effective controls over territory and aspects of social life; and (5) a crisis of distribution, in which government meets demands for allocations of material benefits and property rights among strata of the citizenry.

Bravely, the Committee on Comparative Politics decided at a certain point to come down from the theoretical clouds and ask an earthbound collection of ten distinguished historians (and historically oriented social scientists) to “apply” political development theory to their countries of specialization: the United Kingdom, Belgium, Scandinavia, the United States, Spain and Portugal, France, Italy, Germany, Russia, and Poland. The committee was especially curious to see how well its ideas would work for the political history of the West, the birthplace of modernity. Skillfully edited by Raymond Grew, *Crises of Political Development in Europe and the United States* is the offspring of this shotgun marriage of grand theory from political science with the evidence of history. Yet, as any reader will notice from the start, this mixed interdisciplinary union has already ended in divorce; indeed, the partners took to quarreling on the very night of the wedding.

Most of the contributors to *Crisis of Political Development* never were comfortable with the concepts they were supposed to apply. As evidenced by Keith Thomas's scathing critique in the opening pages of his lead-off essay on the United Kingdom,

some of the contributors rejected altogether the teleological and elitist assumptions of an approach that sees modernization as “progress” achieved through stabilizing maneuvers by governmental leaders. At a more practical level, the five crises themselves seemed imprecise and difficult to associate with certain historical events or phases rather than others. The historians ended up treating the crises simply as problems that any government might have to face at any point in history. No significant cross-national sequences were established; nor did it turn out that every country had faced—or resolved—severe difficulties in all of the problem areas. Virtually all of the authors concluded that political development theory downplays matters of fundamental explanatory importance, such as the impact of international environments on national sovereignty and domestic conflicts and the impingement of social structures on political processes and institutions.

What value there is in *Crises of Political Development* lies in the parts rather than the book as a whole. Some of the essays are especially skillful, up-to-date surveys of national political histories: Keith Thomas on the United Kingdom, David Bien and Raymond Grew on France, and Walter Pintner on Russia fall under this rubric. Other essays pull together information not readily available; for example, Aristide Zolberg on Belgium and Roman Szporluk on Poland. Here and there, contributors have taken advantage of the freedom allowed by this sort of book to offer fascinating speculations on long-standing historical problems: examples include Thomas on peripheral nationalisms in British history (pp. 46–56 *passim*); Bien and Grew on the old-regime origins of democratic participation in France (pp. 248–49); and Zolberg on the evolution of Belgian national identity (pp. 106–10, 124–29). Finally, all of the essays contain bibliographies of English-language works on the political histories of the countries discussed, and these may prove useful for students and nonspecialists.

Editor Grew concludes his introduction to *Crises of Political Development* with what can be considered a fitting epitaph not only for this book but also for political development theory itself. “A next step,” Grew says, “should be the careful formulation of historical (and therefore not just developmental) problems, followed by the comparison of realities rather than abstractions” (p. 39). To this one can only murmur a heartfelt “amen.”

THEDA SKOCPOL
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LESZEK KOLAKOWSKI. *Main Currents of Marxism: Its Rise, Growth, and Dissolution*. Volume 1, *The Founders*; volume 2, *The Golden Age*; volume 3, *The Breakdown*.

Translated by P. S. FALLA. Oxford: Clarendon Press. 1978. Pp. xiii, 434; viii, 542; xii, 548. \$19.95; \$19.95; \$19.95.

Leszek Kolakowski first came to the attention of Western readers after 1956, when he cast off the Stalinist orthodoxy of his youth to emerge as the most articulate spokesman of Marxist humanism in Poland, if not in the entire Communist world. By 1968, however, when he was dismissed from his professorship of philosophy at the University of Warsaw and sought exile in the West, his commitment to Marxism, humanist or otherwise, had grown increasingly tenuous. In the decade since, which he primarily spent at All Soul's College, Oxford, Kolakowski turned his ambivalence into outright hostility. Appeals by socialists who had earlier been inspired by his libertarian critique of Stalinism, most notably E. P. Thompson's long and impassioned "Open Letter" of 1973, were turned aside with contempt, as Kolakowski recapitulated the familiar "God that failed" syndrome of prior apostates from Marxism. During that period, his newly militant anti-Marxism expressed itself in a number of shorter essays in journals such as *Encounter*, which often relied on religious as well as theoretical and historical arguments (Kolakowski adopting the label "inconsistent atheism" to describe his outlook). But at the same time, he let it be known that he was preparing a more substantial case for his political transformation. *Main Currents of Marxism*, modestly characterized as a "handbook" devoted to the history of Marxism as a doctrine, is the extraordinarily valuable result. In three hefty volumes totaling some fifteen hundred pages of powerfully written text, Kolakowski has attempted nothing less than a synoptic overview of the entire Marxist tradition from its origins to what he sees as its contemporary "breakdown." Drawing on literature in six languages, he summarizes and analyzes the thought and actions of more than a hundred leading figures in or related to that tradition.

As befits a philosopher, Kolakowski begins volume one, *The Founders*, with a long and insightful account of "the origins of dialectic," which he traces back to Neoplatonist soteriology. Arguing that Marx himself was first and foremost a "German philosopher," he dwells on the philosophical dimension of his theory with particular care. The analysis he presents derives, as he freely admits, from Lukács's interpretation in *History and Class Consciousness*, although he vigorously distances himself from Lukács's Leninist politics. Even though little new ground is broken in his discussion, with its familiar distinction between Marx's anthropocentric philosophy of praxis and Engels's scientific naturalism, the exposition is a model of lucidity and fidelity. In addition to the attention he pays to the philosophi-

cal roots of Marxism, Kolakowski skillfully presents its economic foundations and supplies telling critiques of the labor theory of value and other essential dimensions of the Marxist position. He also spells out the tenets of historical materialism, whose heuristic power he admits but whose dogmatization he deplores. His remarks on the fallacy of relying on the causal power of the economy "in the last resort" are particularly acute. Volume one also includes short sketches of Marx's major competitors among nineteenth-century socialists and a concluding consideration of the links between Marxism and Leninism, a theme that preoccupies Kolakowski throughout the trilogy.

As its title implies, *The Golden Age*, his second volume, portrays the frequently maligned era of the Second International in essentially sympathetic terms. Not yet rigidly dogmatic in doctrine, fragmented into warring sects, or split into theoreticians and practical politicians, Marxism in the years before World War I experienced rapid institutional growth and considerable intellectual distinction. Although he remains faithful to his Lukácsian reading of Marx's own work and disdains the antinomic fissures in Second International theory, Kolakowski nonetheless gives essentially respectful accounts of the major figures of the era. Included among his portraits are familiar faces like Kautsky, Luxemburg, Bernstein, Jaurès, Lafargue, Sorel, Labriola, the Austro-Marxists, and Plekhanov, as well as less well-known ones such as Krzywicki, Kelles-Krauz, and Brzozowski. The last and most intriguing of this Polish trio is credited with being the first Marxist to sense the distance between Marx and Engels on fundamental theoretical issues. In the second half of the volume, Kolakowski turns to Russian Marxism and the rise of Bolshevism. While acknowledging Lenin's political genius and agreeing that 1917 was not a "Bolshevik *coup d'état*" but a true revolution of workers and peasants, he nonetheless emphasizes the continuity between Leninist principles and subsequent Stalinist practice. He has little patience with those defenders of Lenin who point to his final qualms about the direction of the revolution and argues instead that Lenin failed to recognize the roots of the dilemma in his own contempt for law, democratic procedure, and theoretical consistency.

Kolakowski carries his insistence on continuity between Leninism and Stalinism through his third volume, which is devoted to what he sees as Marxism's "breakdown." His treatment of Marxism in power devotes more space to the interaction of doctrine and events than does his discussion of the tradition before 1917. Even in this volume, however, he attributes more causal efficacy to ideology than to circumstances in explaining the betrayal of the revolution. Among the more notable of his analyses

are accounts of the purge trials and their ramifications, the permeation of Marxist-Leninist dogmatism into virtually all facets of Soviet culture, and the impact of the communization of Eastern Europe. When it comes to examining the contention that Bolsheviks other than Stalin, most notably Bukharin and Trotsky, would have followed a very different path, Kolakowski argues that the constraints of the Bolshevik model of revolution would have precluded such an outcome. He seems to reserve a special scorn for Trotsky's fate after his fall from power, which he characterizes as that of "only a deposed leader who tried desperately to recover his role, who could not realize that his efforts were vain, and who would not accept responsibility for a state of affairs which he regarded as a strange degeneration, but which was in fact the direct consequence of the principles that . . . he had established as the foundations of socialism."

In the second half of *The Breakdown*, Kolakowski shows himself no more amenable to the proposition that Western Marxism or Maoism offer a significant improvement over the Soviet model. In perhaps the most vituperative and ungenerous chapters of the trilogy, he examines the thought of Lukács, Korsch, Gramsci, the Frankfurt School, Goldmann, and Bloch, while passing more rapidly over Sartre, Althusser, and various East European revisionisms, in a general discussion of Marxism after Stalin's demise. Lukács is characterized as "perhaps the most striking example in the twentieth century of what may be called the betrayal of reason by those whose profession is to use and defend it." Marcuse is damned as the "prophet of semi-romantic anarchism in its most irrational form" and the "ideologist of obscurantism." And Bloch is denounced as a "preacher of intellectual irresponsibility." These and comparable judgments are delivered from the perspective of a thoroughgoing positivism, which stresses the irreconcilable disunity of facts and values and rejects a praxis-grounded definition of truth as mere "species relativism." Relying on Popper's criterion of falsifiability, he dismisses the scientific pretensions of Marxism and concludes that it is best understood as "the greatest fantasy of our century." Attempts at grounding Marxism in nonscientific ways, most importantly that of Habermas, he gives only an impatient glance. In short, no one is likely to quarrel with Kolakowski's own admission that he could not treat the more recent period "with the desirable detachment."

Inevitably, the careful reader can also find the odd factual error or dubious interpretation to challenge, although it is remarkable how few of these there are in so mammoth an undertaking. And with equal certainty, some readers will bemoan the absence of their own favorite "main current," such as

Marxist historiography or the Marxist attitude toward the Jewish question. But however one may wish Kolakowski had come to different conclusions or investigated other issues, there can be no doubt that *Main Currents of Marxism* is an awesome achievement of synthetic scholarship and critical analysis. Ironically, in exorcising his demon, Kolakowski has produced a monument of committed learning whose every page undercuts the positivist assumption that facts and values can indeed be held apart.

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JAMES MILLER. *History and Human Existence: From Marx to Merleau-Ponty*. Berkeley and Los Angeles: University of California Press. 1979. Pp. ix, 287. \$17.50.

James Miller's *History and Human Existence* is a lucid and extremely readable account of rationalist subjectivity and individuation as essential aspects of Marx's thought, the eclipse of these aspects in the objective determinism of orthodox Marxism, their relatively dogmatic retention in a variety of early twentieth-century Marxists (for example, Rosa Luxemburg and György Lukács), and their critical reformulation in the "existential Marxism" of Jean-Paul Sartre and Maurice Merleau-Ponty. Parts of this story have been told before, especially in the cases of orthodox Marxism and Sartre, but Miller's book is well worth reading, for it provides an informed discussion of the pertinent problems in Marx's own work, and it re-emphasizes the importance of Merleau-Ponty. It brings out the one-sidedness of interpretations of Marx that devote exclusive attention either to his "objective," deterministic science of history or to the communitarian elements of his thought that might seem to justify totalitarianism. It also shows how the thought of Sartre and Merleau-Ponty raised questions about Marx's understanding of rationality and freedom—Sartre through his exploration of the anxiety and burdens of freedom and Merleau-Ponty through his rich and problematic interpretation of the ambiguity of history. Seen in more general terms, Miller's study is an excellent example of the recent attempt to reinvigorate critical thought by reassessing the role of subjective agency and individuation in the history of radicalism.

Miller is especially good in tracing the tensions in Sartre's thought, notably the seeming contradiction between a notion of absolute freedom and a notion of situated freedom. He also explores in an extremely suggestive way Merleau-Ponty's vision of ambiguity, its crucial relation to language and institutions, and its implications for an understanding of the historical process. Miller recognizes important

differences between Sartre and Merleau-Ponty, but his own attempt to set forth the basis of existential Marxism in human subjectivity and individuation makes him take those differences less seriously than did Sartre and Merleau-Ponty themselves. Miller has the pieces to carry the debate between these two giants of modern French thought beyond the more immediate political issues that divided them, but he does not put the pieces together in a forceful way. His desire for a synthetic perspective, centered on the active human subject, also leads Miller to underplay somewhat the tensions he sees in Marx's account of subjectivity (for example, in relation to class consciousness) and objectivity (for example, in relation to laws of economic development) as components of critical thought and action. A pointed discussion of "interest" in Marx describes the notion as an ambivalent supplement to seeming opposites such as passion and reason or individual and class. But Miller strains to present this notion as the "heart of Marx's synthesis," reconciling subjectivity and objectivity through a neo-Enlightenment "comprehension of subjectivity as rationally directed interest and causally effective labor" (p. 97). An approach to interpretation with a less one-sided interest in synthesis might have enabled Miller to explore more fully the interaction between unifying and self-contestatory movements in Marx, and the results might not have been altogether negative.

One important question that Miller does not adequately address is whether the attempt to provide a historical and critical account of what is living and what is dead in Marxism can enter into a constructive dialogue with recent French thought if it confines itself to selected aspects of existential Marxism. Miller understandably dismisses the heavy-handed, tendentious attack upon subjectivity of Louis Althusser, who equates the critique of humanism with an often confused defense of "scientific" objectivism. But Miller neglects the more cogent and nuanced critiques of humanism and the subject to be found in a number of recent French figures (Jacques Derrida, Philippe Sollers, Michel Foucault, and Gilles Deleuze among others). These figures might have forced Miller to reconsider some of his own assumptions about interpretation, for they have focused attention upon the problem of the relationship between the desire for binding synthesis and disseminating tendencies that contest it. In the process, they have provided rather different interpretations of Nietzsche and Heidegger from Miller's own.

With only relatively marginal doubts, Miller fits the historical process he treats into too neat a pattern: the achievement of Marx's synthesis, the breakdown of the synthesis in orthodox Marxism, its often doctrinaire recapitulation in more or less idealist forms of Marxism, and its primarily con-

structive re-creation in existential Marxism. This historical process was, I think, both more intricate and somewhat different in nature. (An elaboration of Miller's own brief discussion of Gramsci, Luxemburg, and the Critical Theorists would itself alone complicate the story considerably, and it is difficult to reconcile his often critical analyses of particular works by Sartre, especially the *Critique of Dialectical Reason*, with his favorable general estimate of Sartre's contribution to existential Marxism.) Yet, despite the objections one may make to the overall cast he gives to his account, one must readily recognize Miller's study as a significant contribution to the ongoing attempt to come to terms with the Marxist tradition. It is consistently informed by a fine intelligence addressing itself to important problems, and many of its specific interpretations are cogent and knowledgeable.

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A. J. H. LATHAM. *The International Economy and the Undeveloped World, 1865-1914*. Totowa, N.J.: Rowman and Littlefield, and Croom Helm, London. 1978. Pp. 217. \$21.50.

As secretary to the Third World Group of Economic Historians, A. J. H. Latham's primary purpose is to correct the view that the world economy of the period 1865-1914 was chiefly a British and a North American phenomenon. On the contrary, says the author, the world economy would never have developed as it did without the indispensable contribution of the poorer parts of the world.

In order to obtain "a truly global view of what the world's economy really was" (p. 13), Latham has provided us with several essays concerning communications, money and capital, international trade, population and migration, growth and fluctuations, social responses, and economic development and social change. Alas, the underdeveloped world being as large as it is (just how large is not clear) and the book being as slim as it is, the author has had to restrict himself to dealing with certain aspects of Asian and African history. For those parts of the world that are covered, there is ample statistical data to support the quantitative approach adopted here.

Although this work provides us with an informative and stimulating discussion of the world economy in the fifty years before 1914, it does so (partly out of necessity where the canvas is so large) within the confines of existing themes and with the use of existing materials. The student and the non-specialist may find a good deal that is new here, but for the international economist and economic historian this book holds few surprises. Nor are the hopes

aroused in the introduction, concerning the development of the world's railways and world freight rates, fully realized. There is, in fact, little here that has not already appeared in print—not least, in my own *Impact of Western Man* (1967) and my *Emergence of an International Economy* (1970). Moreover, while the author is to be commended for criticizing those who see the development of the world economy in Western terms alone, the idea that the West is not the world long precedes these pages. The late A. J. Toynbee, L. S. Stavrianos, Geoffrey Barraclough, and I—among others—have been saying this for years.

Indeed, my worry is whether the author does not press his point concerning the role of the underdeveloped world a bit hard. His assertions that Britain's free-trade policy was dependent on its Asian balances, or that the Africans and Asians were equally able to respond to economic incentives, I find difficult to accept. The point about economic incentives, surely, is not that individual Africans and Asians (under constraint) could be as capitalistic and money and market oriented as any Westerner, but whether these societies were as committed to these values as Western societies were.

Ultimately, the worth of any book depends on what use is made of it. I would think that the best use that could be made of this work would be to employ it as a text or as supplementary reading, and for this purpose it would have my support.

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TREVOR I. WILLIAMS, editor. *A History of Technology*. Volume 6, *The Twentieth Century c. 1900 to c. 1950*, part 1; volume 7, *The Twentieth Century c. 1900 to c. 1950*, part 2. Oxford: Clarendon Press. 1978. Pp. xxv, 689; xix, 692–1530. \$82.00 the set.

During the 1950s the history of technology assumed the institutional features of a distinctive historical specialty complete with a society, a journal, university courses, and graduate curricula. The new discipline was also fortunate that in these same years a team of British scholars surveyed the entire subject from prehistory to 1900 in five monumental volumes entitled *A History of Technology* (1954–58). Its chief editor, Charles Singer, had conceived the project and subsequently recruited three associate editors: E. J. Holmyard, A. R. Hall, and Trevor I. Williams.

To make the project manageable, the editors defined technology fairly narrowly and confined the narrative largely to the West. Believing that twentieth-century technology was too diverse and complex for communication to nonspecialists, they chose 1900 as the cutoff date. They also felt it to be too recent for discerning historical analysis.

Despite these reservations, one of these editors, Trevor I. Williams, decided some twenty years later to carry the narrative forward to 1950. The passage of time had provided a surer perspective, and the unexpectedly vigorous sale of the initial volumes encouraged him to take up the challenge of making the development of recent technology comprehensible to laymen. He has succeeded to a high degree, thereby assuring that the whole set, now augmented by two volumes, will remain in the foreseeable future the most comprehensive treatment of the subject, an indispensable reference work for any academic or public library.

Some fifty-eight chapters by as many authors treat almost every conceivable technical topic plus a few, such as the technology of ocean depths, which might not come readily to mind. Chapters two through seven on the symbiosis of modern technology with science, business, trade unions, government, and education are excellent. Nothing quite like them was possible when the earlier Singer volumes were written. Yet, although they reflect a quarter century of expanding scholarship in the history of technology, they do not do so to the fullest extent. Most curious, for instance, is the absence of a discussion of the growth of the engineering professions. Also, I believe that an introductory chapter on the historiography of technology in the twentieth century would have served the reader better than the existing one, which reviews political history since 1900 with no attempt at integrating technology with general history.

The remaining fifty-one chapters describing the progress of various technologies are all technically competent. Many are fascinating, but their historical sophistication varies widely. Although the chronology is usually clear, most chapters are Whiggish and betray a protechnical bias that soft-pedals the effects of modern military technology and occasionally erupts into open contempt for environmental critics (pp. 256, 1481). Still, there are several examples of outstanding historical craftsmanship. My favorite is Lynwood Bryant's history of the internal combustion engine, which blends high erudition, both technical and historical, with keen observation on the obvious and subtle ways these motors have transformed society.

The illustrations and charts are very useful. One wishes for more, but space and cost considerations probably made this impractical. Much less excusable is the documentation. Footnotes and bibliographies are frequently skimpy, in some cases altogether absent. The bias is distinctly Anglo-Saxon: British technology receives primary attention; the United States comes next; Western Europe and Japan are a weak third; Eastern Europe is covered even more lightly; and the rest of the world is hardly mentioned—Latin America, not at all. Yet

this history of twentieth-century technology is the most competent and comprehensive available. It challenges other societies to do likewise from their cultural perspective. To do it better would be quite an achievement.

JOHN J. BEER
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THEDA SKOCPOL. *States and Social Revolutions: A Comparative Analysis of France, Russia, and China*. New York: Cambridge University Press. 1979. Pp. xvii, 407. Cloth \$29.50, paper \$7.95.

This is a clear, concise, and admirably well-crafted book. The argument may be summarized in Theda Skocpol's own words as follows: "The revolutionary crises developed when the old-regime states became unable to meet the challenges of evolving international situations. Monarchical authorities were subjected to new threats or to intensified competition from more economically developed powers abroad. And they were constrained or checked in their responses by the institutionalized relations of the autocratic state organizations to the landed upper classes and the agrarian economies. Caught in cross-pressures between domestic class structures and international exigencies, the autocracies and their centralized administrations and armies broke apart, opening the way for social-revolutionary transformations spearheaded by revolts from below" (p. 47). And, "Questions of state power have been basic in social-revolutionary transformations, but state power cannot be understood only as an instrument of class domination, nor can changes in state structures be explained primarily in terms of class conflicts. In France, Russia, and China, class conflicts—especially between peasants and landlords—were pivotal during the revolutionary interregnums. But the nature of the New Regimes that emerged from the revolutionary conflicts depended fundamentally upon the structure of the state organizations and their partially autonomous and dynamic relationships to domestic class and political forces, as well as their positions in relation to states abroad" (p. 284).

Politics, in short, comes first. Society and the economy may set limits but do not define the course nor determine the result of social revolutions. The state is not reducible to an expression of class interest, but has interests and a dynamic of its own. Competition between tax and rent collectors for peasant resources may, in fact, be taken for granted in Old-Regime, predominantly agrarian societies. State interests and the interests of the predominant social class are therefore systematically opposed, save when both are threatened by some foreign power. If such a threat fails to provoke reconcilia-

tion of the contending interests of rulers and landed classes, social revolution has its chance.

Skocpol illustrates her general ideas by analyzing the French, Russian, and Chinese revolutions in some detail, finding a basic parallel in the causes of each revolution, even though their outcomes diverged significantly and systematically from case to case. Along the way she scatters a series of arresting summary statements. For example: "To make his eclectic system work, Napoleon judiciously dispensed with nonroutinized mass mobilizations and with all manifestations of ideological commitment. Wielding instead symbols, rituals, and propaganda of a highly generalized French nationalism, Bonaparte decorated his essentially authoritarian-bureaucratic regime with a hodgepodge of symbolic concessions to the inherited factions: plebiscitary and patriotic rituals for the radicals; consultative councils with restricted electoral bases for the liberals; and a Concordat with the Catholic Church for conservatives" (p. 195). Shrewd and persuasive comparisons punctuate her analysis of the separate revolutionary processes. I particularly liked her argument about why the Bolsheviks were able to hold power whereas the Montagnards were not (p. 192) and the discussion of divergent relations of Russian and Chinese Communists to the peasantry of their respective countries (pp. 277-79).

With so much to admire, it is perhaps captious to complain about Skocpol's implicit repudiation of the role of personalities in affecting the revolutionary processes she analyzes. To allow that sort of variable would, perhaps, spoil the sociology she seeks to discover in human affairs. She also clings to a residual revolutionary romanticism, as her final pages demonstrate, even though the logic of her argument makes Marx's aspiration for "working-class-based socialism" entirely implausible so far as I can see, though she declares it a "necessity" (p. 292).

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DAVID BIALE. *Gershom Scholem: Kabbalah and Counter-History*. Cambridge: Harvard University Press. 1979. Pp. vi, 279. \$16.50.

This well-conceived and impressively executed study of Gershom Scholem as a historian deepens our appreciation and extends the importance of this pioneering and still leading scholar of Jewish mysticism. David Biale shows how Scholem uses history as a discipline for determining indigenous Jewish sources and thus for preserving the legitimate tradition of Judaism. He also shows how Scholem's use of history is itself a part of that legitimate tradition—that is, one more application of the interpretive mode of Kabbalah, the esoteric Jewish mys-

tical tradition of documentary reinterpretation. Yet Biale, who otherwise closely identifies with Scholem, skillfully shows that Kabbalah and Scholem's study of Kabbalah also provide a structural model for revitalizing modern historiography. In this, Biale goes beyond Scholem's intent but for laudable purposes.

In the most erudite and convincing parts of his study, Biale argues that Scholem's youthful return to Judaism at the turn of the century was among the most radical Jewish responses to the crisis of bankrupt assimilation. Motivated less by anti-Semitism than by filial dissatisfaction with an apologetic accommodation of Judaism to rational and ethical ideals, and finally rejecting as equally assimilative the attempts of his contemporaries to reconstruct their ties to Judaism (notably, Martin Buber's pre-war Nietzschean *Erlebnismystik* and the Zionist program of militant nationalism), Scholem struck out on his own "idiosyncratic" course to restore Judaism to its indigenous tradition.

As Biale makes clear, Scholem found in Kabbalah the most genuine and vital expression of Judaism. In describing Scholem's discovery of resurgent religious vitality in Jewish mysticism, Biale makes too little reference to the content of Kabbalah, a limitation which not only obscures the reason for Scholem's enthusiasm for Kabbalah, but produces a difficult stylistic elusiveness. Biale's strength lies instead in describing Scholem's historiographical argument for restoring Kabbalah to the center of Judaism. Two of Scholem's arguments are especially well represented in this study: the distinct immunity of the secret Kabbalah from secular influences and its consequent capacity to rejuvenate the normative though errant philosophico-legal tradition of rabbinic Judaism.

Ultimately, Biale argues that, by bringing to light the sources as well as the Sabbatian and Hasidic legacies of the subterranean Kabbalistic Jewish tradition, Scholem not only created a major revision of Jewish historiography but also contributed to constructing a new historiography. Though the new historiography bears a structural resemblance to Kabbalah, Biale prefers to call the unapologetic highlighting of previously suppressed material "counter-history." Counter-history clearly differs from "mainstream" historiography and even from revisionist historiography, which proposes a new philosophy of history or finds new facts. Yet, in spite of Biale's suggestion to the contrary, counter-history does not seem to escape the typical pitfalls that preclude historical objectivity. For example, Scholem's lifelong "hunt for [the] vital element" of irrationalism in Judaism (p. 201), which Biale himself characterizes as "a steady attempt to substantiate certain extraordinary intuitions as a student" (p. 133), is difficult to reconcile with the

critical method of constantly modifying *a priori* hypotheses under the impact of the sources, a process that forcibly redirects, not merely refines, the original investigation of the sources.

Nevertheless, counter-history serves as a useful framework for defining the structure of Scholem's prodigious penetration of the Jewish sources from the interpretive layers of mythmaking to the alien, crystalline past. By bringing to light one of the major thinkers of the twentieth century, Biale has provided an important study, one that will stimulate discussion among Jewish and European historians as well as among philosophers of history.

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WILLIAM A. CLEBSCH. *Christianity in European History*. New York: Oxford University Press. 1979. Pp. xii, 315. Cloth \$14.95, paper \$4.50.

William Anthony Clebsch in about 280 pages has drawn together a fresh synthesis of the history of Christianity in European culture. Thoughtful and knowledgeable about church history, Clebsch concentrates on human responses to manifestations of Christ in history, and thereby attempts to combine the partial insights of cultural and church history by reference to a human-centered history of religion. He accomplishes his purpose by selecting illuminating cases of varied human responses to manifestations of Christ. After setting the stage for such manifestations in European culture, Clebsch divides the history of Christianity into five main periods, which he labels citizenship, chaos, unity, allegiance, and autonomy, divided chronologically by Constantine, Charlemagne, Innocent III, and Louis XIV. As sovereignty shifted, according to Clebsch, Christianity served as an agent for European civilization in varied ways.

Within each of the five periods, Clebsch selects two religious exemplars of Christian forms of humanity in cultural transition. These are martyrs (Perpetua) and monks (Anthony) in the first period, theodiscists (Boethius) and prelates (Gregory the Great) in the period of chaos, mystics (Bernard of Clairvaux) and theologians (Anselm of Canterbury) in the unity period, moralists (Jeremy Taylor) and pietists (Zinzendorf) in the period of allegiance, and in the recent autonomy period Clebsch selects Bonhoeffer as an activist representative and waffles between Newman, Kierkegaard, and Ritschl as apologist representatives before finally selecting Kierkegaard.

Clebsch notes that traditional interpretations favor his martyr-monk and pietist-moralist categories, while theodicy-prelacy and activist-apologist rubrics

are less well supported by traditional historiographies. At any rate, Clebsch's method provides the reader with an exemplary teaching tool and a suggestive organizational scheme. The strengths of his book are his willingness to marshal materials under a clear interpretative scheme and his frequent ability to make use of unhackneyed materials. The level of learning throughout is high.

On the negative side, Clebsch's interesting book is likely to fall between two stools. While Clebsch may put off theologically oriented historians by a bit of disdain for their doctrinal commitments, his obvious enthusiasm for the effects of the "various manifestations of Christ" in history may offend his more secular historical colleagues.

The reviewer appreciates Clebsch's willingness to give credit to the claims of faithful Christians for being what the believers said they were—a perspective impenetrable to many modern sophisticates. Clebsch does neglect some of the greatest figures of Christian history, including Augustine of Hippo, the reformers, and Eastern Orthodox leaders except Justinian. He is weak on Scriptures and ecclesiology, and he appears to be enamored with what the most recent period has contributed to the understanding of Christianity. What Clebsch has done, however, he has done well.

Clebsch's presuppositional method deserves further discussion, development, and restatement. On first reading, it appears to be facile and insufficiently grounded methodologically. But it has the advantage of middle-ground, not too erudite characteristics that might possibly bridge gaps and establish connections among historians, sociologists, and theologians, who sometimes find difficulty in speaking and working together.

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KENNETH LEVIN. *Freud's Early Psychology of the Neuroses: A Historical Perspective*. Pittsburgh: University of Pittsburgh Press. 1978. Pp. ix, 314. \$12.95.

Kenneth Levin's monograph is a detailed account of the development of Freud's earliest psychoanalytic theories and of interest only to those already deeply involved in psychoanalytic history. By no means could it serve as a primer or an introduction for students. It is a book whose primary theses I find unsubstantiated, a work with many faults, and yet it contains some truly fine sections on the history and themes of nineteenth-century medicine, psychiatry, and neurology.

Levin argues two major positions. The first is that Freud, from the very start of his work with neurotic patients (ca. 1886), offered only psychological explanations and eschewed physiological theories of neurosis. Levin's second theme is that Freud's psy-

chological theories were not influenced by his early neurophysiological training and experience. I am at a loss to understand why Levin espouses two such extreme positions. His own discussions of Freud's theory-building even show that, at first, Freud strongly supported Charcot's physiological theories about hysteria and hypnosis and also that Freud believed neurasthenia and anxiety had physiological etiologies. Even stranger is Levin's insistence on totally negating the abundant evidence that Freud's neurophysiological training influenced his conceptualization of psychological mechanisms. Levin is committed to making an alternate case for the significance of Freud's clinical experience in his development of psychoanalysis. This is an important point (though hardly a novel one), but it does not justify the lopsided assertion that years of physiological research and experience made no impression on Freud. Levin could have written a much finer book if he had not felt compelled to mold his study around tenuous theoretical constructs, which often force him to distort his evidence to fit their rigid lines.

Levin is an able scholar when it comes to presenting the medical and psychiatric themes and controversies of Freud's day. He has accumulated a wide variety of information, especially on the struggle between the anatomists and physiologists over the etiology of disease, and he has a talent for explaining clearly the essence and significance of the thought of various scientists. He elucidates neatly the controversy surrounding Freud's 1886 paper on male hysteria as well as the enmity that developed between Meynert and Freud.

Unfortunately, more yet is amiss with Levin's monograph. It is a doctoral dissertation evidently completed in 1973 and published in 1978 with no attempt to incorporate recent scholarship. The references end at 1972, and the book suffers accordingly. The most glaring omission is Pribram and Gill's *Freud's "Project" Re-assessed* (1976). Even Levin's doctoral research is not complete. He should have consulted David Rapaport's *The Structure of Psychoanalytic Theory* (1960) and Stephen Kern's 1970 Ph.D. study, "Freud and the Emergence of Child Psychology, 1880-1910," a work that covers some of the same medical-historical ground as Levin's monograph. Levin's consistent misspelling of Salpêtrière is annoying, as are his overly long sentences (up to eighty-two words). He is given to broad statements, either oversimplifying a complex situation or making assertions without proof. These shortcomings, as well as Levin's one-sided and intrusive theorizing, mar his lucid account of certain late nineteenth-century medical and psychiatric developments and his well-organized presentation of Freud's early writings.

HANNAH S. DECKER
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VERN L. BULLOUGH and BONNIE BULLOUGH. *Sin, Sickness, & Sanity: A History of Sexual Attitudes*. New York: Garland Publishing. 1977. Pp. ix, 276. \$13.95.

There has long been a need for a short, concise, and readable history of sexuality not only for the general reader but also for undergraduate courses that deal with the topic. Vern L. Bullough and Bonnie Bullough have filled the gap by providing us with a volume that successfully draws on Vern Bullough's larger study, *Sexual Variance in Society and History* (1976). The new book has the strengths and weaknesses of the longer study. There are few, if any, scholars who know as much about the history of sexuality as Vern Bullough. His knowledge is encyclopedic, and it has been effectively distilled into this general history. The Bulloughs also are able to bring in a comparative perspective that will be very useful for the general reader. While the larger study gives us much more, there is some material in this volume on how the Western experience differs from Islamic, Taoist, and Hindu attitudes toward sexuality.

The Bulloughs study, however, has some shortcomings. Christianity is defined as a sex-negative religion, and there is great emphasis on the origins of antisex attitudes in Greek philosophy. After that, according to the Bulloughs, not much happens until the nineteenth and twentieth centuries. Differences from age to age and place to place shrink in comparison to what happened when Christianity was young. The subtle and not so subtle distinctions that are so important in history are ironed out. Origins are all; development and change very little. Readers of the Bulloughs earlier study on women in history will recall the same tendency. Women have always been the subordinate sex, and this single fact drowns the differences in the position of women in time and space.

A second difficulty lies in the very kind of history the Bulloughs write. Because they deal with attitudes rather than practices, one cannot help but wonder if their study has not confused "what is and what ought to be," to borrow Carl Degler's phrase. There are a growing number of studies that examine sexual practices by drawing on legal and church records. These studies show that under the blanket of two thousand years of purported sex negativism, there was remarkable variety. Studying the pronouncements of Church fathers and nineteenth-century doctors does not take us very far in penetrating into temporal and spatial distinctions. A study of these differences raises a whole host of questions barely touched by the Bulloughs. Explaining these differences takes us out of the world of ideas into the social, political, and economic permutations that account for sexual change.

In addition, concentration on attitude leads the Bulloughs to overemphasize the ambiguity of so-

domy and masturbation in the West. It is true that studying those few writers who commented on sodomy leads to the conclusion that there was no clear-cut view on precisely what these sinners did in their sexually deviant careers. A study of legal practice helps to lift the veil of obscurity. On masturbation it seems reasonable to assume that those medical men who not only wrote about but prescribed cures for the masturbator knew precisely what they meant by the *solitary* vice and *self-abuse*.

One should not be too hard on the Bulloughs for not writing a different book. This is a well-done and useful volume. My comments should serve as a reminder that there is more to the history of sexuality than attitude studies and a great deal more of largely unexplored richness and variety in the Western experience than the Bulloughs' study indicates.

ARTHUR N. GILBERT
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TAMARA K. HAREVEN, editor. *Transitions: The Family and the Life Course in Historical Perspective*. (Studies in Social Discontinuity.) New York: Academic Press. 1978. Pp. xx, 304. \$19.50.

Unfortunately, this book will not persuade anyone that, as one of the contributors claims, statistics "offer us a richer and more accurate understanding of the social history of the family than has hitherto been available" (p. 66). On the contrary and as the contributors admit, it presents us with conclusions already well known. The result of "workshops" directed by Tamara Hareven and sponsored by the Mathematics Social Science Board, *Transitions* is an expression of that modern movement aiming to make the study of human life appear scientific by presenting its evidence in numerical form.

The "exposure" of Hareven's groups to Glen Elder's notion of the "life course" revealed to them that the idea of family life as a series of stages did not do justice to change. While this might seem obvious to the uninitiated, Elder and his followers are not daunted and have managed to mystify it in deadly sociologese. Four of the chapters, on marriage, school attendance, women and work, and old age, address family "transitions," basing their conclusions on the United States 1880 census of eight Massachusetts towns. In other chapters, Elder provides the common theme, Peter Uhlenberg addresses the different experiences of birth cohorts since 1879, John Modell and Howard Chudacoff briefly describe the Essex county "setting," Modell and Hareven compare preceding chapters to Elder's notion, Stanley Engerman tells us how useful to history is the theory of "budget restraint," and Robert LeVine describes the "life course" of the Gusi of Kenya. The authors confess to the profound weakness of basing their analyses on a single date. This is

particularly ironical given their desire to look at change rather than "static snapshots" (p. xix). The best chapter is by Carl A. Kaestle and Maris Vinovskis on school attendance, in large part because it draws its evidence from two points in time.

The book's faults are characteristic of its genre. It assumes that statistical coincidences add up to historical explanation. Although it ignores or denigrates nonstatistical evidence, even calling it "gossip" at one point, *Transitions* depends on it. It accepts the functionalist view that the family is simply an agent of socialization and, as a corollary, it presents "social discontinuity" invidiously, as "deviant sequences" and "faulty socialization." With such a world-view, one wonders how social change can ever be understood. The same attitude fosters the invidious view of women one finds in much historical demography, where their only useful function is viewed as reproduction; here Uhlenberg writes of a "hardcore of 'unmarriageable' females" (p. 85). The statistical superstructure rests on a ludicrously simple idea of motive whereby people in the past calculate suspiciously like the historians describing them. "Individuals weigh the benefits of acting now rather than later by considering the nature of the present state as against the quality of the future state, discounted for the uncertainties of attaining the desired state in the future" (p. 247). Hareven and her co-authors choose "behavioral data" and largely ignore "phenomenological data" even though they recognize that this makes it impossible "to reconstruct the meaning to people" of "life course decisions" (p. 14). They cite each other and ignore the relevant work of other kinds of historians. They describe no individuals, even though they refer frequently to "individual decisions." Finally, the language in which they present their findings is crippling to their subjects as well as to themselves. It is a moot point whether it is better to neglect the lives of ordinary people as they were neglected before the advent of the "new social history" or to represent them, as these historians do, in the dehumanized form of income-consumption percentages.

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Daedalus: Journal of the American Academy of Arts and Sciences. Volume 107, number 4, *Generations*. Cambridge: American Academy of Arts and Sciences. 1978. Pp. vii, 207. \$4.00.

A generation—the ordinary period of time required for children to succeed parents—used to be counted as a third of a century or about thirty years. Now, although parents with children in college or gradu-

ate school may find it hard to believe, its length is supposed to be twenty years. In the future, given the precocity of youth and American preference for the new, the span of a generation may shrink to a decade. We already use the word generation not only to denote a period of years but also to characterize a cohort of people born around the same time. In one of the essays included in the twentieth anniversary issue of *Daedalus*, Noel Annan reminds us that it is the intelligentsia who define a generation as "lost" or "beat" and that they do so mainly in terms of their own ideas, tastes, and behavior.

The thirteen contributors to *Generations* were born between 1896 and 1948, all but four before 1930, and only two after World War II. The oldest, Douglas Bush, acknowledges that he feels "something of a stranger in the present world" (p. 165). Noel Annan, born in 1916, writes as a member of an aging generation, once the hunter of old fogies, now the prey of young achievers. On the other hand, Jonathan Lear, in an article on two generations of leading positivist philosophers, addresses himself to and seems to consider himself one of the current generation of young nonpositivist philosophers. Laura Nash (born in 1948), with Lear the youngest of the contributors, concludes her essay on Greek origins of generational thought with the observation that in an age when people refuse to have children and talk more about generations of computers than generations of mankind "generation has lost its reference point" (p. 19).

One of the advantages and intentions of collections like this is to acquaint readers with the concerns, methods, and vocabularies of students working in different disciplines but sharing a common allegiance to humanistic culture. Contributors to *Generations* represent the fields of history, anthropology, sociology, political science, literature, classics, and philosophy. The essays, grouped under the headings "The Idea of Generations," "Generations in Historical Perspective," and "Generations in Recent Scholarship," range from Harold R. Isaac's "Bringing Up the Father Question," described by the author as a "question-raising exercise" (p. 202), to Matilda White Riley's more formal and expository "Aging, Social Change, and the Power of Ideas." Historians will probably be most interested in three articles in the middle section, each of which includes the word "Reflections" in the title: Annan reflects on three generations of twentieth-century England, particularly the achievements and foibles of the interwar generation; Carl Schorske presents a concise and thoughtful summation of his studies of cultural innovation and generational tension in late nineteenth- and early twentieth-century Vienna; and Morton Keller, in a temperate, undogmatic way, explores the role of intragenerational similarity and intergenerational change and conflict

on American political development. "It may be," he states at the outset of his analysis, "that generations play in American politics a role not unlike that of classes in European political life" (p. 123).

The most reassuring note in the volume comes at the end of Shirley Robin Letwin's article on the treatment of generational differences in Trollope's novels. In Trollope's world "a gentleman is immune to the vulgar error of elevating every disagreement or attempt at innovation into a battle between the forces of good and evil. He assumes that a difference in judgement or taste can be indulged without making a revolution" (p. 70). Let's hope this applies to gentlemen, and ladies too, in our own world.

ROBERT H. BREMNER
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JOSÉ MARIA ALEGRE. *Las relaciones hispano-danesas en la primera mitad del siglo XVIII*. (Études Romanes de l'Université de Copenhague. Revue Romane, special issue 14.) Copenhagen: Akademisk Forlag. 1978. Pp. 534.

The title of this well-researched study is misleading in that the contents are almost exclusively concerned with a decade of diplomatic relations between Madrid and Copenhagen during the War of the Austrian Succession. José María Alegre, a professor in the Institute of Romanic Studies at the University of Copenhagen, has carefully sifted the relevant archival resources in Spain and Denmark to document a story of ambitious opportunism at the two courts during the 1740s.

After Denmark reopened its embassy in Madrid in 1740 its representatives were urged to seek three major commercial advantages: a greater freedom for Danish vessels in the Caribbean trade, a reduction of tariffs on its Baltic commodities in Iberian ports, and Spanish naval protection for Danish shipping against mounting losses to North African pirates. The court of Philip V at first welcomed the return of the Danish embassy out of its own strategic considerations. It was anxious to prevent Denmark from joining England in the year-old War of Jenkins's Ear against Spain in the Atlantic and the Caribbean, and, in keeping with its close collaboration in French diplomatic aims, Spain desired Denmark to be a buffer against English entry into the Baltic area. On the basis of this mutual interest, both courts seriously studied previous treaties with other powers to compose a draft text for final negotiation. Alegre presents a lengthy comparative analysis of the twelve treaties used by the Danes and the five preferred by the Spanish to establish their objectives.

Early in 1742, however, new and compelling reasons for a Spanish agreement to Danish commercial

ambitions emerged in the outcome of the first phase of the War of the Austrian Succession, as well as in the strong dynastic hopes of Elizabeth Farnese, the wife of the feeble Philip V. Fearing that Denmark might abandon the Bourbon courts and enter a partnership with their enemies (such as the Treaty of Breslau), both Paris and Madrid promised many commercial concessions. For Spain the price was ruinously high in that its historic Caribbean policy would be in jeopardy. Furthermore, the author has shown that Elizabeth Farnese, through her obsequious favorite José Campillo, drafted secret articles for Philip's signature in which even more would be conceded to the Danes. This sacrifice of Spanish overseas interests was planned so that the Danes would remain anti-Austrian during the war. With Maria Theresa's forces engaged elsewhere, Elizabeth planned that a Spanish army could fight more successfully in north Italy for the claims of her children. Without the knowledge or approval of the Spanish foreign secretary, Villarias, her diplomatic gamble was written into the Treaty of San Ildefonso of July 1742. The sudden death of Campillo in April 1743 ended this bizarre intrigue since Ensenada, Carvajal, and Wall, sensitive to Spain's long-term interests, delayed with excuses any actual observance of the treaty. Subsequently the two monarchies again drifted apart.

This meticulous study provides an unfamiliar perspective into the conduct of Spanish and Danish diplomacy in the mid-eighteenth century. The author promises a continuation of his work, which, while leaving unchanged the general outline of the international relations of the century, accords the lesser powers a new and overdue appraisal.

ALBERT J. LOOMIE
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NANCY NICHOLS BARKER. *The French Experience in Mexico, 1821-1861: A History of Constant Misunderstanding*. Chapel Hill: University of North Carolina Press. 1979. Pp. xv, 264. \$18.00.

Nancy Nichols Barker's *Distaff Diplomacy: The Empress Eugénie and the Foreign Policy of the Second Empire* and her two volumes of *The French Legation in Texas* have earned prestigious awards. The former work is already a part of the standard, classic literature on the reign of Napoleon III. It is therefore no reflection on the quality of the author's research or writing to say that her newest book is of comparatively little consequence. Franco-Mexican relations from 1821 to 1861 suffered from a lack of meaningful continuity. We are left therefore with a series of interesting narratives suitable for specialized journals but lacking the unity that justifies a book. Many of the most valuable contributions of this work have

already appeared in the author's journal articles on such French diplomats as Dubois de Saligny.

Students of the subject will, however, acknowledge their indebtedness to Barker for her original and sound research on the demography of the French in Mexico, 1821-61. She has made innovative and sound use of archival and newspaper sources to describe the class structure, economic behavior, and social attitudes of that population. We also learn much of the logistics of communication and travel. The author's archival research is impressive. It includes, besides the correspondence of the French and Mexican ministries of foreign affairs, the gold mine of materials in the archives of the Mexican embassy at Paris. Accompanying these rich collections are Austrian archival materials at Vienna and at Washington, as well as the personal papers and career dossiers of French and Mexican diplomats. British diplomatic correspondence, available on microfilm in this country, is also used. The only glaring gap in an otherwise complete research effort, is the absence of United States diplomatic correspondence. Why should we trust a British diplomat's quotation of an alleged report by a United States minister to the state department (p. 159)? The reader questions how a British diplomat got hold of an American dispatch that could only be damaging to American interests if revealed to the world. The author's bibliography of published secondary literature is unbalanced and highly selective. Important new research and classic works have been omitted, while peripherally important or irrelevant works are included.

A little more attention could have been given to organization. For example, Barker does an excellent job in relating developments throughout the world to French intervention or nonintervention in Mexico, but she sometimes fails to clarify their concurrent interrelationship with events in Mexico that are described fully on other pages.

At a petty level, the reader may become irritated by the practice of putting explanatory material into parentheses thrust into the text. This goes so far as to include translations of French phrases. Still, the author has a delightful sense of humor that makes her most parentheses-laden pages readable. We are left to assume, however, that she is pulling our leg when she states that prior to 1849 "Mexico had usually armed itself in Great Britain (from the Tower of London), buying . . . obsolete models and in small lots" (p. 120).

In addition to the original conclusions drawn from her previous books and journal articles, the most important contributions of this work concern French interest in Tehuantepec, the circumstances of the "Pastry War of 1838," and the evolution of schemes to enthrone various European princes in Mexico prior to the reign of Emperor Maximilian (1864-67).

The last short chapter entitled "In Retrospect" could have been omitted. It indulges in a great deal of sociological speculation concerning the racist motives of nineteenth-century Frenchmen, which are relevant only in twentieth-century terms. The utterly mistaken views of Karl Marx are also explored, though he had no access to the truth about Franco-Mexican relations and drew false conclusions from the little that he did know. Why was he not consigned to a footnote? With allowance for flights of poetic license, it stretches things a bit to describe Emperor Maximilian as "the last of the *conquistadores*" while neglecting to describe the firm and courageous way in which he blocked French schemes to establish a protectorate over Sonora. A book covering the years 1821-61 might better have avoided the subject.

ARNOLD BLUMBERG
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FRITZ VAN BRIESSEN. *Grundzüge der deutsch-chinesischen Beziehungen*. (Grundzüge, number 32.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1977. Pp. xi, 206.

Fritz van Briessen's short book outlines German-Chinese relations from the seventeenth century to the present, paying primary attention to diplomatic contacts in the late nineteenth and twentieth centuries. Van Briessen, for many years a journalist in the Far East, later an official of the West German embassy in Tokyo, and an author of popular books on oriental art, displays the effects of his background in the presentation of his subject. He assumes the primacy of *Aussenpolitik* and connects the formulation of foreign policies to the nature of countries' cultural development and interaction in the traditional style of German diplomacy and historiography. This gives van Briessen's analysis a somewhat old-fashioned sound from the standpoint of recent historians' concentration on the internal political roots of foreign policy. Thus, for example, van Briessen sees the ending of the German imperial presence in China at Versailles in 1919 as a "crystallization point" for the process that led to the radical political changes in China during the following decades. His journalistic background is evident in his tendency to emphasize the formal, publicized events of Sino-German affairs rather than long-term processes of social and economic change, which he treats only cursorily. Certain events of particular interest to a German "old China hand" but of limited historical significance, such as the "war crimes" trial of German agents in China in 1946-47, are given more attention than is probably necessary in so short a book.

The book's main strength is the way it ties together material from disparate sources on a rela-

tionship of secondary importance to both China and Germany. It is highly readable, if occasionally repetitive, and it contains excellent references to the limited published work available on various aspects of the subject. Van Briessen handles the events leading to the seizure of Kiaochow in 1898 and the operations of German "advisers" in China in the 1880s and 1930s particularly well. He has succeeded in producing an overview for the nonspecialist that is both useful and interesting.

WOODRUFF SMITH
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JOSEPH SMITH. *Illusions of Conflict: Anglo-American Diplomacy toward Latin America, 1865-1896*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1979. Pp. xv, 276. \$16.95.

This work on the Latin American diplomacy of the United States and Britain during the post-Civil War era is part of the current Latin American series of the University of Pittsburgh. The first two chapters are devoted to generalized discussions of British and American policy in the nineteenth century. Joseph Smith emphasizes Britain's peripheral interest in Latin America in contrast to America's drive for pre-eminence in the area, which did not disturb Britain unless its commercial interests were threatened. Thus, Britain (despite American suspicions to the contrary) 'was little more than an observer during the War of the Pacific and the other armed encounters of the period. It was increasingly content to stand aside and let the United States attempt to keep peace in the area.

The author next turns to the various isthmian canal projects. Here Britain's interest was occasionally stimulated by events, especially in 1882 when America charged that Britain had violated the Clayton-Bulwer Treaty on a number of occasions and sought to exclude Panama from the agreement. Quiet British diplomacy, as well as American political bickering, combined to permit Britain to maintain its rights intact at this time.

An interesting chapter traces the American drive to enlarge Latin American trade, culminating in the cleverly devised free list of the McKinley Tariff, which forced some Latin American states to make reciprocity treaties with the United States. Various factors caused the failure of the scheme, but not before America's increasingly intimate relations with Brazil had caused some consternation in London.

The writer then takes up American diplomacy in Argentina during the financial crisis caused by the collapse of Baring Brothers and the Chilean Revolution of 1891-92. Although America by no means won the friendship of these states, its political influence south of the border was obviously growing

apace. That influence, as well as the status of the Monroe Doctrine in international affairs, was confirmed by the Venezuela boundary dispute when Britain accepted an American demand to submit the question to arbitration. This incident, the author rightly observes, merely involved a public acknowledgment of a British policy long in gestation.

References to the private correspondence of the British statesmen of the period, which often reveal nuances of British diplomacy missing from the official documents subject to the scrutiny of the general public, would probably have strengthened the work in places. The author nevertheless effectively demonstrates the baselessness of most of the American suspicions and fears of Britain in Latin America, and his book provides a splendid introduction to a recent study by Warren G. Kneer, who takes up the Anglo-American story in 1901.

WILBUR DEVEREUX JONES
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WALTER ULLMANN. *The United States in Prague, 1945-1948*. (East European Monographs, number 36.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. x, 205. \$13.00.

The establishment of a new international political order in Europe in the 1940s will provoke debate among historians so long as they care to discuss the topic. That radical revision of power relationships has already proved to be relatively durable. Events of such magnitude do not promote the establishment of an easy orthodoxy of historical interpretation, and it is not only the grand view that is debatable. Particularist memories of the struggles in individual countries still arouse passions. Czechoslovakia is no exception. On the contrary, precisely because both sides in the contest hoped at the time that Czechoslovakia might prove different from the general pattern of events, the Czechoslovak case has remained especially controversial.

A new book based on newly available information must therefore be a welcome addition to historical literature. Walter Ullmann's study of American policy toward Czechoslovakia exploits the diplomatic archives in Washington, and that is its main value. The heavy reliance on this one source, however, is also the major weakness of the book, for the resulting narrowness of view sharply limits the potential impact of the monograph.

The material that Ullmann has gleaned from state department records contains no revelation radically different from the general views that have been aired previously. But it is intriguing to see policies spelled out in the words of the actors themselves, and the book contains candid glimpses of some of the major personalities in the struggle. The

central figure is American Ambassador Lawrence Steinhardt, whose reports supply most of Ullmann's information. Steinhardt's confidants such as Hubert Ripka, Jan Masaryk, and Eduard Beneš are the major figures on the Czech side. Steinhardt and his Czech associates alike were given to wishful thinking, as their persistent and mistaken optimism about the ability to contain Czechoslovak communism demonstrated. Given Steinhardt's reports, American policy makers in Washington must have been as surprised as were the democratic leaders in Czechoslovakia by the overwhelming Communist victories in the May 1946 elections.

Some historians have argued that the Czech Communists staged their coup in February 1948 because they saw that their electoral support was declining and that they would suffer losses in elections scheduled for the spring. It is worth noting that in his reports to Washington Steinhardt perceived no such decline and that the British Foreign Service, as well, could find little change in Communist strength. Ullmann also supplies other useful information on topics such as the debate within Czechoslovakia concerning the offer of Marshall Plan aid and the political struggles in Slovakia between the Slovak democrats and the Communists.

On the other hand, the major weakness of the book is that, despite his meticulous attention to detail, Ullmann's ultimate conclusions are frequently unsupported by hard analysis. Instead, his judgments seem to be influenced more by a desire to defend Czechoslovak policy and criticize American decisions wherever possible. He is impatient with the American lack of enthusiasm for the transfer of the Sudeten Germans. He thinks that the Americans were far too hard-nosed in their reaction to the Czechoslovak refusal to participate in the Marshall Plan. He disparages "Washington's political acumen" for allowing "such a prestigious position [as Bohemia] to fall into Soviet hands" (p. 137), yet three pages later he criticizes American policy makers for failing sufficiently to draw distinctions between Czechoslovak Communists and the Soviet Union. Ullmann criticizes American reaction to the February coup for failing to reassure the "Czechoslovak opposition about America's intention to restore democracy to their country" (p. 157). Just exactly how such a restoration could have been accomplished or what such an attempt would have meant for the international situation in 1948, Ullmann does not bother to explore. Instead, Ullmann relies simply and repeatedly on rhetorical phrases such as the need for "stronger medicine" (p. 154) and "a bolder defense of mutual hopes and aspirations" (p. 171).

The other main weakness of the book is the narrowness of its scope. Ullmann tends to treat American policy toward Czechoslovakia almost as if it were independent of all other considerations. For

example, in the first chapter Ullmann discusses the failure of the American army to occupy Prague in the spring of 1945 while barely mentioning the military situation in Germany and Austria or the broader military and strategic considerations that would logically have contributed to the decision. And the book is narrow in scope also in relation to the existing historiography of the outbreak of the Cold War. Certainly the Czechoslovak experience was a crucial part of the process by which the Grand Alliance broke into two competing alliance systems, yet little effort is made to draw connections between events in Czechoslovakia and the broader international political situation. In short, whereas the book does provide knowledge about American policy toward Czechoslovakia, the significance of that knowledge still requires explanation.

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University of Chicago

ANCIENT

R. F. WILLETTTS. *The Civilization of Ancient Crete*. Berkeley and Los Angeles: University of California Press. 1977. Pp. 279. \$22.75.

The restoration of Crete to its proper place in the history of Greece has been one of the central achievements of modern classical scholarship. R. F. Willetts has contributed significantly to this process in the past, and in this, his sixth book on ancient Crete, he has attempted to provide scholars and the general public with a comprehensive and up-to-date account of ancient Cretan civilization.

Willetts views Cretan civilization as the result of a continuous process of adapting foreign influences to local needs by the inhabitants of the island. Accordingly, three introductory chapters survey the geography of Crete, its relations with the other cultures of the eastern Mediterranean, and the contributions of the first settlers to the development of a distinctively Cretan civilization. The remaining thirteen chapters then treat that civilization in the bronze and iron ages. Relatively little space is devoted to the narrative of events. The focus instead is on the socioeconomic components of Cretan civilization and its cultural manifestations. Reflecting the differing natures of the source materials, archaeological for the bronze age and literary and epigraphical for the iron age, the section dealing with the former concentrates on the great Minoan palaces while that on the latter emphasizes social organization, law, and government. An appendix contains a translation of the Gortyn Code.

There is much to praise in *The Civilization of Ancient Crete*. Its author is thoroughly familiar with Crete itself, the ancient sources, and the voluminous

modern scholarly literature. His descriptions of archaeological sites are clear and his analyses of the relations between Cretan culture and its environment are often illuminating. Finally, every student of Greek history must be grateful to him for not only including a translation of the Gortyn Code in his book but also for making that difficult but important document accessible through a lucid analysis of its contents in chapters twelve and thirteen. Despite these virtues, however, there are serious methodological and theoretical flaws in the work that prevent it from being the sound *vademecum* to ancient Crete its author intended.

The first of these flaws is a surprisingly naive faith in the basic historicity of myth and the ability of archeology to validate that historicity. So we find, for example, the myth of the rape of Europa by Zeus cited as evidence for relations between Crete and Phoenicia and the Homeric characterization of Minos as *enneoros* as evidence for the theory that the Minoan Minos was a priestly figure who held his position for renewable eight-year terms. Such treatment of myth smacks of the nineteenth century, and, therefore, we are not surprised to find his analysis of the development of Cretan civilization dominated by the concepts of Victorian evolutionary thought. A variety of sources—the Messara tombs, neolithic figurines, the prominence of female figures in Minoan art and cult, and the lack of defenses and military themes in Minoan art—are adduced to produce the familiar picture of a peaceful matriarchal Minoan culture followed by a militaristic patriarchal Greek domination without any reference by the author to the virtual abandonment of the concept of matriarchy by modern anthropology or the revisions in the theory of a pacific Minoan culture that the military frescoes from Thera seem to require. These flaws seriously weaken Willetts's interpretation of Minoan civilization and limit his book's potential value to both scholars and the general public. A reliable and up-to-date survey of ancient Cretan civilization, therefore, unfortunately still remains a desideratum.

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ELEANOR GOLTZ HUZAR. *Mark Antony: A Biography*. Minneapolis: University of Minnesota Press. 1978. Pp. ix, 347. \$20.00.

This book provides a narrative account of the political events during Antony's lifetime. It does not offer a broad reinterpretation of his public career or new insights into the various scholarly conundrums posed by the period.

The footnotes contain few modern references, a lack that may explain evidence in the text that

Eleanor Goltz Huzar has not fully absorbed some of the major, recent contributions to our understanding of Roman politics. For example, she depicts the Senate as consisting of many "factiones" (in spite of Seager, *JRS*, 62 [1972]: 53–58); she refers to "political parties" and "party lines," using the anachronistic terms "liberal," "conservative," and "radical"; she believes the equestrians were the "commercial classes" (apparently unacquainted with C. Nicolet, *L'Odre Equestre*) and, finally, confuses nobles with patricians when referring to the Gracchi as "patrician brothers who became tribunes" (p. 8).

There is a serious inconsistency in her attitude to the sources. For thirteen chapters she uses them uncritically as the basis of her narrative, but the fourteenth consists of a brief *quellenkritik* that suggests they are unreliable, products of a hostile Cicero and a propagandistic Augustan literary tradition, and therefore unlikely to give Antony a fair shake. This chapter is unfortunately bound to read as a disconcerting afterthought, for its conclusions have not in any way affected Huzar's approach to her sources in the main part of the book; instead of treating them as possibly tendentious or salacious propaganda, she assumes their accuracy. Accordingly, we read that Antony had "an inborn inclination for a carouse" (p. 24) so that his inability to resist a wild party not only damaged his reputation but also diminished his capacity to conduct crucial affairs the morning after; that early in his career he amassed a gambling debt to the tune of \$300,000 (*sic*) and purchased two beautiful serving boys for \$10,000 (*sic*); that he was politically inept, an incompetent domestic administrator, incapable of formulating long-range policies; and that he was a hopeless womanizer.

This is the picture before the *quellenkritik*. After it, Huzar tells us that Antony's political successes were considerable and must have depended on genuine practical virtues; that even his final failure was the product of virtue, of the moral inhibition that prevented him from being as "ruthless as the triumphant Augustus" and that he was a man who "relished being in love" (p. 254). The tone here belongs more to a whitewash than to an apologia. The fact remains that he authorized the grisly murder of Cicero and that he was, by Huzar's own admission, most responsible for the proscription of three hundred senators and two thousand equestrians. With Antony's blessing, every free man was promised one hundred thousand sesterces, every slave forty thousand and his freedom, when he presented the head of one of the twenty-three hundred proscribed to Antony or either of his colleagues, Octavian or Lepidus. Not even Huzar's explanation that Antony was simply "the product of a century of civil wars" (p. 254) can excuse that.

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RONALD SYME. *History in Ovid*. Oxford: Clarendon Press. 1978. Pp. 240. \$26.00.

Finding history in Ovid is a task to daunt lesser men. Poetic responsibilities are rich visions and excellent style, not sober, unexaggerated historical truth; and the Roman elegiac poets wrote of love and individualism with only incidental references to the world beyond their passions. But Ronald Syme draws on his redoubtable learning—and known convictions—to present Ovid as poet and citizen of Rome's empire in the later Augustan age.

Collecting all of the identifiable events and individuals, Syme compares Ovid's evidence with that of other poets, historians, and inscriptions and with prosopographical material to enlarge or correct our vision of Ovid's world. Triumphs, festivals, and construction of public buildings provide a close chronology for Ovid's life, poems, editions, even the sequence of poems within the *Tristia* and *Epistulae ex Ponte*. The evidence also argues for otherwise unattested matters of state, such as a German campaign and triumph for Tiberius in A.D. 13. Syme dares to trust Ovid even over Cassius Dio (lacunae in his text distort his record), Velleius Paterculus (devotion to Tiberius kept him silent about the successes of others), or Tacitus (his study of the Augustan age was superficial).

Individuals in the poems elicit Syme's formidable prosopographical skills. Ovid moved in many circles: his companionable *sodales*, poets like Propertius, "barbarian" friends of his exile. But when soliciting appeals to Augustus against his exile, Ovid named twenty-one leading *nobiles* in his *Epistulae ex Ponte*. Syme exploits these references to identify the individuals and determine who was in favor at a specific time during the reigns of Augustus and Tiberius. For Syme, who can affirm that "the central theme in the history of Rome is the enlargement through the ages of the senatorial order" (p. 95), Ovid's friends in the Senate, their places of origin, marriages, and political powers, are of central importance. Syme concludes that Ovid's ineffectual appeals even to friends of long standing show the opportunism and subservience of the Augustan *nobiles*.

The portrayal of Ovid's age (though only incidentally of Ovid) describes governmental patronage of and restraints on writers, as well as Augustus's moral and sumptuary legislation. Refuting other Ovidian scholars, Syme tries to clarify the vague charge of *carmen et error* (*Tristia*, 2.207) for which Ovid was relegated to Tomis. The improprieties of the *Ars Amatoria* were only public justification for the severity with which Augustus punished Ovid, who was unluckily present in a compromising situation that must have represented a political threat to Augustus. As in previous books,

Syme judges Augustus harsh and oppressive and rejoices that victory now rests with Augustus's victim, Ovid.

Regrettably, the book has deficiencies. Even thorough hunting and vigorous worrying of the few findings cannot compensate for the meager historical content in Ovid. Around the slight evidence Syme has drawn together chapters on the Augustan age that he acknowledges in the preface were written at different times and for different purposes. The integration is only partial. Moreover, Syme's Tacitean prose style remains cumbersome and distracting, an obstacle course to overcome. Notwithstanding, the book is valuable as quintessential Syme: profound erudition, fresh insights, and rich appreciation of the poet and the age.

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ROBERT BROWNING. *The Emperor Julian*. Reprint. Berkeley and Los Angeles: University of California Press. 1978. Pp. xii, 256. Paper \$4.95.

Late antiquity receives welcome scholarly attention in this superstitious age of ours, and Robert Browning is now having his say. This biography of the Emperor Julian is arranged in chronological, not topical, order. An introduction caricatures (as such chapters must, a colleague of mine insists) the age and its antecedents while a useful epilogue surveys the influence of Julian's life and personality on successive generations from his death until 1962, when Gore Vidal published his novel *Julian*. (Ironically Browning omits the strong fascination Julian had for the imagination of another backward-looking Hellene, C. P. Cavafy.) There are also a note on sources (a short list of materials in translation), suggestions for further reading (annotated for the general reader), Julian's family tree, table of dates, maps, and illustrations.

For Browning, Julian emerges as the protagonist of a Greek tragedy brooding over his identity and his condition, not always understanding his choices or their implications until too late. Shaping his account thus allows Browning to speculate about his subject's attitudes when the evidence is wanting, most noticeably during the youth's exile in Cappadocia. Occasionally, Browning reduces these silences to the trivial. When Julian, in exile, is haunted by the murder of his father and brother, he observes, "Whether these moments of intense emotion were worse than the long months of inescapable boredom only Julian himself could tell us, and he does not" (p. 43).

The story sometimes labors under its too linear narrative; issues must be discussed as the subject

meets them. The author's sense of Julian's development is thereby aided but not the reader's overview of relevant problems. Christianity, obviously important in the life of a man nicknamed the Apostate, gets fuzzy handling in this way. It is discussed throughout the book, and there is a major chapter on "Julian and the Christians" treating his attitudes and policies after accession. But there is a difficulty. In his introduction Browning surveys nearly three hundred years of Christian history and emphasizes its nonpolitical aspects. He overlooks theoretical considerations for persecuting the Christians on political grounds. To examine the Roman case, Browning should have explored the three ideas of kingship (divine, dynastic, Roman imperial) prevalent in the Palestine that invented Christianity. These were real issues for the Jews of the age and obviously affected the nascent Christian community. Taken with the political and military metaphors of early Christian writing, these ideas informed a tradition that was eventually abused to justify the atrocities of Christian emperors in the fourth century. Julian knew a great deal, as Browning insists, about Christian inconsistencies and Roman history; the reader should be allowed to explore this range also.

What he ignores is a strong intellectual tradition linking some attitudes of the early Christians, those of the Stoic opposition of the first century, the metaphors of the increasingly Stoicized Christian writers of the second and third centuries to fourth-century Christian and Cynic attitudes about defective monarchy or tyranny. This would have clarified how Christianity was "politicized" by Constantine (otherwise vague to the reader) and why contemporary Christians persecuted fellow Christians and pagans alike. (In general, the institutionalization of religious persecution as an ordinary device of political repression deserves more attention from the historians of this period.) My objection is that, although Browning deals with these subjects, the conflicts are not made precise in his treatment.

In short, Browning here shows the merits and defects of others of his recent books. He knows the sources well enough and generously states, as often as necessary, where the evidence fails us. We only wish that he had left us more markers to follow; footnotes would correct an impression of source mining. This volume meets the general reader's needs, not the scholar's.

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Cambridge University Press. 1978. Pp. xiv, 268. \$22.50.

This stimulating and uneven book takes its title from the first of its five studies. The next two also concern slavery ("The Growth and Practice of Slavery in Roman Times" and "Between Slavery and Freedom: On Freeing Slaves at Delphi"), but the last two ("The Political Power of Eunuchs" and "Divine Emperors or the Symbolic Unity of the Roman Empire") relate to the others only in a concern for sociological features of their subject matter.

Keith Hopkins puts new questions to familiar material and seeks evidence for plausible answers. Every textbook of Roman history describes the growth of large estates and the importation of slave labor that marked the end of Italian small freehold agriculture in the later Republic. In his first study, subtitled "The Impact of Conquering an Empire on the Political Economy of Italy," he attributes this development to the continuous conscription of peasants for overseas conquest. Analyzing the complex economic factors and political issues involved, he shows why the importation of slaves relieved certain social conflicts and how Julius Caesar and Augustus brought a final resolution by the massive resettlement of Roman colonists in the provinces. The discussion is heavily indebted to Brunt's demographic studies, with many speculative assumptions, usually well marked, and with detailed examination of particular developments. An essay on "structural differentiation" in education, the army, and the law ends this section with a wide-ranging account of what "change" meant in the late Republic.

The second study focuses on the puzzle of widespread manumission. Why did an owner relinquish the wealth represented by a slave? Examining the conditions imposed on manumission, Hopkins concludes that in fact it brought economic advantages as well as personal and social rewards. The records of slave manumissions at Delphi provide the third study with a mass of precise data but without a social context: dated statistics of price, age, and sex are here more sharply defined, but Hopkins's insistent questions about motive and status and relationship can be answered only with a "possibly" or "perhaps." In contrast, the study of the power of eunuchs in the Eastern imperial bureaucracy is entirely one of context. An illuminating comparison with the position of the Court Jew in German states of the seventeenth and eighteenth centuries suggests one reason for influence that Hopkins hardly mentions: the absence of all hope of succession.

The infectious and occasionally ingenuous enthusiasm that enlivens the earlier studies emphasizes the inadequacy of the final one. That the Romans took emperor cult seriously, that it had sociological consequences, that there were parallels to Christian

KEITH HOPKINS. *Conquerors and Slaves*. (Sociological Studies in Roman History, number 1.) New York:

attitudes and language, and that the supernatural loomed large in Greco-Roman life—these have been matters of intense research. The chapter in the *Cambridge Ancient History*, volume 10, on religious developments covered this same ground years ago with fuller and more sensitive discussion even of the sociological repercussions. Both the approach and the bibliographical knowledge in this final study are narrow and out of date. It mars a spirited and informative book.

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MEDIEVAL

NORMAN DANIEL. *The Arabs and Medieval Europe*. (Arab Background Series.) 2d ed. New York: Longman, and Librairie du Liban, Beirut. 1979. Pp. xiv, 395, £9.50.

This is the second edition of an important book that examines the relations between the Arabs and Latin Europe during the Middle Ages. Its author, who has written extensively on this general subject for the last few years, here sums up his views as to how the Latin West approached and dealt with the Muslim world during some six medieval centuries. Norman Daniel begins with a general sketch of the state of the Islamic and Latin worlds in the eighth century. Then he considers a number of topics, beginning with ninth-century Spain at the time of the "Martyrs of Cordova" and continuing on with an examination of the tenth century in the western and central Mediterranean, the origins of the Crusades in the late eleventh century, Italy and the Kingdom of Two Sicilies from the eleventh to the thirteenth century, and the crusading states of Syria and Palestine down to 1291. His last three chapters are concerned with Christian religious conflicts with Islam, the reception of Arab science in the West, and the last years of the Middle Ages, which saw the deepening of the conflict between these two civilizations.

The author's approach is to examine all available contemporary Latin sources, concentrate upon them, and confine himself to areas like Spain, Sicily, Syria, and Palestine, where the drama of confrontation was played out during these centuries. He is to be congratulated upon the depth of his learning and the completeness of his coverage of relevant Latin Western source material. What emerges from his study is a view of the Latin West that emphasizes its unwillingness and inability to come to terms in any meaningful way with either Islamic civilization or Muslims during these medieval centuries—on whatever level one chooses to examine the confrontation or in whatever part of the Mediterranean one wishes to consider. He is espe-

cially critical of the way in which rulers of Sicily like Frederick II approached the Islamic world, and he doubts that Christian Spain was much more enlightened in coming to terms with its Muslim brethren and their culture—despite much that has been written to the contrary.

Valuable as all of this is, one cannot but be struck by the one-sidedness of the evidence Daniel presents to us, for despite his all-inclusive title he gives us only Latin European views and actions and never those of their Islamic counterparts. Save for a few twelfth-century Syrian Arab writers, we are never told of how historians like Ibn Al-Athir, Nuwairi, Juvaini, or Raschid-al-Din viewed the Franks; Ibn Khaldun's famous remarks on Christianity are not even mentioned. When we get to the later Middle Ages nothing is said of Mamluk historians like Ibn Taghri Birdi, who had much to say concerning Latin Westerners.

It is possible, of course, that if one included the views of such historians, one would not change the overall picture that the author has given us, but how can one be sure? Confrontation between civilizations is a two-way affair, and, excellent as Daniel's presentation of the Latin West is, the other side needs to be weighed or his title should be changed to *Medieval Latin Europe's View of and Dealings with the Arabs*.

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LESTER K. LITTLE. *Religious Poverty and the Profit Economy in Medieval Europe*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. xi, 267. \$27.50.

Lester K. Little's "broad, interpretative essay" is an account of the role played by voluntary poverty in the new profit economy of Western Europe between the eleventh and the fourteenth centuries. His particular interest is in the ways by which ideas of voluntary poverty helped shape a kind of spiritual life that accommodated the merchants, town dwellers, and clerics whom the new economy brought into prominence. The book is a synthesis of much of the most interesting and important recent medieval scholarship on theories of society, economics, morality, and lay and clerical spirituality, a good deal of which is by Little himself.

Little begins (part one, "The Spiritual Crisis of Medieval Urban Culture") by considering the cultural consequences of the growth of a profit economy in terms of Georges Duby's model of the shift from a gift economy to a profit economy. The immense changes and challenges to the aristocratic-monastic culture of early medieval Europe that even the small-scale urbanism and commerce of the

twelfth century caused shaped new attitudes toward wealth, money, and poverty and lay behind the deterioration of Jewish-Christian relations that so indelibly marks the late eleventh and twelfth centuries. "The guardians of conscience," however, monastic representatives of the aristocracy whose spirituality and view of the lay world was closely tied to aristocratic values, "were not prepared with an ideology or an ethic favorable to urban life" (p. 35). To them, the city was still the legacy of Cain and the home of avarice, a term that, as Little shows, became a blanket condemnation of the shift from a rural gift-aristocratic culture to an urban exchange-profit-commercial-civic culture, particularly in northern Italy, France, and the lower Rhine area in the late twelfth and thirteenth centuries. The tone of these condemnations seems anticipatory of some of the criticisms of early nineteenth-century England in the work of William Cobbett and Sharon Turner.

Some representatives of the older cultural values (part two, "Avoiding the Crisis: Monks and Hermits") made what Janet Nelson has elsewhere called "a heavy investment of religious capital" in old forms—traditional monasticism, purged and once again withdrawn from the world, as at Chartreux and Citeaux. It is useful to compare Little's treatment of this topic with that of Alexander Murray (*Reason and Society in the Middle Ages* [1978], pp. 314–404). Others, however, (part three, "Confronting the Crisis: Canons, Laymen, and Friars") faced the cultural implications of the twelfth century head-on. Little's remarks here, particularly on dissidents, heretics, and the mendicant orders, illustrate the process by which voluntary poverty in the face of rising wealth became the chief element in that confrontation. His focus on spirituality builds solidly on the earlier work of Herbert Grundmann and M.-D. Chenu and avoids the usual pitfalls of discussing spiritual reform in materialistic terms.

Part Four ("The Formation of an Urban Spirituality") emphasizes the role played by scholastic social thought, developed independently of the religious movements Little has considered so far, but later made their own by the Franciscans and Dominicans in their "reformed apostolate" to the towns. The chief elements of this reformed apostolate were preaching, a new emphasis upon the psychological dimensions of penitence, popularizations of the new learning in handbooks for confessors and preachers, and the orders' support of lay piety. The mendicant orders, with their response to the urban demand for talk and persuasion, their spiritual guidance for people in diversified and particular walks of life, their recognition of the urban sense of fraternity, and their cults of urban saints, combined to create an appropriate spirituality for the new life in the towns and the new lay intellectual and mate-

rial culture. Little's book describes how one dimension of thirteenth-century civic culture—the creation of an adequate and appropriate spirituality for townspeople—came to be the great spiritual and social achievement of the mendicant orders.

There are a number of editorial and typographical errors, and even the pared-down bibliography in the notes suffers from the absence of such names as Robert Bultot and John Mundy. But these are minor objections. Somewhat more serious is the argument that Little should have had more space and leisure to develop the argument more fully. As it stands, the book may put off some readers a little by the sharpness and categorical character of many of its statements. But this quality is the result of the book's brevity, not its scholarship or intellectual rigor. This work should speak to more historians than medievalists alone and should take a distinguished place beside the earlier studies on similar subjects by Benjamin Nelson, Raymond de Roover, John Baldwin, Jacques LeGoff, Pierre Michaud-Quantin, Duby, and Chenu.

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JAROSLAV PELIKAN. *The Christian Tradition: A History of the Development of Doctrine*. Volume 3, *The Growth of Medieval Theology (600–1300)*. Chicago: University of Chicago Press. 1978. Pp. xxvii, 333. \$17.50.

In this third volume of his five-volume history of Christian doctrine Jaroslav Pelikan provides students of medieval intellectual history and the history of dogma with an unusually valuable supplement to and reworking of the classics in this field. Many of these older works traced the evolution of medieval Christian doctrine through a series of major theological controversies and the ideas of key ecclesiastical figures. Pelikan takes an alternate approach by examining major doctrinal *loci* found in the works of both the stars and lesser luminaries in the medieval theological galaxy. What is especially impressive about this synthesis is that it is based not so much on secondary literature (which the author clearly knows) but on a fresh and perceptive reading of a vast number of primary sources ranging from the standard theological treatises to liturgical commentaries, canon law, and vernacular literature.

The volume is arranged chronologically, but each chapter, given a thematic title, may describe doctrinal evolution across several centuries, because theological topics are treated in the century in which they reached their consummate stage of development. Thus, medieval doctrines of redemption are clustered in the chapter discussing Anselm and Abelard.

Chapter one, on the seventh and eighth centuries, examines the early medieval elaboration of the consensus of faith received from patristic antiquity, especially from the credal statements and Augustine. The doctrinally rich ninth century, treated in the second chapter, is considered as a period of reworking the Augustinian synthesis; not only are the well-known controversies over adoptionism, predestination, and the eucharist described but also trinitarian doctrine and the beginnings of Mariology. The "christocentric period of Western Civilization" from 900–1100 provides the focus for the third chapter, which examines medieval atonement theory on the basis of its classic statements in Anselm, Abelard, and Bernard together with an unexpected mélange of sources ranging from Odo of Cluny to the *Dream of the Rood*, *Victimae paschali*, and Cynewulf's *Christ*. Chapter four, the most brilliant of the volume, deals with the way in which medieval theologians and liturgists described the multiple channels of grace in Mary, the saints, the eucharist, and other sacraments. During the twelfth century, treated in chapter five, the problem of authority became a crucial concern in Christian doctrine and was manifest in a multiplicity of issues—the relative values given to Scripture and the Fathers, the Church's response to popular and doctrinal heresies, the encounters of the Christian faith with Muslim and Jewish systems, and the relationship between faith and reason. The final chapter of the volume describes the thirteenth century as a period not so much as one of growth of Christian doctrine but as one of consolidation, integration, and summarization of the Catholic tradition. Aspects of the doctrines of predestination, Trinity, and the *filioque* were reopened and examined with the new tools of scholastic method, and there was a systematization of aspects of angelology, ecclesiology, and eschatology.

To criticize this volume for its lacunae and slighting of various topics will clearly be a temptation for any reviewer. There will undoubtedly be grumblings that this or that specialization of the reviewer has not been accorded its rightful amount of space or has been misplaced in another volume. One thinks, for example, of the paucity of "liturgical theology" drawn from the liturgical formulas themselves or of a periodization of medieval doctrine that ends with the thirteenth century. But, whatever the lacunae, they are more than amply compensated for by the scope of the book and by its treatment of aspects of medieval doctrine rarely touched upon in the standard handbooks. Among these might be mentioned the origins of medieval Mariology in the ninth century, the "leap theory" of redemption, the place of the saints in Christian prayer, and Muslims and Jews in medieval Christian doctrine. All of these forays into topics rare in

general medieval intellectual histories and histories of doctrine would have been impossible without an intimate acquaintance with a great diversity of theological sources and the ability to see connections where few have been noticed before. Pelikan's volume—clearly to be numbered among the classic surveys of medieval doctrine—is an invitation to students to read anew the primary sources of medieval theology and to be sensitive to the subtle and related patterns in the intricate web of medieval doctrine.

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PAUL SPECK. *Kaiser Konstantin VI. Die Legitimation einer fremden und der Versuch einer eigenen Herrschaft: Quellenkritische Darstellung von 25 Jahren byzantinischer Geschichte nach dem ersten Ikonoklasmus*. Volume 1, *Untersuchung*; volume 2, *Anmerkungen und Register*. Munich: Wilhelm Fink Verlag. 1978. Pp. 419, 424–857. DM 48; DM 48.

The reader's initial reaction to this work is to ask how it is possible for anyone to write more than eight hundred pages about the very poorly documented reign of Constantine VI. Paul Speck, however, succeeds in accomplishing what he has set out to do, that is, he provides the most thorough study of the little-studied Constantine VI that has ever been attempted. In fact, very little has ever been written on Constantine VI, and there is no other work on his reign. Speck's study should remain standard for a long time even though he cannot give definitive conclusions to all of the historical questions he discusses. This work, Speck's third, takes a historical approach, while his earlier books were really works of textual edition, criticism, and literary history. He has discovered no important new sources but has attentively and critically read the existing ones, except for some untranslated Arabic materials, and he knows the modern scholarship quite well.

The first volume contains the text and the second appendixes and copious footnotes that discuss and explore many technical questions. Speck analyzes the entire late eighth-century reign, taking up problems in chronological order and concentrating on political, military, and religious history. The work's greatest value lies in his probing analysis of the internal history of the reign. He offers many hypotheses, not all of which can be proven; but his hypotheses are reasonable, even if they will not satisfy everyone. He has made a fundamental contribution to the understanding of iconoclasm, and he correctly concludes that it is impossible and indeed incorrect to attribute any monolithic social basis to it.

He provides almost no discussion of institutional, economic, or cultural topics, but the sources do not permit discussion of them. The work is dry reading, a serious and sober piece of research that probably will not have a wide audience. Medievalists will find that Speck does make useful observations about Carolingian and papal relations with Byzantium. But his discussion of Byzantine-Arab relations suffers from a lack of knowledge of the Arabic materials, including the recently published history of Ibn A'tham.

In general, Speck demonstrates a judicious weighing of the extant sparse evidence on many topics of internal history. His study will be an indispensable tool for specialists because of his thorough familiarity with the sources and his long contemplation of many difficult questions of the late eighth century. The appendixes and indexes are detailed. A *Habilitationsschrift*, it is another noteworthy accomplishment of Byzantine studies at the University of Munich.

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SIGFÚS BLÖNDAL. *The Varangians of Byzantium: An Aspect of Byzantine Military History*. Translated, revised, and rewritten by BENEDIKT S. BENEDIKZ. New York: Cambridge University Press. 1978. Pp. xiii, 242. \$35.00.

The strange story of the Varangian corps—the body of Viking mercenaries who served as the life guard of the Byzantine emperors—has always had a special attraction for the English-speaking and Scandinavian worlds. Vikings from Sweden followed the Russian waterways that they ruled south to the imperial capital on the Bosphorus, where their legendary military prowess gave them easy entree into the Byzantine army. First they came as individual recruits to be absorbed into various regiments. But, from the last years of the tenth century on, they normally came to be part of special Varangian military corps, most notably the personal guard and crack troop of the Byzantine emperor himself; they were the famed and dreaded “ax-wielding barbarians.” The Varangian regiments were particularly important in the eleventh and twelfth centuries when, along with other mercenaries, they came to replace the dwindling thematic national army of the Byzantine state. The high point of Scandinavian activity in Byzantium coincided with the career of Harald Haardraade, half-brother of St. Olaf the King, son-in-law of the grand prince of Kiev, and himself king of Norway. Having fought as a junior officer with the Byzantine army in Sicily, Asia Minor, and Bulgaria, he was destined to die at Stamford Bridge in 1066.

Surely this is the stuff of novels as much as of history!

Benedikt S. Benedikz has certainly done historians a service by translating and updating Sigfús Blöndal's basic work on the Varangians in Byzantium. Almost without exception, the original author and his reviser record and analyze every shred of evidence that touches on the Vikings on the *Oesterveg* or in the empire, or that might conceivably refer to Norsemen and their activities at the eastern end of the Mediterranean. The authors have scoured the voluminous Norse saga-related literature and reproduced here most of its references to the Byzantine Empire; they have weighed and evaluated these references and attempted to relate them to each other and, when possible, to historical sources from elsewhere. Anyone seriously interested in Varangians in Byzantium must consult this book.

The book, however, is in many ways unsatisfactory. The authors' conscientious exegesis of any material even potentially relevant to questions of Viking service in Byzantium leads constantly to chains of interesting, but unconvincing, conjectures dependent one upon the other and adding nothing to real knowledge about the subject. The introductory chapter on the Viking penetration of Russia rehearses in minute detail material long since available elsewhere. The footnoting is careless and undependable, and the book is not particularly well written. Finally, a consistent picture of the Norse and later English “Varangians” in Byzantium never emerges from the mass of accumulated facts and surmises. Although this volume will sometimes prove extremely suggestive on specific questions, for a more balanced tracing of the role of the Varangians in the Eastern Roman Empire, treated, indeed, in a wider historical perspective, one might better consult Hilda Roderick Davidson's recent *Viking Road to Byzantium* (1976).

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CHARLES R. YOUNG. *The Royal Forests of Medieval England*. (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1979. Pp. ix, 221. \$14.00.

This is a highly readable general history of the English royal forest system from the Normans' introduction of the technical concept of royal forest until the later Middle Ages, when this system declined. The treatment is fundamentally chronological, the preface and introduction being followed by chapters on the Norman kings' organization, the Angevins' reorganization, and the Angevin system at work. The fourth chapter shows the forests be-

coming a political issue around the beginning of the thirteenth century, when the king's exploitation of them made forest administration, which already had legal, economic, and social repercussions, an important baronial grievance. After two chapters on the law and administration of the forests and on their economy when the system was at its height in the thirteenth century, attention is refocused on the political aspect of the forests in the later thirteenth and early fourteenth centuries. The final chapter charts the decline of the forests as an institution after 1327. There are helpful footnotes, a selective bibliography, and an index; three maps, five tables, and two graphs buttress the text.

Charles R. Young seeks "the overall pattern of forest administration buried in the detail of record sources" (p. viii) and disregards individual variation in order to establish main trends. His work highlights some interesting aspects of English medieval history, for the royal forests, previously studied mainly individually, were not merely regional phenomena or simply wooded land. In the thirteenth century a quarter of the land was technically forest and the identifiable royal forests were spread widely over England. In addition to the common law, a particular forest law, with its own officials and courts, applied within the area designated as royal forest. Young describes the evolution, maturity, and decay of the forest system, clearly sets out the hierarchy of officialdom and courts, and probes the relationship between forest administration and the ordinary local and central administrative systems.

The economic exploitation of forest resources can be traced through government records, and Young's assessment of the economic value of the forests in relation to contemporary resources and needs helps to explain the forests' political significance. The royal forests produced for kings goods in kind, cash profits, and opportunities to exercise patronage; but landholders resented controls on estates held within designated forests, and forest laws were seen as arbitrarily and capriciously enforced. After 1327 the forests' extent was limited and the particular forest administration began to disintegrate. The royal forests, becoming economically insignificant, declined into mere hunting preserves much as they had begun.

The complicated technicalities of forest administration are clearly explained in this comprehensive book, and Young's humane interest in his sources leads to the inclusion of many illustrative cases that enrich his text.

HELEN M. JEWELL
University of Liverpool

träge zur Mediävistik und Renaissance-Forschung, number 29.) Munich: Arbo-Gesellschaft. 1978. Pp. xv, 212. DM 25.

In the third quarter of the ninth century, Laon formed an important link in a chain of cultural centers responsible for the Carolingian renaissance, inasmuch as its cathedral school transmitted to an entire generation of Continental scholars the fruits of the intellectual labor of a small group of Irish scholars, most notably Martin Hiberniensis and John Scottus Eriugena. Despite this, little has been known about the school's masters, students, and books or about its nature and influence, largely because of the paucity of sources available for the history of the school.

John J. Contreni set out some years ago to remedy this deficiency, producing an intellectual history of the school as his doctoral dissertation. Since then he has made a fine translation of Pierre Riché's *Éducation et culture dans l'Occident barbare* (1976) and has learned much from Riché's technique of going beyond narrative sources to milk the last drop of significance out of diplomatic, juridical, epigraphical, and archeological sources. This has led him in the present work to retain from his dissertation only the first three chapters, and in the remaining nine to shift from intellectual to institutional history, tapping a previously unexploited source, the school's manuscripts, to supply important detail regarding the composition of the library and the operation of the scriptorium at Laon's cathedral school. His book begins with a detailed analysis of the ecclesiastical and political history of ninth-century Laon, which does a great deal to clarify the nature and extent of the indispensable support given the school by the city's bishops. It then examines the manuscripts in detail, illuminating the formation and composition of the library as well as the origins of its manuscripts. In the final section, it discusses the teaching activity of the masters, calling attention to the central role of Martin Hiberniensis and drawing conclusions about the school's students and influence. The book is thorough in documentation and provides appendixes detailing the contents of the library and presenting a concordance of shelfmarks of ninth- and tenth-century manuscripts. A bibliography, general index, and index of manuscripts are included.

The greatest drawback of this book, one that Contreni himself points out, is that it is the study of a single school in isolation, without reference to its complex linkages to other centers of study. The fault is not the author's. There is need of a study of all ninth-century intellectual centers, and Contreni suggests that it ought to be, like the present work, an archeological investigation of culture, performed codicologically by reconstructing libraries, deter-

mining the origin and provenance of manuscripts, and studying their notes and text traditions. Con-
trenti's provocative and sound study is a worthy
milestone along the road to such an investigation.

JOSEPH M. MCCARTHY
Suffolk University

MALCOLM BARBER. *The Trial of the Templars*. New
York: Cambridge University Press. 1978. Pp. vii,
311. \$28.50.

The intent of the author of this book was to make
available, in English, a complete account of the
trial of the Knights Templar and to demonstrate
the relevance of that event to the political turmoil
of the twentieth century. In the first of these goals
he succeeds admirably; in the second he does not.
Since the first purpose is by far the more important,
the book can still be judged a significant contribu-
tion to the literature on the Templars.

Increasingly, students of our educational system
are unable to read foreign languages. In their study
of medieval history this means simply that a large
body of scholarship on many important topics is in-
accessible to them. Translation alone is not the an-
swer; there is little to be gained by translating old
interpretations. Malcolm Barber's study is the only
proper response to current academic needs. It is a
judicious blend of old and new work, past and per-
sonal interpretation, written in a good descriptive
English prose style. The modern student now has
available a thorough and balanced account, incor-
porating all prior studies, of one of the more inter-
esting and important episodes of the Middle Ages.

Barber's presentation of the facts of the pro-
ceedings against members of the crusading order is
a very straightforward descriptive account, almost a
day-by-day record of a six-year event. Enormously
detailed and heavily documented from original
sources, these chapters convey by sheer weight of in-
formation the complexity of Church-state relations
in medieval society and the conflicting motivations
of all of the principal characters in the great drama
of the downfall of an order that medieval men had
once held in awe. Particularly valuable in regard to
evidence are the appendixes where the reader can
find the original articles of accusation brought
against the order in 1308 and the description of a
reception into the order in 1311. One cannot
quibble, let alone quarrel, with the author's mastery
of the sources for his subject.

The conclusions that Barber draws from his in-
tensive study of the trial proceedings and of the po-
litical maneuverings of popes and kings during the
process are well balanced and well argued but not
highly original. Serious students of the suppression
generally agree that the Templars were unjustly ac-

cused and convicted, that Philip IV was greedy for
their wealth, that other European monarchs acted
better than he, and that Clement V was a weak
pope. The reader seeking a new interpretation will
not, therefore, find what he is looking for in Bar-
ber's conclusions. Personally, I do not regard this as
a failing. We have for too long in historical writing
suffered from "extravagant originality." Barber's
conclusions are fully supported by his facts and that
is sufficient for any scholarly study.

The author's belief, stated in the preface, that the
trial of the Templars "has some relevance to the
world of the late twentieth century" is curiously ir-
relevant. He never attempts to demonstrate this
and the evidence does not support it. It is difficult
to know what he had in mind. In any event, this
unfulfilled promise is inconsequential considering
the ample merits of what he has done.

C. LEON TIPTON
Lehigh University

JEAN FAVIER. *Philippe le Bel*. Paris: Fayard. 1978. Pp.
v, 587.

"He is neither a man nor a beast. He is a statue."
Such was the assessment of Philip the Fair by Ber-
nard Saisset, the notorious bishop of Pamiers. Un-
like such figures as Charlemagne, Henry II of Eng-
land, Saint Louis, and Frederick II of Sicily, whose
personalities and character emerge however dimly
from the available medieval sources, Philip the Fair
has remained particularly enigmatic. Historians
have been virtually unable to piece together from
narrative and nonnarrative sources any of the per-
sonality of this last great Capetian, an especially
frustrating situation because his reign is of special
significance for the development of French institu-
tions, the transformation of the Church, and the
secularization of society.

What was Philip's role in the many devaluations
of the French currency and the inflation, French ex-
pansion in the Midi, the traumatic conflict with
Flanders, the epic struggle with Boniface VIII, the
destruction of the Templars, and the summoning of
the assemblies? Was he a champion of nationalism
and a precursor of secularism? Was he capable of
true friendship and loyalty? Did he initiate, imple-
ment, and control policy? Historians have wrestled
with such questions since Pierre Dupuy first ex-
amined the records of Philip in the seventeenth cen-
tury. Romantics like Dumas and Michelet blamed
Philip for the notorious crimes and dramatic in-
cidents of his reign, whereas positivists like Charles-
Victor Langlois and Robert Fawtier ultimately con-
cluded that it was impossible to assess Philip's influ-
ence. A contemporary historian who believes it pos-
sible is Joseph R. Strayer. In a most convincing

article he has argued that Philip, rather than such councillors as Nogaret, Flote, and Marigny, was responsible for what was done.

Mindful of the difficulties of studying the reign of Philip the Fair, Jean Favier, director of the Archives de France, has virtually marinated himself in the sources and has written many studies on various aspects of Philip's reign. No one is better equipped to write about Philip the Fair, and in this book Favier has done a remarkable job, paying special attention to the king, his council, the household, the royal servants, monetary policies and finance, war, diplomacy, the struggle with Boniface VIII, and the affair of the Templars. He portrays Philip as no great innovator but as one who honored the tradition of Saint Louis and operated within a feudal ambience and mentality. Nothing Philip did can be interpreted as a dramatic rupture with the past and a leap into the modern world.

To see behind the mask of Philip is not easy because, as Favier so well observes, Philip listened more than he spoke and became involved in a decision or a negotiation only at the opportune moment. Although some of his councillors and officials specialized in certain tasks and business, none set or dominated policy; Philip was always in control. In disagreement with attempts of other historians to portray some of Philip's principal councillors as Machiavellian, Favier sees them as but reflecting the political ideas of a Thomas Aquinas. His treatment of Philip's conflicts with Flanders, Boniface VIII, and the Templars is eminently fair. He concludes that Philip did not covet the material resources of the Templars but wanted to remove from his realm this crusading order that had become anachronistic and remained a kind of independent enclave responsible only to the papacy. If Philip was ruthless in the battle with Boniface VIII, perhaps it was because Boniface was unrealistic in the authority he thought he could wield over early fourteenth-century Europe. Regarding the feudal lordship of the Capetians over Flanders, Favier does not question the legality but the manner of Philip's intervention in Flemish affairs.

Throughout the text Favier quotes extensively from key sources but generally develops an argument or comes to a conclusion without citing the specific evidence, which the reader must infer from the sections devoted to sources and an *orientation bibliographique*. This is unfortunate and unfair to those scholars, particularly American, whose recent studies have advanced our knowledge of the reign of Philip the Fair, some of whom are not even cited in the *orientation bibliographique*. One is often left uncertain and confused. Had Favier used footnotes, Philip might not have remained the enigmatic statue that he still does. Yet this book, developed from an extraordinary knowledge of the primary

sources and secondary materials, can only elicit admiration. Its sensible and moderate course generally places the well-known events and policies within the broad context of European and French history. It will now be doubly interesting to read the study of Philip the Fair by Strayer that reportedly is soon to appear.

BRYCE LYON
Broun University

THOMAS N. BISSON. *Conservation of Coinage: Monetary Exploitation and Its Restraint in France, Catalonia, and Aragon (c. A.D. 1000-c. 1225)*. Oxford: Clarendon Press. 1979. Pp. xxiii, 250. \$33.00.

The coinage of medieval Western Europe is an important subject, of interest not only to economic and art historians but also to scholars studying representative assemblies, political debate, and legal theory. In an economy of expanding trade and fixed rural rents, the coinage was a matter of great public concern, yet the lords who operated mints regarded them as private and potentially profitable enterprises. Good historical studies of medieval coinage have been hard to come by because historians and numismatists have been reluctant to become deeply involved in each other's specialties. These considerations make Thomas N. Bisson's book particularly welcome. It deals with a complicated subject, and much of it makes difficult reading; but it is a work of extraordinary scholarship. It throws important light on medieval attitudes about money and on economic and political problems that have been difficult to study because of innumerable regional differences that tend to defy generalized treatment.

Historians are more familiar with the politics of coinage at a later date, notably the fourteenth century, when rulers of large territories manipulated the currency in order to attract bullion and to make a profit from the mints. The profits amounted to a disguised tax on the rich and created a political uproar that has been easier to study and to generalize about because the debates were conducted at a national level. Bisson has tackled the more difficult task of examining the earlier, regional debates over the coinage. He has started at the beginning, the eleventh century, when coinage followed the path of other once-public functions and became essentially proprietary. As this happened, "the disappearance of the uniform Carolingian denier and the progress of commercial exchange combined to foster a heightened sensitivity to monetary value" (p. 12). The first reaction to this development was a demand to stabilize the coinage, not merely the weight and alloy (which seem to have received less emphasis than one would expect) but also the ac-

tual design of the coins. In some regions, rulers who agreed to mint no new coins were not even to replace worn issues with new coins of identical characteristics. This "conservation of the coinage was easily among the more reactionary economic notions known to history, [a] child of rural anxieties..." (p. 196).

Bisson considers in turn Normandy and the old Capetian domain, Languedoc and Catalonia, and the regions of northeastern France. Rulers confirmed or guaranteed their coinages under a variety of different circumstances and political pressures. In Normandy and the Capetian lands, inhabitants paid a small regular tax to guarantee a stable coinage. In Catalonia, by contrast, they paid less regular, but larger, amounts to secure confirmations of coinages by rulers whose mints apparently were profitable. After the 1180s, economic realities everywhere began to moderate the old desire to maintain the same currency. Popular concern shifted toward a desire to maintain some control over mutations and rates of exchange.

These few words cannot do justice to this excellent book. It contains an impressive bibliography, an appendix of important documents, and facsimiles of both documents and coins that attest to the author's scholarship.

JOHN BELL HENNEMAN
University of Iowa

ROGER BOASE. *The Troubadour Revival: A Study of Social Change and Traditionalism in Late Medieval Spain*. Boston: Routledge and Kegan Paul. 1978. Pp. xvi, 219. \$23.95.

In *The Troubadour Revival* Roger Boase asserts that troubadour poetry transcended the bounds of literature and that courtly love "was a literary and sentimental ideology, a secular profession of faith, which... was the chief impulse behind the cultural achievements and the style of life of the European aristocracy" from the twelfth to the sixteenth century (p. xi). Spain saw a substantial revival of troubadour literature and chivalric attitudes during the late fourteenth and fifteenth centuries, and Boase believes that this was due to Spain's unusual historical development, particularly the growth in numbers and influence of the aristocracy at a time when the military justification for its status was waning. As a consequence, Spain's high nobles "retreated into an anachronistic world of make-believe, and cultivated literary conventions and chivalric practices associated with a utopian past" (p. 5).

This, on the surface, is a plausible thesis. To prove it, Boase first provides a static description of the aristocratic theory that divided society into the familiar three estates—*defensores*, *oratores*, and *labora-*

tores. He demonstrates that many Spanish writers accepted and supported this division, even though it did not take into account the developments and differentiations in the third estate. In the same section, he shows how patronage regulated relations between the estates. Next, he examines Spanish history with the special aim of showing the reasons for the growth in the numbers of aristocrats in the fifteenth century. Finally, he prints a selection of documents to show that Aragonese, Navarrese, and Castilian rulers encouraged chivalric practices and sponsored academies of the "gay science" of chivalric behavior.

But Boase's evidence does not sustain his thesis. It is hardly surprising that fourteenth- and fifteenth-century Spanish writers maintained a belief in the three-fold division of society; it was widely held in Europe long after the fifteenth century. More important, Boase's survey of Spanish history is flawed by his reliance on an outmoded view of it. This is most apparent in his attempt to explain the rise of the aristocracy mainly by focusing on the role of the kings, who, facing external wars and internal contentions, created new titles and promoted new nobles. This is partially true, but what of the nobles themselves? Boase gives the impression that the sole utility of nobles was war and that, once their military importance declined, they no longer had any reason to exist. A wider reading in Spanish history, particularly of those historians who have analyzed the nobility, could have shown him a great deal more about noble attitudes, about the history of noble families, and about the economic activities of the nobles, many of whom were vitally interested in trade. The documents he reproduces in the third section could be used to support a modified thesis: that the monarchs, interested in building royal power, deliberately fostered an archaic ideology to bring the nobles under control.

Boase's scholarship is wide-ranging, and some of the points he makes are sound and useful. It is unfortunate that his overall analysis is ultimately unsatisfying.

WILLIAM D. PHILLIPS, JR.
San Diego State University

J. N. HILLGARTH. *The Spanish Kingdoms, 1250-1516*. Volume 2, 1410-1516, *Castilian Hegemony*. Oxford: Clarendon Press. 1978. Pp. xi, 716. \$39.95.

This continuation of J. N. Hillgarth's study of Spain in the later Middle Ages extends from the Compromise of Caspe to the death of Ferdinand of Aragon. The first part considers economic, social, religious, and cultural matters, while part two deals with political history to the death of Enrique IV of Castile. Part three, constituting almost half the

book, is devoted to the reigns of Ferdinand and Isabella.

One can comment on only a few of the many topics discussed. Hillgarth rejects the notion that the decline of Catalonia was the consequence of a series of events going back to the Black Death and argues that it can be demonstrated only after the civil war of the late fifteenth century. The most important economic development was the rise of Castile and Portugal whose opening to the Atlantic gave them an advantage in the early modern era that Catalonia lacked. The collapse of the centuries-long coexistence of Christians, Muslims, and Jews was of exceptional significance. Although the Moors seem not to have attracted intense hostility, anti-Jewish sentiment grew ever stronger and was encouraged by Jewish converts to Christianity. The push to suppress religious groups other than Christians was primarily a popular movement that did not win the support of the higher powers until the end of the fifteenth century. Also noteworthy is the limited influence of humanism in the peninsula. The first Catalan humanists were men attached to the royal court such as Bernat Metge, but in Castile they were nobles such as Santillana or *conversos* such as Cartagena. Traditional cultural strains, however, remained paramount.

In political terms the fifteenth century was a time of Castilian ascendancy, beginning with the establishment of the Trastamaran dynasty in Aragon following the Compromise of Caspe. This had the effect of creating a breach with the dominant classes in Catalonia and led to the civil war, so destructive of Catalonia's position in the Crown of Aragon and in the peninsula. The marriage of Ferdinand and Isabella effected a personal union of Aragon and Castile, but rather than see these two rulers as conscious creators of a modern state, Hillgarth thinks that they built upon and continued the traditions of the past, especially those of Castile. Their principal innovation was to make existing organs of government work more effectively than ever before. They departed from tradition in two ways. First, their marital diplomacy brought about the accession of Charles V and a foreign dynasty that involved Spain in northern European politics. Second, their determination to achieve religious unity by the expulsion of the Jews and the forcible conversion of the Moors deprived Spain of that uniqueness that had marked it throughout the medieval era.

As in his previous volume, Hillgarth stresses the diversity of the Iberian peninsula, arguing that unity was a concept fostered chiefly by propagandists laboring on behalf of Castilian claims to hegemony. In this, and in challenging other long-accepted interpretations, his work is stimulating and provocative. Castile and the Crown of Aragon remain the focus of his attention, while Portugal and

Granada are dealt with only incidentally. Even so, this remains a major study of late medieval Spain. There is an appendix on coinage, an extensive, annotated bibliography, an index, two maps, and two genealogical tables.

JOSEPH F. O'CALLAGHAN
Fordham University

DICK EDWARD HERMAN DE BOER. *Graaf en grafiek: Sociale en economische ontwikkelingen in het middeleeuwse "Noord-holland" tussen ± 1345 en ± 1415* [Count and Counting: Social and Economic Changes in Medieval "Noordholland" between ca. 1345 and ca. 1415]. Leiden: New Rhine Publishers. 1978. Pp. xiii, 395. f 30.

This study of social and economic developments in the northern part of the county of Holland in the second half of the fourteenth century is based on data series generated from records of the counts' officials. These figures are supplemented by numbers from urban and religious institutions. The results are significant both because they add to a growing body of knowledge about Europe's economy in this period and because they reveal major structural changes in the Holland economy. The book's emphasis is squarely on the latter.

Discussion of demographic changes takes up nearly half the book. That is followed by a digression on coinage, then a treatment of wages and prices, and finally a section on production and trade. In each case there are introductory remarks, presentation of data, typically in tables and graphs, and then an evaluation. The last is generally short but precise and clear. The author has a tendency to present any figures and to rely on even the most sparse series. He also tends to ignore changes for which no data series exist. But with those caveats the rewards of the book are still great.

Dick Edward Herman De Boer is not able to establish conclusively how much the 1349-50 outbreak of plague affected Holland. But he does show beyond a doubt that there were "echo-epidemics," later outbreaks with high mortality leading to an overall fall in population. From 1369 to 1415 the proportion of the population living in towns rose, as farmers deserted poor land in the peat regions. Grain output fell both in those areas and overall, despite a rise in production in the coastal clay region. Arable land was no longer productive because of a series of floods in the 1370s and 1380s, caused by a rising water level and peat cutting, which lowered the level of the land. Real wages for peat cutters rose because of the demand for energy in the expanding towns' industries: textile manufacturing and especially brewing. Grain prices rose in Holland despite the fall in population since output fell

while demand, for example from brewers, rose. The solution was grain imports from northern France and then from the Baltic. By 1415 Holland had already changed from subsistence to specialized agriculture, had a much larger proportion of its population in towns, and had come to rely more heavily on internal and international trade.

De Boer claims all too often throughout this dissertation that his work is only a start and a base for further research. That is bothersome, but more serious is his insistence on repeating data while paying all too little attention to what is an interesting story. The extremely short conclusion does not do justice to the product of the research. The structural changes, the shift from traditional agriculture to the relatively modern sectors of trade and manufacturing that De Boer identifies, were in fact the basis for the long term growth of the Dutch economy. He is able to place the origins of Dutch success precisely in the second half of the fourteenth century. In addition it is fortunate that De Boer has not stopped with just the publication of his work but also has taken an active part in distributing this book, all to keep the price under control.

RICHARD W. UNGER
University of British Columbia

HELGE SALVESEN. *Jord i Jemtland: Bosetningshistoriske og Økonomiske studier i grenseland ca. 1200-1650* [Agriculture in Jemtland: Settlement History and Economic Studies in a Frontier Area, ca. 1200-1650]. (Det Nordiske Ødegårdsprojekt, number 5.) Östersund, Sweden: Wisénska Bokhandelns Förlag. 1979. Pp. 187.

Scandinavian historians are currently involved in an ambitious project to study the "abandoned farm" phenomenon in all Nordic countries (*Det nordiske ødegårdsprojekt*, DNØ) in an effort to understand the late medieval agrarian crisis. The first volume of the series (*Nasjonale forskningsoversikter* [National Research Surveys] [1972]) announced the goal of examining comparatively the settlement patterns from 1300 to 1600 through in-depth studies in the various countries, culminating in a comprehensive synthesis. So far, three studies have appeared, dealing with areas in Denmark, Sweden, and Norway. In this volume the Norwegian historian Helge Salvesen treats the Swedish province of Jemtland. Since Jemtland belonged alternately to Norway and Sweden during the Middle Ages and became permanently attached to Sweden only in 1645, this assignment makes good sense. For more than a century Norwegian historians have sought economic answers to explain their lack of political independence since late medieval times. More than other Scandinavian historians they have developed

methodological finesse and arrived at far-reaching conclusions in this field. These merits Salvesen brings to his own work.

Jemtland is of special interest because it was the only high plateau in Scandinavia with a relatively dense population since prehistoric times. The combination of height and latitude made it susceptible to even slight changes in economic patterns. Since the Black Death did not reach Jemtland, the effect of the plague, which is often given the greatest importance in accounting for the late medieval agrarian crisis, can be eliminated here. As established by the DNØ guidelines, the book is largely descriptive. One chapter deals with nature, climate, and resources. Another, which comprises 60 percent of the study, is devoted to an exhaustive and methodologically astute analysis of sources in three (out of forty) parishes. The key elements are Swedish tax records and tithe lists from the second half of the sixteenth century, which provide the oldest complete evidence of settlement. From here Salvesen works backward and examines medieval charters, place names, and archeological remains. In a concluding chapter he estimates the ratio of abandoned farms in Jemtland to be 36 percent in the mid-sixteenth century and argues that, while the process started here as elsewhere in the late fourteenth century, recultivation did not pick up until the first half of the eighteenth century. He sees this as a conscious change during the sixteenth century from intensive farming, necessitated by the population increase during the High Middle Ages, to extensive agriculture, where the "abandoned" farms were incorporated into existing agricultural units but were used only for the grazing of animals. The absence in Jemtland of the influence of both the noble class and the state, which elsewhere in Scandinavia encouraged recultivation, made it possible for the farming community to enjoy the labor-saving device of mixed husbandry for two hundred years. The book suffers from some repetition and lacks an index, but a Swedish glossary is provided.

JENNY JOCHENS
Towson State University

ARNO BORST. *Mönche am Bodensee, 610-1525*. (Bodensee-Bibliothek, number 5.) Sigmaringen: Jan Thorbecke Verlag. 1978. Pp. 584. DM 68.

"Gäbe es Brüderlichkeit ohne Brüderschaften?" (p. 389). This rhetorical question summarizes Arno Borst's thesis. Superficially, this book is deceiving, appearing to be a regional history of monasticism, told through a series of biographical sketches. Actually, Borst has used selected monastic and religious houses to write a history of the interaction between the evolution of monastic communities and

the development of clerical and lay societies in the region around Lake Constance. Using a clear style unimpeded by jargon, Borst succeeds brilliantly in conveying the personalities and settings of his narrative.

Conflicting forces shaped monastic communities. They strove for separation from the day-to-day world, although the degree of their isolation and the means by which they attained it varied considerably. Against this segregation of themselves, the monks served their neighbors, both within and outside the cloisters, in a variety of ways that Borst traces ably. All monastic communities had poverty and spiritual autonomy as principles, but they had to support themselves. Whether they built up a complex of landed estates or participated in an urban market, their need for an economic foundation was another source of interaction with the outside world. Monasticism around Lake Constance originated outside the region and was repeatedly influenced by new movements from without, but regional power structures shaped the particular communities. Monastic communities were always on pilgrimage, moving not spatially but historically. They were in quest, shaped by a dynamism that prevented them from becoming static.

Borst divides medieval monastic history into four periods, the first two of which are clearly distinct, the latter two, in this reviewer's opinion, more arbitrary: From the arrival of St. Columban until the eleventh century, Benedictine monasticism arose within the Frankish and Ottonian aristocratic churches. In the later eleventh and early twelfth centuries, the Gregorian movement channeled monasticism into the reform of the clerical church. From the early twelfth century onward monasticism sought to shape the church of the laity. After ca. 1300 it found itself in the church of the burghers.

The eighty-four photographs are well chosen and fully described, but the format of the book unfortunately restricts their size. Perhaps to compensate for the space the pictures require, the publisher uses an exceedingly small typeface, which this reviewer found hard on his eyes. Borst argues that St. Francis of Assisi intended to found a brotherhood of laymen, without fixed convents or clerical structure, both of which were subsequently imposed on his movement, an interpretation with which this reviewer sharply disagrees. On the other hand, the bibliography, for example that on the Franciscans, is adequate and relatively recent.

Mönche am Bodensee is an original study of medieval monastic communities, a book whose importance extends far beyond its regional limits. No other historian has shed as much light on the monks in their physical and social environment.

E. RANDOLPH DANIEL
University of Kentucky

LAWRENCE G. DUGGAN. *Bishop and Chapter: The Governance of the Bishopric of Speyer to 1552*. (Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions, number 62.) New Brunswick, N.J.: Rutgers University Press. 1978. Pp. xv, 298. \$19.50.

The great difficulty in medieval history is that there is so much time to cover and so few documents to cover the historian's nudity. Thus, any attempt to study a minor but important institution such as the bishopric of Speyer over a long period of time is fraught with dangers. This book by Lawrence G. Duggan is more remarkable for the many pitfalls it has avoided than for the occasional unwarranted generalization. It is a study neither of the bishops of Speyer nor of the bishopric but rather of the evolving role of the chapter and its relationship to the bishops.

The author's basic assumption, stated in the introduction, is that "the cathedral chapter . . . in time became the ordinary, functioning 'parliament' of the principality" (p. 7). This is the study's main interest. Even though it does not succeed in proving this hypothesis for the period under consideration (1090-1552), it sheds a new and interesting light on both the governance of bishoprics and on the nature of representative institutions.

Chapter one describes the period of ascendancy of the chapter, 1101-1272. Its principal interest lies in its treatment of the increasing power of the chapter as a process of local history, influenced but not dominated by the great movements of canon law and the conflict between emperor and pope. Although more might have been made of Speyer's very special position under the Salian emperors (the great cathedral itself is a symbol of imperial politics), the author's approach shows convincingly that the implementation of canon law relative to the chapter's position between 1140 and 1234 was not a foregone conclusion: it succeeded because in bishoprics like Speyer social and economic relationships were conducive to its implementation. The study strikes an interesting balance between explaining the developing relationship of bishop and chapter in terms both of general developments and of the small local problems that tended to add up to the big headaches in the provincial atmosphere of the bishopric.

A careful discussion of the electoral capitulation of 1272 follows in chapter two. This is in many respects the lynchpin of the argument; Duggan interprets it essentially as a modified form of *Herrschaftsvertrag* such as were concluded between temporal princes and their estates in the late Middle Ages. In this he is on a weak ground, as the wide disparity of cited examples, ranging all over Europe and over at least five hundred years, shows. The interesting lo-

cal details that may in fact go a long way to explain the document, unusual as it is, would probably have borne somewhat greater development. As the following chapter shows fairly clearly in describing the crises that befell the bishopric between 1272 and 1396, a major part of the struggles between bishop and chapter concerned a conflict between local and universal issues, with the chapter representing the economically well-founded local interest in which the increasing tensions with the citizens of Speyer, the financial management of the churches' property, and the efficiency of the courts played an absolutely central role. As Duggan points out, the background of the bishops (until 1396 generally from higher nobility and thereafter more often from the lower) was a decisive factor in their relationship to the bishopric and the chapter. In the latter period, discussed in chapter four, the bishopric was under the tutelage of the Elector Palatine. Although this diminished its standing, it obviously also contributed to peaceful and slightly more prosperous conditions after over a century of hardship.

Duggan offers a convincing picture of the way in which the chapter helped steer the bishopric through troubled times, often in spite of the bishop. But does this make a case that the chapter constituted the "parliament" or "diet" as he variously claims? Here he makes an argument by analogy; unfortunately, his ideas are too strongly influenced by concepts derived by others from fifteenth and sixteenth-century evidence, concepts hardly applicable to provincial Speyer in the thirteenth and fourteenth centuries. Otto Brunner is mentioned, but the lessons of his important work, *Land und Herrschaft* (1943), are not really absorbed; and Eugen Rosenstock-Huessy's seminal *Königshaus und Stämme* (1914), which deals with the rise of the principle of territoriality in the late twelfth century, is not mentioned at all. On the other hand, Duggan cites Max Weber on the theory of representation with respect to medieval Speyer. Whatever the merits of Max Weber, his knowledge of medieval history was rudimentary at best.

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RICHARD KIECKHEFER. *Repression of Heresy in Medieval Germany*. (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1979. Pp. xiv, 161. \$12.00.

This book deals with ecclesiastical and secular suppression of heresy in Germany from 1000 to 1520. Most of the work, however, is about the beguards, beguines, free spirits, and Waldensians of the fourteenth century.

Richard Kieckhefer argues that the inquisition as a formal institution never existed in Germany—cr,

for that matter, anywhere else in Europe before the sixteenth century. Against Henry C. Lea he claims that episcopal or secular authorities rarely if ever challenged the jurisdictional rights of the papal inquisition. The reason for the ineffectiveness of the papal inquisition in Germany, 1311–1520, lies not in local opposition but rather in the structural defects of the papal inquisition itself. Insufficient staff, ill-defined procedure, and lack of direction from the curia explain its misguided attacks on fringe heretics. There is no need to assume political or economic motives by emperors, bishops, and townsmen to account for the arbitrary actions of the papal inquisitors. Regional powers resisted the outside inquisitors only when the latter exceeded their mandate. Ordinarily, the papal agents could count on local support in their repression of heretics. During the fifteenth century the inquisitors wasted their time hunting witches, political dissidents, and harmless fanatics; Waldensians and Hussites escaped. In contrast to the bungling papal inquisition, local episcopal judges often mitigated the effects of dangerous heresies.

Kieckhefer's approach holds up well in his treatment of fourteenth-century Germany, the most original part of the book. Historians may dispute some of his arguments for the nonpolitical motives of episcopal-municipal authorities, but his general conclusions about local operations against heresy are perceptive and convincing. He usually avoids socioeconomic interpretations, and his stress on the popular backing for antiheretical measures nicely complements his *European Witch Trials* (1976), which demonstrates the popular fears of sorcery.

The author unfortunately tries to make his brief (112 pages of text) study imply too much. His book hardly renders an "anachronism" (p. 10) of the term "medieval inquisition." It seems contrived to make Germany "typical" of "medieval repressions" (p. 10) and France and Italy "exceptional." In his sketchy discussion of the fifteenth century, he blames the lack of a papally controlled inquisition for the "ineptitude" (p. 110) of the inquisitors. If Germans did not perceive heresy as a major threat, does it make sense to have the papal inquisitors "dissipating" their energies against "marginal" heretics? But inconsistently the author would also have the antiheretical sentiments of most Germans make them disposed to assist papal agents. It seems unnecessary, even irrelevant, to speak of institutional inadequacies to explain why the fourteenth- and fifteenth-century popes did not intervene more vigorously in Germany. Obviously (as the author is forced to admit), the deficiencies of organizational machinery is a result, not a cause, of papal inaction. Perhaps it is true, as Kieckhefer contends, that the papal inquisition's very weakness explains the relative absence of jurisdictional conflicts between papal inquisitors and episcopal-municipal compe-

tence (p. 42). But surely the point at issue is the reasons for the *papacy's* inability or unwillingness to stamp out the major heresies in Germany. Would anyone seek to account for the ineffectiveness of Charlemagne's administrative network by pointing to the "institutional weakness" of the *missi dominici*? At the least, this way of stating the problem begs the question. Finally, the author's determination to see in this study of medieval Germany a moral about the nature of institutionalized repression (pp. x, 112) seems better suited to "relevant" pop history and book club selections than to serious scholarship.

But these criticisms of the opening and closing sections of the book do not detract from its solid analysis of the fourteenth century. Kieckhefer has written a fine synthesis of the German inquisition. His important study complements the recent works on heresy by Lerner, Leff, and Patschovsky.

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CLAUDIA ULBRICH. *Leibherrschaft am Oberrhein im Spätmittelalter*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 58.) Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. 327. DM 70.

Late medieval and early modern western Germany was famous as a land of fragmented authority where peasants could simultaneously owe allegiance to an array of different territorial, judicial, land, and personal lords. The southwest especially knew a peculiar form of lordship over persons (*Leibherrschaft*) or personal serfdom (*Leibeigenschaft*), the meaning and impact of which have remained unclear, especially for the fourteenth and fifteenth centuries. Claudia Ulbrich's book, once a dissertation for the University of the Saar, investigates this phenomenon in a region around the great bend of the upper Rhine, roughly from the southern Black Forest south through Basel to Solothurn. The author traces the rise and development of personal lordship in exemplary ecclesiastical, municipal, and noble lordships and then devotes her last chapter to a systematic statement of findings.

Personal serfdom constrained the free movement, marriage, and inheritance of peasants along the upper Rhine. Except for the rare exaction of labor services or death dues, however, lords achieved thereby little direct material gain, only a few hens paid each year in token of subjection. Ulbrich connects the origins and intensification of this institutional structure with the economic difficulties and territorial ambitions of late medieval lords, asserting that it served to control peasant labor, enforce other, more directly remunerative, rights, and extend authority over persons who inhabited the lands of other lords. With the subsequent growth in

the region of true territorial states, however, personal lordship could be enforced only within one's own territory. Lords thus rationalized their states by exchanging enclaves of land and people to make of personal serfdom by the late sixteenth century just a common status for their subjects. Ulbrich argues, therefore, that personal lordship had a significant, though indirect, role in the institutional and political evolution of the area but that the particular form and function it assumed in each tiny lordship depended more on local conditions than on the differences in lords' status around which her study was organized.

Ulbrich's is a thorough and well-documented work of scholarship in the German tradition of institutional and legal history. The evolution of personal servitude in the region is plausibly connected to the lords' economic problems and political ambitions. Yet the context and implications of the phenomenon remain less than fully developed. The author skirts the precipice of an ecological fallacy when she appeals to general and vague evidence of a late medieval agrarian crisis in southwestern Germany to motivate specific measures taken by individual lords to intensify personal lordship. At the other extreme, however, little concern is shown for similar socioeconomic questions in the complex situation around 1500 when personal servitude was an issue relevant both to territorial state-building and to the peasant demands of 1525. The peasants are, in fact, rather the missing link throughout this study, for the perspective is wholly that of their lords. The objects of *Leibherrschaft* are just that, objects, who receive only seven pages of cursory comment at the end. Ulbrich's conclusion that personal serfdom had in the later Middle Ages more significance than students of more recent periods have perceived is, therefore, convincing and useful but very limited. In retrospect it is not surprising but fitting that the book does not conclude; it simply ends.

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MODERN EUROPE

R. R. BOLGAR, editor. *Classical Influences on Western Thought, A.D. 1650-1870: Proceedings of an International Conference Held at King's College, Cambridge, March 1977*. New York: Cambridge University Press. 1979. Pp. xiii, 394. \$45.00.

This volume contains the papers presented at the Third International Conference on Classical Influences held at Cambridge University in 1977. It is the last of a trilogy that traces classical influences on Western thought from the early sixth century A.D. to the late nineteenth century. This volume covers the period from 1650 to 1870, during which the

classical heritage was diluted, distorted, redefined, adapted to political and ideological causes, and finally superseded. But through it all, as the distinguished contributors to this volume show, the classical heritage remained remarkably vigorous and fecund.

The book is divided into four parts. The first part, entitled "The Epilogue to the Renaissance," examines classical influences in the seventeenth century: the influence of Roman law on the emerging national legal systems (H. Coing), Neoplatonic influences on scientific theory (J. Mittelstrass), hermetic and cabalistic influences on philosophy (C. Vasoli), the Neoplatonism of Comenius (K. Schaller), and the Pyrrhonism of Pascal, Bayle, and Hume (E. D. James). What all of these papers show is that the intellectual culture of seventeenth-century Europe, although no longer dependent on classical thought, would have been impossible without it. By 1650, as R. R. Bolgar correctly observes in his intelligent introduction, "there was not much more that the classical heritage could immediately contribute to the progress of European civilization, or at any rate there was not much more that it could contribute in the old straightforward manner that had been favoured by the humanists. But it had transformed the intellectual life of Europe, and its reputation stood high" (p. 8).

Part two, entitled "Antiquity as Myth," deals with the adaptation of the classical past to the political and social discontents and rising revolutionary temper in eighteenth-century Germany (M. Fuhrmann), Italy (J. H. Whitfield), France (R. A. Leigh), and America (M. Reinhold). The outstanding piece in this section is "La formation de l'Athènes bourgeoise: Essai d'historiographie, 1750-1850," by N. Loraux and P. Vidal-Naquet, which shows how the revolutionary picture of ancient Greece advanced in the eighteenth century was gradually transformed in the nineteenth to serve reactionary causes and interests. What all of these papers show, however, is that eighteenth-century nationalists, political radicals, and discontented intellectuals, although they still respected classical antiquity, consulted and quoted classical writers merely to confirm what they already believed or in order to give their causes intellectual respectability.

Part three, entitled "Antiquity as a Source of Facts," contains papers on the classicism of Vico (G. Costa) and Adam Smith (P. G. Stein) and S. G. Pembroke's study of classical influences on several theories of the early human family conceived between 1770 and 1870. These papers trace the assimilation of classical culture to the modern interest in primitive man and the origins of civil society. For Vico and Smith, classical literature was valuable not only as a mine of information about primitive societies, but also as itself an expression of the gen-

eral culture of the primitive epoch. When Stein writes that "Smith treated Homer almost as a text of social anthropology" (p. 265), it becomes clear how important this new conception of classical culture was in the development that led from the romantic idealization of primitive man to the beginnings of a new science.

The papers in part four discuss classical influences on the English utilitarians (H. O. Pappé), Goethe (T. Gelzer), Thomas and Matthew Arnold (R. R. Bolgar), Nietzsche (U. Hölscher), and Renan (J. Seznec). These thinkers were still able to find inspiration in the classical, especially Hellenic, heritage, but they were no longer able, or even inclined, to accept it at face value. For them, the classical world was only one source of enlightenment among many but no longer a uniquely privileged source. Yet their ideal of a long-extinct world, characterized by spontaneity and harmony, was still potent enough to guide their efforts to revive it in a new and higher form. Their failure marked the end of classicism as a significant influence on Western thought.

This volume is not, and does not purport to be, comprehensive; nor does it add much in the way of new knowledge. What it does, and does very well, however, is to remind us of the remarkable vitality and longevity of the classical heritage and the wide range of purposes it served for more than two centuries.

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NANCY L. ROSENBLUM. *Bentham's Theory of the Modern State*. (Harvard Political Studies.) Cambridge: Harvard University Press. 1978. Pp. xv, 169. \$15.00.

Recent scholarship has demonstrated that Jeremy Bentham was an Enlightenment philosophe dedicated to the progress and improvement of the human species. His range of interests included linguistics, ethics, and behavioral psychology as well as jurisprudence, penology, and government; his work was designed to form a complete social as well as political system based on the principle of utility. It is Nancy L. Rosenblum's contention, however, that Bentham subordinated all to the primacy of statecraft and the exigencies of political authority (pp. 6-7). For Rosenblum, Bentham was, first and foremost, a political thinker; the principle of utility was designed to serve "rulers." Again and again, Rosenblum writes, "The chief claim Bentham advanced for utility was as a rationale for legislation" (pp. 7, 9, 38, 39, 50, 55, 58). Indisputably, much of Bentham's writings included political theory; that this is the be-all and end-all of Bentham's work is simply too large a claim, given the facts. Moreover, this

is a very limited study since Rosenblum relies predominantly on the Bowring edition of Bentham's *Works*; the enormous mass of unpublished Benthamiana has not been investigated.

Yet the usefulness of Rosenblum's study lies in its introductory account of the political thought of a major Enlightenment thinker. Of particular significance is Rosenblum's chapter on Bentham's development of the social psychology of pleasure and pain as the ethical basis of laws in which the diversity and changing desires of the state's inhabitants had to be considered in the legislative process. There are also two strong chapters on Bentham's theory of sovereignty and international relations. Here Rosenblum argues that Bentham accepted the absolutist nature of sovereignty as part of his command theory of law but rejected its monarchical form and in its place substituted his theory of "popular sovereignty," which acts both to restrict and legitimize its power. His internationalism was predicated on the principle that a state insured its own well-being first, without doing injury to other states—a point that leads Rosenblum to conclude that Bentham, though an adversary of Machiavelian state egoism, might thus be placed within the "reason of state" tradition. Though debatable in some of the details, Rosenblum's book has opened up some interesting lines of discussion for Bentham scholars to confront in the future.

Perhaps the most debatable issue is Rosenblum's view that Bentham did not just accept the state—meaning the sovereign, territorial state of his own times—as "a norm of order," but, in her words, no one before him "has proselytized more fiercely on its behalf" (p. 2); no one has been more concerned with its political development, which Rosenblum calls modernization. As Rosenblum notes throughout her book, Bentham's state was one whose institutions and rationale for existence were diffused with the application of the principle of utility. Indeed, one might call Bentham's modern state the utilitarian state; in this sense, perhaps the equivalent of modernization is utility. When Rosenblum writes that Bentham opposed fanaticism, subjectivity in politics and all higher law, and "anti-legal ideologies," or that he divorced "reason and virtue" from the security that law provides (p. 54), or that his principle of the equal utility of states opposed universalism and "Enlightenment Cosmopolitanism" (p. 101), or that Bentham opposed philanthropic arguments and did not "advance the golden rule" (p. 104) in his theory of international relations, this could well demonstrate Bentham's utility and not necessarily his enthusiasm for the state as "a norm of order." It is useful to recall that utility, by definition, wars against meaningless phrases, catchwords, clichés, and abstractions that have little or no reference to specific human contexts. Ben-

tham was a thoroughly consistent utilitarian, and perhaps he "proselytized more fiercely" on behalf of this principle and the "greatest happiness of the greatest number" than he did for what Rosenblum calls the modern state.

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PETER MATHIAS and M. M. POSTAN, editors. *The Cambridge Economic History of Europe*. Volume 7, *The Industrial Economies: Capital, Labour, and Enterprise*. Part 1, *Britain, France, Germany, and Scandinavia*. New York: Cambridge University Press. 1978. Pp. xx, 832. \$46.50.

The sixth, seventh, and eighth volumes of *The Cambridge Economic History of Europe* are intended to cover modern developments not only in Europe but also in the whole "Western world" (including the United States and Japan together with European countries) "during and after their industrialization," starting in Britain in the eighteenth century. The sixth volume, previously published, was concerned with the "external parameters" of economic development: geographical expansion, demographic change, and technical progress. The present (seventh) volume focuses more narrowly on economic growth in the classical sense, dealing separately with the main "factors of production": capital, labor, and entrepreneurship. These factors are examined country by country, with national experts writing on each specialist topic, prefaced by a brief theoretical chapter from Solow and Temin interrelating these "inputs for growth."

The depth of specialist analysis is such, however, that this volume has had to be divided into two parts, each requiring a book of very considerable size, yet even so not covering the whole of Europe. Part one (here under review) deals with Britain, France, Germany, and Scandinavia, part two with the United States, Japan, and Russia. The inclusion of the United States and Japan and the exclusion of several important European countries inevitably raises doubts as to whether this can really be regarded as an "Economic History of Europe." Can it, with Russia and Japan included, even be meaningfully considered an economic history of the "Western world"? It is, in fact, neither but is, as the main title of this and the forthcoming eighth volume indicates, a collection of studies on the most developed "Industrial Economies" of the modern world, starting from a European core.

That being understood, this particular book provides an admirable analysis of the contributions and interrelationships of labor, capital, and entrepreneurship in the development of modern industrialized economies. With such well-known scholars as

Feinstein, Pollard, and Payne in Britain, Lévi-Leboyer, Lequin, and Fohlen in France, Tilly, Lee, and Kocka in Germany, writing expertly in their own specialist fields, and with Hildebrand dealing more comprehensively with Scandinavian developments, this book provides rich sources of information and illuminating interpretations, combining both quantitative and qualitative aspects, in what can only be regarded as a triumphant vindication of the editors' classical or neoclassical approach to analysis of economic development. The specialized investigation of factors of production is thoroughly justified by the insights provided into capital formation, investment, and financial institutions; into labor supply, recruitment, training, and industrial relations; and into entrepreneurial innovation and managerial functions, together with business structure and strategy.

The periods covered are not the same for each topic or country, some being confined to the classic period of the first industrial revolution in the eighteenth and nineteenth centuries, others continuing well into the twentieth century and up to recent years. Nevertheless, with their common approach and with such close economic, social, and cultural similarities between the European countries dealt with in this book—despite variations in factor endowments, institutions, and chronologies of development—the pattern that emerges is strikingly uniform, emphasizing the common features of industrialization, as against the differences or special features usually displayed in traditional national histories. Not only are we all “Europeans” nowadays, it seems, but we have always been, at any rate in our basic economic characteristics.

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ARTUR ATTMAN. *The Struggle for Baltic Markets: Powers in Conflict, 1558–1618*. Translated by EVA GREEN and ALLAN GREEN. (Acta Regiae Societatis Scientiarum et Litterarum Gothoburgensis, Humaniora, number 14.) Göteborg: Kungl. Vetenskaps- och Vitterhets-Samhället. 1979. Pp. 231. 50 KR.

Artur Attman, whose splendid work of 1944 on the trade at Narva in the sixteenth century immediately became a classic, should have left superb alone. This book will add nothing to his reputation and very little to Baltic historiography. For the specialist it is totally redundant. To the general reader, new to Baltic history, it will convey much interesting and even detailed information not easily available in English. But even the nondemanding reader will be distressed by the clumsiness of the language and upset by obvious errors. The translators, Eva Green and Allan Green, did not do a

good job; for example, “kapers” were not “pirates,” the “utliggare” of the Swedish navy off Narva were not “outriggers,” and the word “medlarna” means mediators not negotiators. The footnoting, aside from being excessively compressed, is carelessly done with items cited sometimes having no relevance to the text. The bibliography, although it is extensive, leaves out important works.

The main body of this book, and indeed it is little influenced by input from more recent publications, is copied directly from Attman's earlier writing. Paragraph after paragraph, chapter after chapter, is almost word for word the translation of the classic, *Den Ryska Marknaden* of 1944. The concluding, much briefer, portions are from his 1949 article on Stolbova. Since Attman, in his originals, could hardly have said it better, the decision simply to copy is understandable. Unfortunately, much is also left out, the gaps are only partially bridged with new text, and what we have is an inferior version (in English) of a good thing (in Swedish).

The subject matter is, of course, not at fault. It is as fascinating as it is many faceted. The Swedish effort to control trade in the eastern Baltic during a period of great flux, 1558–1617, was facilitated by many (sometimes quite extraneous) developments, complicated by others, and bitterly opposed, constantly or sporadically, by various combinations of Russians, Danes, Hanseatics, Poles, English, Dutch, Livonian Germans, and others. The melee was a military-political struggle for economic advantage for military-political advantage for . . . and Attman is master of the detail as well as the general scene. Unfortunately, as suggested above, in this book much of the detail has been dropped while the overview does not come through. If this is a new effort by the Swedes to conquer a larger market (in this case for history books), they would have done better not to adulterate their goods.

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ERNST L. PRESSEISEN. *Amiens and Munich: Comparisons in Appeasement*. Boston: Martinus Nijhoff. 1978. Pp. x, 153. f 32.50.

“Comparative history,” Ernst L. Presseisen tells us at the start of this little book, “will not attract those who insist on the uniqueness of each historical event.” To the “open mind that is willing to give it a try,” however, the comparative method “offers exciting possibilities,” indeed, the “best chance to go beyond history as mere chronology and to arrive at those comprehensive conclusions that evidence plus insight allows.” In fact (and this is Presseisen's purpose), when applied to the historical phenomenon of appeasement, the comparative approach “pres-

ents unique opportunities" for understanding causes, motives, and structure.

With open mind, then, and willing to give it a try, the reader follows Presseisen into his comparison of two episodes of British foreign policy: the deal with Napoleon that produced the Treaty of Amiens in 1801 and the appeasement of Hitler that culminated at Munich in 1938. It is immediately a path of confusion. The object before us is to chart the similarities of two revolutionary ages, two epochs of explosive, aggressive "ideologies," two eras of "mass participation in politics and war." At "first glance," Presseisen remarks, "these periods do not have much in common": the French Revolution "universal in its 'progressive' or 'liberal' character"; the "Nazi revolution" that is reactionary and racist. But somehow they fit together, as do Bonaparte and Hitler, who are described alternatively as "the embodiment of revolutionary storms" and as "autocrats." As for the ideologies of the two periods, our author-guide manages a dazzling integration. In the 1930s, he says, "the ideological picture is complicated by the simultaneous strength of communism. In fact, nazism and communism should be compared as a single ideological factor with jacobinism in order to appreciate the revolutionary character of both periods."

This reader, for one, parted company with Presseisen exactly here, at his introductory "Point of Departure." He has a story to tell, but it has to be winnowed out of the artificialities of the "comparative method." The only possible comparative focus is upon the British elites, in both periods men of limited vision and abilities, negotiating from economic weakness and frightened by popular unrest at home, desperate to make "peace at any price" with the foreign leaders whom they *perceived* to be respectable counterrevolutionary forces. (Napoleon in fact consolidated the "liberal" revolution—minus its nonessentials of political liberalism; Addington misread that, as Chamberlain, differently, and in the specificity of his own historical age, misunderstood Hitler.) The evidence for all this is in Presseisen's pages, but one has to search for it, extract it from his muddling together of Jacobinism, Bonapartism, socialism, communism, and nazism. The alternative to comparative history, Presseisen thinks, is "historicism"; it is an absurd choice, but in the circumstances that he offers, historicism wins.

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JAN ROMEIN. *The Watershed of Two Eras: Europe in 1900*. Translated by ARNOLD J. POMERANS. Middletown, Conn.: Wesleyan University Press. 1978. Pp. xxxviii, 783. \$25.00.

Congratulate the Wesleyan University Press for publishing this long work, which at first glance ap-

peals neither to specialists nor generalists. Specialists will find their focuses dispersed over too broad a canvas; generalists will lose their way in the myriad of detail that Jan Romein presents in each of his chapters. And yet the book is a treasury of illuminating fact and perceptive commentary.

This Dutch historian was a controversial personality all his life. He frequently polemicized against Johan Huizinga, whom he considered his intellectual mentor, and Pieter Geyl. He termed himself a Marxist, yet in 1927 was expelled from the Dutch Communist Party. His firm belief that history could only be understood in terms of dialectical contradictions was ironically borne out in 1949, when he was denied a visa to America because he was a "Communist" while the party still denounced him as a renegade. He died in 1962 fully involved with this study. His wife and several colleagues completed parts one and two, which constitute this book. Part three, intended as the theoretical culmination of his life work, was fully conceptualized in his mind, but unfortunately nothing had been committed to paper before his death. So the book remains incomplete but still impressive.

Romein's dialectical approach was more than Marxist; he developed it as an esthetic and dramatic principle. He saw "no forward movement without a return, no change without resistance, no oppression without the pursuit of freedom, and no progress without the risk of being overtaken by those who have lagged behind" (p. x). He tried to overcome the barriers of specialization and sought to understand this significant historical era by a three-dimensional comprehension of all facets of human thinking and endeavor. The Marxist pattern served as a guide but not as a strait-jacket. Ultimately he sought to establish a common human pattern by bringing in major elements of non-European history in order to understand better that crucial era when Europe was dominant in the world but not alone in it.

Romein commanded a wide range of bibliography, including seminal works of the diverse areas he studied. Yet he failed to list new works of his later lifetime that might have influenced his conceptions and methods: Henri Hauser, H. Stuart Hughes, J. R. von Salis, Gerhard Masur, and others. As an example of these limitations, see his chapter "Berlin to Baghdad." His views of imperialist economics and diplomacy were dated even in the context of his own life span. There are major references to works by Hallgarten, Helfferich, J. B. Wolf, and lesser items. But lacking are references to major works on international capital movements (Feis, Staley), German public enthusiasm (Dehn, Jäckh, Rohrbach), relations with Turkey (Earle, Holborn, Mühlmann), and recent diplomatic history (Langer, Chapman). The narrative on this subject

is sprightly, but the explanations are simplistic and the treatment is necessarily somewhat uncomplicated.

And yet, despite such evidence of omission, this is an original and intellectually suggestive book that is written in an engaging style. Most historians should be able to read it with stimulation and professional reward.

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RICHARD VAUGHAN. *Twentieth-Century Europe: Paths to Unity*. New York: Barnes and Noble, and Croom Helm, London. 1979. Pp. 261. \$21.50.

Richard Vaughan's *Twentieth Century Europe: Paths to Unity* opens with a list of eighty-eight abbreviations that include not only such central institutions of the Common Market as CAP (Common Agricultural Policy) and COREPER (Committee of Permanent Representatives) but also more peripheral European organizations such as JET (Joint European Tours), a nuclear research project, and FIFA (Fédération Internationale de Football Associations). The index contains even more of the abbreviations with which the text abounds, such as Coreunet (a cipher network), the aptly named COST (Committee on European Cooperation in the Field of Scientific and Technical Research), and the happily abortive Fritalux (customs union of France, Italy, and Benelux). The curse of the acronym lies heavy on the author who attempts to write a synthesis of the history of the European integration movement that is comprehensive in its coverage.

Vaughan's short book outlines the antecedents of the movement to limit the power of the European nation-state from the thirteenth to the nineteenth centuries, assesses the reasons for the failures in the interwar years of such "false starts" as Pan-Europa and the Briand Memorandum, and demonstrates the genuine progress toward supranationalism made since 1950. His excessive coverage of minor organizations and writers does, however, distract the reader from the significance of the work of the European Communities and creates the same ennui that now affects the general public in Europe whenever integration is mentioned. There is a certain advantage, however, in Vaughan's approach. His coverage of the federalist organizations and of the vast federalist literature provides evidence of the widespread public readiness for concrete moves after the Second World War to restrict the powers of national government. Without the wartime writings of federalists like Ernesto Rossi and Henri Frenay or the earlier work of writers like Gaston Riou, the proposals of Robert Schuman and Jean Monnet would have seemed far more revolutionary.

Vaughan's most unusual departure is to devote detailed analysis to the Scandinavian countries, whose cooperation he traces from the Norway-Sweden customs agreement of 1825 to the difficult adjustment of the Nordic Council to Denmark's membership in the Common Market; and he demonstrates that Nordic cooperation has not been so successful as to form a model on which Western Europe might have shaped its own integration.

Vaughan fails to set the integration movement adequately within the context of Cold War rivalries. In particular, he does not assess whether the European integration movement placed Western Europe more firmly in the American orbit or was a successful attempt to establish a counterweight to American influence. He also ignores the stimulating attempts of American political scientists, such as Carl J. Friedrich, to assess the extent to which integration has brought about a new form of political and social community in Europe. The book is useful as a form of textbook giving an introduction to the basic events of the integration movement. It does not claim to be a piece of research. It would, however, be greatly improved if the author had provided footnotes or, at the very least, a bibliography.

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VOLKER METTIG. *Russische Presse und Sozialistengesetz: Die deutsche Sozialdemokratie und die Entstehung des Sozialistengesetzes aus russischer Sicht, 1869-1878*. (Politik- und Gesellschaftsgeschichte, number 4.) Bonn: Verlag Neue Gesellschaft. 1979. Pp. 476. DM 46.

The author of this work seeks to define the "image" held by the Russian press toward the new German Empire and to understand, in particular, its response to the evolution of the German Social Democratic Party from its foundation to Bismarck's antisocialist legislation of 1878. The author demonstrates that the Russian press was slow to grasp the significance of the working-class movement, in part, at least, because of a belief that it was irrelevant for Russia where "a poverty of property owners" prevailed. In any event, the Russian press was preoccupied for the most part with domestic matters, and when it did report events in Germany, it tended to do so on a day-to-day basis with little inclination to examine the larger forces that were shaping German society. Those efforts by the press to be analytical tended to be shallow, and in the early part of the period the press perceived a phenomenon such as "ultramontane fanaticism" as the chief antisocial evil. In the wake of the Polish insurrection of 1863, in which Russian nationalists professed to see the fine hand of the Vatican, this was a view that was easy for the Russian press to accept.

But in 1878, when Bismarck began to use attempts upon the life of the German emperor to push antisocialist legislation through the Reichstag, the Russian press began to follow German affairs more closely and to view them against the background of rising terror within Russia. The situation proved particularly agonizing to Russian liberals, who had to choose between unfettered terror or legislation that threatened their own freedom. This dilemma divided the Russian liberal press, and, as the controversy evolved, the Russian press as a whole came generally to reflect similar divisions within Germany. *Golos*, *S. Peterburgskie vedomosti*, and *Birzhevy vedomosti* adopted a "progressive" national-liberal posture; and *Vestnik evropy* pursued a "progressive" course "with democratic tendencies." These relative positions, according to the author, reflected the attitudes of these publications toward Russian domestic issues, although his tendency to resort to such unqualified political labels as "liberal" and "conservative" leaves this conclusion without firm foundation. These terms have meaning only within the Russian context and require considerable explanation to possess any real significance.

The treatment of the material in this study is excessively narrow. Two-thirds of this lengthy volume treats a few months in 1878 in tedious detail. Much could have been summarized and characterized, making the work both more readable and more focused. In view of the author's unsurprising conclusions, one regrets the editorial decision not to require substantial pruning of the manuscript. Perhaps the reason for not doing so lies in the fact that this work was a dissertation at the University of Bonn, appearing here as volume four in a series published by the Friedrich-Ebert Foundation, and is cheaply produced in offset typescript. The editors may also be faulted for the fragmented and choppy structure, owing to the author's constant use of subtitles rather than transitions. The contribution this work makes to our understanding of Russia is small, and it does little to illuminate the history of Germany.

FORREST A. MILLER
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GERHARD SCHREIBER. *Revisionismus und Weltmachtstreben: Marineführung und deutsch-italienische Beziehungen 1919 bis 1944*. (Beiträge zur Militär- und Kriegsgeschichte, number 20.) Stuttgart: Deutsche Verlags-Anstalt. 1978. Pp. 428. DM 58.

One of the primary functions of the "new" military history is to integrate strategic and tactical considerations into the broader spectrum of social, economic, political, and diplomatic history. This book admirably achieves the goal. Gerhard Schreiber's

analysis of German-Italian naval relations rests upon an impressive documentary basis: the files of the Federal-Military Archive in Freiburg, the Foreign Office Archive in Bonn, the Naval Historical Branch of the Ministry of Defence in London, and especially the Archivio Ufficio Storico della Marina Italiana in Rome. Nor is Schreiber afraid to tackle the plethora of books dealing with Germany's role in the Mediterranean in particular and in the Second World War in general.

A major theme of the work is revisionism. During the early years of the Weimar Republic, German naval officers were already planning on armed alteration of the Versailles Treaty and for this reason rejected the politics of Stresemann, even though the latter was not averse to revisionism. Schreiber accurately describes the naval officers' early acceptance of National Socialism and their almost sycophantic loyalty to the Führer under Admirals Raeder and Dönitz until the last days of the war, if not beyond.

Schreiber also stresses that the navy was firmly committed to a "final solution" of the English question, even though the term *Endlösung* was not applied to Britain until 1939. At a crucial meeting at Friedrichshafen with the Italian Chief of the Admiralty Staff, Domenico Cavagnari, Raeder depicted the coming conflict with London as a "life-and-death struggle." It is also interesting to note, in light of Raeder's later claims in behalf of his so-called Mediterranean strategy, that at this meeting he bluntly informed the Italians that not even the seizure of Egypt and the Suez Canal could displace England from the Mediterranean; an inferior English fleet would defeat any superior Italian fleet "on the basis of their experience and tradition" (pp. 173, 174).

Close relations with the Italians did not come readily. Memories of Italy's defection from the Triple Alliance in 1915 died very slowly in the German navy, and it was not until 1926 that naval officers, in stark contrast to political leaders, embraced the Fascist regime in Rome—while actively working against democracy at home. In the process, the fiction of the "unpolitical" officer was once again exposed. Schreiber concludes that the German-Italian naval cooperation can best be depicted as "an alliance to negate the existing," that is, to drive England from the seas (p. 393).

The author asserts that in the absence of sufficient means with which to conduct a war at sea, German naval planning constituted little more than mental gymnastics (p. 12). Yet this "dreaming in continents," as General Franz Halder depicted it, reveals an incredible degree of continuity from the 1890s (Tirpitz) through the First World War (Hötzendorff, Levetzow, Trotha) and the victorious period of the Second World War (Fricke, Schniewind, Wagner). And it did not end even here. In Septem-

ber 1944, long after Hitler and his military paladins had conceded that the war was lost, Admiral Alfred Saalwächter penned a memorandum detailing Germany's needs for the next major war. He called again for German naval bases in the Netherlands and Norway, a "protectorate" over much of Belgium and France, coaling stations on Iceland and the Faeroe Islands, a "missile-launching base against America" on Greenland, advanced naval depots on the Azores, Cape Verdes, and Canary Islands, and a Central African colonial empire. In short, not even the horrendous devastation of 1944 could deter the fifty-year-old Machiavellian dreams of Germany's naval leaders.

Finally, Schreiber's work fits well into the genre of historical writings we have come to expect from the Military-Historical Research Office in Freiburg, and especially from Manfred Messerschmidt and Wilhelm Deist. It is the hope of this reviewer and others that they will be given the freedom to encourage such sober reappraisals in place of the apologies and hagiographies penned since 1945.

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ROY DOUGLAS. *The Advent of War, 1939-40*. New York: St. Martin's Press. 1978. Pp. xiii, 167. \$19.95.

This is an excellent summary of fourteen fateful months. In March 1939—more precisely, between March 17 and 21—the government of Great Britain changed its course. It had come to believe that the only way to avoid a second world war was to serve notice on Hitler that future German aggression against certain states would be a *casus belli* for Britain; and a British guarantee was offered to Poland. On May 10, 1940 the elected representatives of the British people demonstrated their preference for Winston Churchill as their prime minister; and the reluctant war began in earnest that day. Important milestones as these events are, they are especially dramatic turning points in the history of Britain. This book is written from that perspective, even though the author demonstrates his spacious comprehension of European international relations. It is principally a diplomatic history, dealing with the relations of states and of their governments, because of the author's reliance on the recently opened British state papers of the period. In this respect, Roy Douglas is more judicious than Sidney Aster, who wrote that these documents contain the vital evidence that "finally" reveals the making of the Second World War; he is also more judicious than A. J. P. Taylor, whose often brilliant but frequently superficial views made him write that the German demands on Poland were not altogether unreasonable.

Douglas has an excellent comprehension of the realities of international relations, perhaps especially in his last chapter, entitled "Retrospect," where he writes that no statesman, including Hitler, "behaved in a manner rooted in ideology, when that ideology ran counter to perceived immediate interests"—an argument of which his illustrations are well taken.

My only criticism of this fine little book is the author's evident underestimation of the American factor. As early as 1939 behind many of the deliberations and actions of the British government stood the inclination of increasing dependence on the United States and the determination of ensuring American sympathies for Britain. This was a factor even when it came to marginal areas of British interest as, for example, in the British deliberation about how to assist Finland. During the visit of the royal family to the United States in the summer of 1939, the first British suggestions were made about turning over certain British bases in the western Atlantic to the United States. No diplomatic historian ought to ignore the American element, even before 1940, when its importance became overwhelming.

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FRANK G. WEBER. *The Evasive Neutral: Germany, Britain, and the Quest for a Turkish Alliance in the Second World War*. Columbia: University of Missouri Press. 1979. Pp. ix, 244. \$19.50.

Wartime Turkey was a den of intrigue. From 1939 to 1945 agents and ambassadors of every power, cause, and faith operated against one another without remission and without remorse in the salons and along the open boulevards of Istanbul and Ankara. Turkey was not officially a participant in the Second World War, and, by remaining elusively neutral, it became a prized plum dangling just out of reach of both sides—a tradition of defensive foreign policy in which the Turks excelled since the 1699 Treaty of Karlowitz signaled the beginning of the retreat of Ottoman power. From its vantage point astride the straits and its ability to resist almost any military invasion, Turkey shamelessly manipulated both the Allies and, to a far lesser extent, the Axis. By diplomacy alone, Turkey maintained its territorial integrity, ultimately milking the Allies for substantial lend-lease, even while denying its friends in Berlin a long-sought-for alliance with the Arab world.

Frank G. Weber's new book, *The Evasive Neutral*, is an odyssey that begins after World War I—a seemingly logical continuation of the author's earlier volume, the masterful *Eagles on the Crescent* (1970). Yet the Turkish republic that survived the

First World War bears little resemblance to the far-flung Ottoman Empire of Weber's earlier book—with one notable exception. Although Atatürk declared in his speech on the abolition of the sultanate on November 1, 1922 that "Sovereignty and supreme power are won by force," Turkey's future was based, as always, on diplomacy and its ability to play off the interests and demands of its suitors. After Turkey's diplomatic victory at the Montreux Conference in July 1936, which allowed it to refortify the straits, its suitors were Britain and France on one side and Nazi Germany on the other. The result was a fascinating courtship dance between foxes, which lasted nearly a decade.

Of particular strength throughout the book is the exposition of Berlin's Middle East policy, such as it was, which Weber unravels in meticulous detail. Against a backdrop of economic missions, political machinations, and agents crisscrossing Europe with secret proposals, the Germans simply could not decide whether to support the Turks (for their strategic geographical position and militant anti-Soviet potential) or the Arabs (who would be geographically invaluable in a giant pincer movement against Russia and able to erode substantially British and French military strength in the Levant). Clearly, the Wilhelmstrasse could not befriend them both, and therein lies the key to Germany's failure in the Middle East. Each approach by the Turks or the Arabs caused Berlin to agonize, vacillate, and ultimately rebuff the advance. When Ankara finally, though half-heartedly, declared war on Germany in February 1945, resurrecting its long-abused 1939 mutual assistance treaty with Britain and France, the decision was brought about as much by Germany's muddled policies as by Ankara's diplomatic duplicity and opportunism.

One marvels at Weber's ability, in slightly more than two hundred pages, to analyze the convoluted efforts of both camps to enlist Ankara in their global struggle, while the object of their attention was playing each for the highest stakes. At the same time, Weber never loses sight of the shifting rivalries within the Arab world—underscored by Turkey's opposition to the unification of the Arab Middle East and by the latter's fear of "Kemalist imperialism." *The Evasive Neutral* is compact and tightly woven and based on impeccable research. The inclusion of maps would have been welcome. Until the Turkish government opens its archives to scholars, a distant prospect at best, Weber's *Evasive Neutral* will doubtless remain the standard work on this subject.

ARNOLD KRAMMER
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HELEN FEIN. *Accounting for Genocide: National Responses and Jewish Victimization during the Holocaust*. New

York: Free Press, and Collier Macmillan, London. 1979. Pp. xxi, 468. \$15.95.

This is an ambitious work, as any attempt at "accounting for genocide" must be. Although the accounting has its flaws, it is also one of considerable importance and, therefore, bound to be controversial. Helen Fein takes on the problem of Nazi genocide from outside the much-explored German perspective to examine in detail the roles played in the Final Solution by non-German peoples, including that of Jews themselves.

The finality of that solution, Fein explains, "has diverted attention from the fact that in almost half . . . the states occupied by or allied to Germany, fewer than half of the Jews counted there were killed" (p. xvi). That various peoples within the Nazi orbit reacted differently to Hitler's extermination policies has long been known. Many collaborated willingly, even eagerly; some few resisted outright. Why reactions varied so widely and the effect of these variations upon the number of Jews finally murdered are the principal problems taken up in this book.

How were Denmark and Bulgaria, where most Jews were saved, different from Croatia, Slovakia, or Poland where most of them were killed? The answer, the author contends, is to be found only by seeing the states and peoples under Nazi hegemony as parts in a "macrosystem of international power" that embraced also those powers allied against Germany. Then alone is the conceptual net cast widely enough to draw in all the possible variables. To keep track of all these variables, Fein employs, as she calls it, "various sociological modes of knowing," including "inductive, historical analysis . . . a *verstehen* approach . . . and statistical analysis, employing cross tabulation and regression analyses" (p. 36).

The yield from so many "modes of knowing" is often no more than what should have been obvious from the outset: the degree of a people's cooperation with the Nazis was determined largely by the extent of their prewar anti-Semitism and the immediacy with which they were under SS control (p. 82). But there were exceptions and Fein's theoretical structure is useful in explaining them. In the Netherlands, for example, there had been relatively little anti-Semitism, and SS control was certainly less immediate than in Eastern Europe; yet three of every four Dutch Jews died in extermination camps. By contrast, in Rumania, where a virulent anti-Semitism had long flourished and where the government, frustrated by Nazi slackness, initiated its own slaughter of Jews, over half of them ultimately survived.

Fein accounts for such seeming anomalies by identifying variables such as protests from the churches, the effectiveness of resistance movements,

the formation of Jewish Councils (*Judenräte*), conflicts within the Jewish communities, calculations about the likelihood of ultimate Nazi victory, and international intervention. To varying degrees and in various combinations, all carefully analyzed, these factors either impeded or facilitated the Nazis' murderous intentions.

Occam's razor might usefully have been employed to trim away some of the theoretical undergrowth in this work. Explanation and exposition would have been enhanced by the resulting economies. Nonetheless, this book warrants a careful reading. It is the first synthesis of Nazi genocide from the non-German perspective. Among its virtues is the widely scattered specialized literature it brings to our attention.

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KATHARINE R. FIRTH. *The Apocalyptic Tradition in Reformation Britain, 1530-1645*. (Oxford Historical Monographs.) New York: Oxford University Press. 1979. Pp. vi, 281. \$26.00.

The concern of recent scholarship with millenarian ideas in Stuart England prompted Paul Christianson and Katharine R. Firth to explore the Protestant apocalyptic tradition out of which this millenarianism developed. Like Christianson's *Reformers and Babylon* (1978), Firth's work is a revised doctoral dissertation. The books cover much of the same ground, commencing with John Bale's identification of antichrist with the papacy and his focus on the fulfillment of prophecy in history. Unlike Christianson, Firth displays little interest in the impact of apocalyptic thought on political events, preferring instead to explore in learned detail the views of the principal apocalyptic authors, including Joye, Foxe, Knox, Napier, Broughton, Brightman, Raleigh, Hakewill, and Mede. Of these, Christianson ignores Joye, Raleigh, and Hakewill and provides token mention of Knox and Broughton but does demonstrate the impact of apocalyptic ideas on such radical critics as Prynne and Lilburne. Essentially, the studies are complementary and substantially in agreement. For a thorough exposition of apocalyptic ideology Firth's is the better book, though Christianson excels in linking the theories to radical protest.

Firth argues that the apocalyptic tradition was revived by the Lutherans, though the example of Münster made many chary. Calvin demonstrated restraint with respect to apocalyptic matters but accepted the historical revelation of antichrist in the papacy. In England the Protestant apocalyptic tradition was established by Bale, Foxe, and Knox. Influenced by Joachim of Fiore, Bale developed the

connection between prophecy and history. "From then on, the two most important characteristics of the Protestant tradition were a history of Antichrist and a periodization based on a succession of apocalyptic images" (pp. 248-49). Foxe, believing that the millennium had begun with Constantine, wrote his magisterial study of the Church partly to ascertain the fulfillment of prophecies. (Firth and Christianson demolish Haller's thesis that Foxe conceived of England as the elect nation.) The application of prophecy in history was continued by Knox, but Napier, influenced by Ramist methodology, developed exegetical techniques that led him to the discovery of propositions on which he predicted the future.

Firth demonstrates that Broughton, Brightman, and Mede refined the schematization of the past and thus the expectation of forecasting the future. Brightman was the first (in 1609) to refer to England as the elect nation, a concept that became increasingly credible in the context of the Thirty Years' War. In the 1630s Brightman was interpreted as a harbinger of millenarianism, though Hakewill and others resisted such views. Millennial ideology was influenced by the Continental writers Andreae, Alsted, and Comenius and by the Englishman Mede. As these ideas spread, the English began to expect a millennial kingdom on earth rather than the end of the world predicted by the older apocalyptic authors. The change partly resulted from a decline in the historical tradition by the postponement to a future date of prophecies previously thought to have been fulfilled in the past. Historical study thus lost some of its relevance and was replaced by the imaginative projection of prophecies into the future. The chiliastic spirit was fanned from the pulpit and kept alive in Parliament through fast sermons. Although the revolution with its chiliastic overtones collapsed, the apocalyptic tradition was still being explored at century's end by Isaac Newton.

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CONRAD RUSSELL. *Parliaments and English Politics, 1621-1629*. Oxford: Clarendon Press. 1979. Pp. xxi, 453. \$36.50.

Conrad Russell's study of the Parliaments of the 1620s has been long awaited and does not disappoint. It is, as we should expect, a direct attack on the "winning of the initiative" thesis of the late Wallace Notestein and his successors. It is also, more obliquely, a reaction against recent arguments that long-term social forces underlay the political conflicts of early seventeenth-century England: there are no Parliaments of rising gentry or aristocrats in crisis here.

Both the lengthy introductory chapter (on the general structure of politics and the nature of parliamentary business) and the succeeding ones dealing with each of the six sessions between 1621 and 1629 exhibit Russell's authoritative mastery of the subject. The book provides by far the best consecutive account of parliamentary politics during this critical decade. Russell's superb control of his evidence is immediately apparent in a marvelous little introductory "Note on Sources," but it infuses the whole book and is buttressed by an equally impressive knowledge of the intricacies of parliamentary procedure. With a sophisticated understanding of the realities of Westminster politics and of their interaction with the more provincial concerns of the members, Russell is able to weave a complex tapestry of court intrigue, misunderstanding, and frustrated expectations. Even during this decade of increasing legislative paralysis, he shows that Parliament's function as a law-making institution should not be entirely neglected. And although proceedings in the Commons inevitably receive more space, he pays due attention to the House of Lords whenever appropriate.

Arguments from the author's earlier articles are here extended and elaborated. Russell rejects both the "conflict model" underlying Notestein's work and the teleological approach (the search for origins of the civil war) that has preoccupied so many other historians from Gardiner to the present day. The committee of the whole, we discover, was not a procedural device to enhance Commons' independence; the Commons were always reluctant to use supply as a lever for obtaining redress of grievance (even appropriation of supply in 1624 turns out to be a court scheme); while in foreign affairs they "were not seizing the initiative: they were evading it" (p. 81). In fact, Russell argues, the real scene of political debate was not Parliament but the court, and parliamentary politics was thus in the main an extension of court intrigue. In 1621 the attack on monopolies was not a move by the Commons against the court but an attack on some courtiers promoted by other courtiers. Impeachment was simply "a tool used by one Councillor to attack another" (p. 15), a subject which belongs "not to the history of ministerial responsibility, but to the history of court faction" (p. 104).

Historians, Russell believes, have exaggerated the sharpness of the court-country division. Most M.P.s, he points out, had a foot in both camps, a dual loyalty to their king and to their "countries"; and he makes much of G. R. Elton's suggestion that Parliament was in essence the point of contact, the conflict-resolving mechanism between Westminster and the localities. There is no doubt that the tension inherent in this relationship explains a lot of the political instability of the 1620s, for much as

they might have wanted to, M.P.s found it increasingly difficult to serve both a king who needed money for war and constituents who did not understand his admittedly often inept foreign policy. The 1620s, Russell does well to remind us, was a war decade, and neither Parliament nor the local administrative system was equipped for this: the result was "functional breakdown." Sheer accident, he argues, has more to do with the conflict than ideological conflict or the remorseless working out of social change: accidents like the "failure in communication" (p. 137) that led to the 1621 Protestation or the nervous atmosphere created by the plague in 1625. Once again Russell finds no continuity between the 1620s and the 1640s and concludes that the nation was not "profoundly alienated" before the outbreak of the Scottish troubles in 1637 (p. 426).

Russell's deployment of this argument is impressive. But even this magnificent book does not tell the whole story. Russell provides what might be described as the "court" view of politics. He is, to be sure, sensibly aware that Westminster represents only the tip of an iceberg, that Parliament was to a large extent a mirror of events taking place elsewhere. He makes a bow to Derek Hirst's recent study of the expansion of the electorate but does not discuss the implications of the Commons' anxiety to encourage an electorate independent of court or aristocratic influence. Outside events such as the growing unpopularity of courtiers and the heightening of religious debate also receive rather short shrift: until 1629, he supposes, even the rise of Arminianism was primarily a court issue. The consensus model, throughout, is overdone. Many M.P.s were clients of Buckingham and other courtiers, true enough; and Russell has some admirably shrewd comments on the nature and limits of patron-client relationships. But the unwary reader might all too easily suppose that these ties of dependence—of Harley on Buckingham, Rudyard on Pembroke, and so on—are a complete explanation for the political behavior of the Commons. When, therefore, we find ourselves in a new political universe in the Petition of Right debates in 1628, a universe no longer dominated by the court and clientage, the transition seems abrupt and not fully explained. Russell's treatment of the great debates of that session has many merits: he provides, for example, his usual excellent coverage of the Lords, and analyzes the Commons' central core of ideas about law and liberties very effectively. But he misses much of the drama and the excitement that breathes through the great speeches such as those of Rudyard and Phelps on March 22. On Russell's assumptions the final 1629 session is bound to " peter out into futility"; its final, almost revolutionary scene of passion and violence, when the speaker was forcibly held

down in the chair, becomes a painless anticlimax rather than a prelude to even more revolutionary events.

In Russell's view the Parliaments of the 1620s seem to differ little from the Parliaments of Elizabeth's reign. Yet there was court faction under Elizabeth, too, without Elizabethan councilors finding it necessary or convenient to impeach each other. The fact is that by the 1620s English society had changed, real differences had emerged over the meaning of laws and liberties, differences that were beginning to agitate people far away from Westminster. Russell wisely observes that the bonfires in the streets that greeted the king's acceptance of the Petition of Right may in the end be more historically significant than the document itself. His book is a brilliant analysis of how the politics of the 1620s looked to the court. But it all may have looked very different to the country.

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JOHN H. PRUETT. *The Parish Clergy under the Later Stuarts: The Leicestershire Experience*. Urbana: University of Illinois Press. 1978. Pp. 203. \$10.00.

This study of the Anglican clergy in Leicestershire from the Restoration to the death of Anne is replete with quantitative studies of social origins, incomes, educational and geographical backgrounds, and political leanings. The text is studded with individual examples, fully treated, and the variety of practice and situation is given full play. There are diverting details about tithes, patronage, and the like, that make for a readable discussion, which, though a local study, gives us a representative, panoramic view of the life and times of later Stuart clergy.

The conclusions are both firm and cautious. The author must have been disappointed after so much labor to have found nothing really surprising. The steady decline of clerics of plebian origin, noticeable since the Reformation, continued throughout this period; educational standards rose; and, because incomes were tied through tithes to agricultural produce and prices were rising, the clergy benefited. So, economically, educationally, and socially the position of the established clergy rose.

Yet enthusiasm tended to dim. The diminution may have been the reflex of comfort. But the intransigent Restoration Settlement cut the Church off from Puritan intensity; and the Act of Toleration of 1689 ushered in a further decline in fervor. Zeal was insidiously defrocked by secularism. The impression remains that most Leicestershire clergy were conscientious. We know how many, like Goldsmith's *Parish Priest*, were "passing rich with forty pounds a year," but we cannot discover how many were "to all the country dear."

All the proper and necessary questions have been asked, from the vantage of the social historian. The daily rhythm of life, the quintessence of doctrines as they were understood, the texture of teachings, and the soul and spirit of life in the parsonage are not much canvassed. That great shift from sacramental to institutional Christianity and from strenuous atonement to tepid ethical theology is left untraced. The kind of research needed for this book, indeed, diverts one's mind from such issues. The research is meticulous. As a result we have a useful study that, by documenting the rise of the clergy, gives us a clue to their alienation from the lower social orders—the copyholders and landless laborers whose position was surely deteriorating. The divorce from the masses, however, was a matter of mind as well as of money and class. In this complex divorce we can see the seeds of the modern Anglican tragedy, where, in so many serenely beautiful country churches, the vicar intones evensong to empty pews and preaches on Sundays to his wife and the verger.

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JOHN BOSSY. *The English Catholic Community, 1570–1850*. New York: Oxford University Press. 1976. Pp. xii, 446. \$24.95.

In his first book after many articles, John Bossy offers an interpretation of English Catholicism from the beginning of the Elizabethan mission to the restoration of the hierarchy. The word "community" in the title is crucial; this is not primarily an account of an institution or a set of beliefs but of a group of people with common loyalties, common behavior, and a common relationship to the larger communities of Catholicism and England.

The work has three sections and a concluding chapter. Part one traces the mental and institutional adjustment of Catholics, particularly the clergy, to the end of the medieval Church and to the new situation in which Catholics were one of several religious minorities in a country dominated by Anglicans. Part two is a portrait of English Catholicism from 1600 to 1770. It discusses the nature of the gentry household and its domination of the community; clerical roles, structure, and finances; the community's size and distribution; and the role of worship and ritual. Part three treats the expansion of the community between 1770 and 1850 (from native English as well as immigrant Irish sources) and the decline of gentry domination—both developments whose roots Bossy finds in the early eighteenth century. Bossy sees, following the decline of gentry control, a period of conflict between lively congregational participation in worship and authority and several forms of clericalism.

This conflict ended in the victory of episcopally centered clerical authoritarianism, which the hierarchical restoration of 1850 both effected and symbolized.

This is an extraordinarily good book. Bossy has great knowledge of his subject and a very creative mind—indeed, sometimes the rapid succession of ideas compels readers to go slowly if they want to absorb everything. Bossy also has just the right degree of sympathy for his subject; he cares strongly about English Catholicism and approves of some tendencies within it and not of others, but his sympathies rarely compromise his objectivity.

One cannot mention all of Bossy's theses in a short review, but some of the more important may be summarized. Bossy sees post-Reformation English Catholicism essentially not as a continuation of medieval Catholicism but as a new entity created by dissatisfaction with the Church of England. In this it was like other Nonconformist sects, and Bossy's work is conditioned by his view that "the Catholic community ought properly to be considered a branch of the English nonconforming tradition" (p. 7). A corollary of this view is that the English Catholics after the launching of the mission were not a dwindling remnant of a formerly dominant church but a sect that had virtually no active members before about 1570 and whose history from 1570 to 1850 was basically one of growth.

In a book of such comprehensive scope and intense thought it would be surprising if a reviewer could find nothing at which to carp. I am not, for example, fully convinced by Bossy's statistical arguments on the size of the community (chap. 8), and his discussion of the large role played by upper-class Catholic women in the fifty years after 1570 (pp. 153–60) may dismiss too easily the fact that women may have been freer than men in their religious behavior because their choices had fewer unpleasant consequences. Others will find other points with which to disagree. But Bossy's overall argument is extremely compelling, and his book should be read by anyone interested in the history of Christianity in Britain or Catholicism since the Reformation.

ARNOLD PRITCHARD
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HILLEL SCHWARTZ. *Knaves, Fools, Madmen, and that Subtle Effluvia: A Study of the Opposition to the French Prophets in England, 1706–1710*. (University of Florida Monographs. Social Sciences, number 62.) Gainesville: University Presses of Florida. 1978. Pp. 97. \$5.50.

This impressive monograph, part of a revised dissertation (Yale, 1974), focuses on the English reaction to the French prophets who arrived in London

in the summer of 1706 and startled that uneasy metropolis by their agitated behavior and disturbing visions. Hillel Schwartz contends that, by studying the twenty-six critics who published responses to three of these *inspirés* between 1707 and 1710, "one can trace assumptions about religious behavior which are guides to early eighteenth-century beliefs about the nature of all human activity" (p. 5).

It is an ambitious undertaking and a useful, though admittedly incomplete, addition to the growing collection of books on millenarianism. In a heavily documented work influenced by a wide range of contemporary scholars, Schwartz has asked pertinent questions about the theories of human and religious behavior; the nature of criticism and the meaning of metaphors; the number, age, sex, and socioeconomic standing of the prophets and their followers; the dynamics of individual and group behavior; the medical explanations of insanity and enthusiasm; and the interplay between memory and time in late Stuart England.

In "Backgrounds," Schwartz explains how the French prophets came to a society that was already much divided over reason and religion. He attempts to link political and religious tensions in the age of Queen Anne to economic and social undercurrents but oversimplifies here and admits that his treatment is abbreviated.

The heart of the book is "The Critique," in which the author analyzes the responses to the prophets according to the categories of animal spirits, effluvia (vapors), and delusions. Diverse critics attacked the prophets for threatening the order and balance in society that had been emerging gradually from Cartesian, Newtonian, and Lockean postulates. In order to preserve the rational basis of human behavior and the body politic, critics felt compelled to advance natural causes to explain the extraordinary agitations of the prophets. By insisting that behavior was rational and that extraordinary behavior was caused by natural and physical unbalances, however, the critics were undermining the role of ritual in religion and destroying the distinction between religious experience and everyday activity; consequently, they were preparing the way for the evangelical revival and the reassertion of the spiritual dimension to worship and being.

This is a demanding and rewarding work of concentrated scholarship. One can overlook the occasional cursory argument by looking forward to what Schwartz promises to be a larger work on the French prophets.

THOMAS W. DAVIS
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ROBERT E. TOOHEY. *Liberty and Empire: British Radical Solutions to the American Problem, 1774–1776*. Lexing-

ton: University Press of Kentucky. 1978. Pp. xiv, 210. \$13.00.

British radicalism in the age of the American Revolution has only recently claimed the attention it deserves. Robert E. Toohey now makes a useful contribution, a welcome companion piece to Colin Bonwick's *English Radicals and the American Revolution* (1977). Concentrating on the brief but important period between the Coercive Acts and the Declaration of Independence, the author presents "essentially a history of ideas" (p. 25). The central preoccupation is the opposition to Lord North's American policy brought forward not by establishment critics like Chatham, Shelburne, and the Rockingham Whigs but by a small number of political thinkers termed "radical" by subsequent historians in the nineteenth century. Alarmed initially at the inroads upon constitutional liberty they conceived the British government to have made in the Wilkes affair of the 1760s, radicals saw in the American troubles confirmation of their deepest suspicions that a corrupt political system in Britain aimed to erect a new despotism throughout the empire. Appalled at the scene before them, they embarked upon highly individualistic and uncoordinated campaigns to arouse the British public to the danger. Convinced that the Americans who resisted the North ministry were defending liberty in Britain as well as in the colonies, an extraordinary band poured out a turbulent stream of pamphlets, books, and communications to the editors of London newspapers. Existing on the periphery of political life—dissenting influence was strong among them, a fact Toohey does not sufficiently appreciate—the radicals shared only two common denominators: the deep sense that something had gone desperately wrong with Britain's political system and the commitment to reforming this degeneration.

Five prominent radicals and their works are analyzed in some detail; and the author successfully achieves a prime objective: "to show that America did not possess a monopoly on men [*pace* Catharine Macaulay] with innovative, enlightened concepts of liberty and empire" (p. x). John Cartwright foresaw the Commonwealth. Granville Sharp anticipated responsible government. James Burgh foretold the Age of Reform. Richard Price gave liberty, the real issue of the American Revolution as he saw it, a broader applicability than even Locke and the eighteenth-century Whigs had done. In such mighty intellectual company, only the works of C. Macaulay, romantic and emotional propaganda, seem out of place.

Toohey is careful not to give radicals too much credit. A certain prescience notwithstanding, they did not create a new order at home or within the empire. Their immediate significance, indeed, arose

from "some influence" they exerted on American revolutionary thought. Regrettably, however, the nature and extent of this influence is not sufficiently explored. That American leaders read British radical literature, corresponded and entertained the authors, and expressed warm and friendly sentiments about them is clear. This is evidence merely of a certain coincidence of views, however, not of formative influences on the Americans. By comparison, Bonwick is much more successful in showing the impact of American revolutionary thought on some of these same radicals.

CHARLES R. RITCHESON
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ALBERT GOODWIN. *The Friends of Liberty: The English Democratic Movement in the Age of the French Revolution*. Cambridge: Harvard University Press. 1979. Pp. 594. \$20.00.

Friends of Liberty provides a more detailed account and subtler analysis of English reformers in the last decade of the eighteenth century than have hitherto been available. Others have surveyed longer periods of radical development, but none have used as much fresh material from English metropolitan and English and French provincial sources. Though at all times aware of antecedent and succeeding movements, Albert Goodwin has focused on the nature and growth of reforming political thought in the 1790s in London, Manchester, Sheffield (where he himself was born and educated), Norwich, and briefly in Ireland and Scotland. The different factors like Nonconformist disappointment at the failure to obtain relief from the penalties of the Test and Corporation Acts, continuing Commonwealth traditions, the stimulus of celebration of the revolution of 1688, the upheavals in France, and the eruption of Thomas Paine on the scene are all disentangled. From such a set of circumstances, added to an awakening consciousness of social and economic grievances by men like Thomas Spence, sprang radicalism of various degrees and sorts.

Goodwin describes a bewildering number of societies and associations; their functions are clarified and distinguished from earlier groups. Motivation and temporary cooperation with organizations in Ireland roused by events in France and in Scotland, adversely affected by wartime exigencies, are noticed, though only tangentially to English ideologies. London reacted to parliamentary conservatism and to polemics like Edmund Burke's *Reflections* (1791), as well as to its own small but persistent liberal element; Norwich responded to the commemoration of the Glorious Revolution; Manchester polarized the aspirations of rational dissenters and those of middle-class Whigs. In Sheffield arti-

sans were prompted by a depressed economy to form working-class groups, thereby setting a pattern for future urban organizations.

Agitation took various shapes; protests were systematized, old or new tracts were printed, and meetings were frequent. The government viewed all of this activity, even on the part of a small minority, as seditious. Arrests, often followed by trials, were made on the most fragile of pretexts. The court actions of 1794 are vividly described here. More valuable, if less dramatic, is the distinction spelt out between followers of Paine and other radicals. Before 1800 few were, like the author of *Common Sense*, genuine republicans. Beginning as proponents of religious liberty and a reformed franchise that should elect a Parliament less adverse to change, critics of other aspects of the establishment—the landholding system, the inequality of wealth and opportunity—became increasingly vocal, if not numerous, and eventually bequeathed fruitful traditions to nineteenth-century propagandists. Significantly, acts absolutely prohibiting all workers' combinations were passed in 1799 and 1800. Their repeal represented the first major if delayed victory for radicalism.

All students of English politics in the age of the French Revolution will find this splendid and original volume invaluable. Even the overlong, repetitious, and irrationally arranged bibliography should not deter either specialist or general reader.

CAROLINE ROBBINS
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MICHAEL W. MCCAHILL. *Order and Equipoise: The Peerage and the House of Lords, 1783–1806*. (Royal Historical Society Studies in History Series, number 11.) London: The Society. 1978. Pp. x, 256. \$20.35.

Tocqueville liked to think of the English nobility as enlightened and intelligent. Michael W. McCahill shares that belief. He is not, to be sure, prepared to countenance all of the political foibles of the titled of the eighteenth century. He finds the peers bored with legislative responsibilities, inept at debate, and inordinately dependent for direction upon the newly minted brethren among them. But these disabilities, severe as they may seem, did not in McCahill's judgment prevent the nobility from playing a central and useful role in the political life of Great Britain: the peers were the great conservators or, to use McCahill's terms, the defenders of "order and equipoise."

McCahill's book is a study of the peerage in the age of the younger Pitt. Neither conceptually nor substantively does it break new ground. The author devotes chapters to such topics as the constitutional understanding of the peers; their service as legisla-

tors; their role in the great political shufflings of 1783–84, 1801, and 1804; and the nature of their political influence. His structural categories are as old as Namier: the peers of the realm are divided into king's friends, politicians, and independents. His major conclusions are equally unsurprising. Peers exercised wide political power by virtue of their landed wealth and their control of parliamentary boroughs. They were much like lobbyists in twentieth-century America, springing to action to protect specific, often local, interests. They justified these actions by reference to the need to defend the balanced constitution and the welfare of the community. Within the peerage the lawyers, the bishops, and the militia officers exercised preponderant influence. And whenever the peerage acted collectively as the House of Lords, it tended to submit to the authority of the few able men of affairs to be found in its ranks. In sum, McCahill simply confirms—though sometimes with arresting force—conclusions already widely accepted.

If the reader is disappointed at the tameness of these conclusions, she or he will be even more dismayed at some other aspects of the work. The opening suggestion that those historians are wrong who see the House of Lords as subservient to the crown is contradicted on almost every page: the author virtually admits as much when he tries to elevate the peers' unfailing readiness to support the monarch to the status of a disinterested principle. A similar discrepancy between conclusions and evidence arises when McCahill asserts that "the house of lords was not, during these years, the scourge of reformers" (p. 62). The reader's immediate doubts about that startling claim are not diminished when McCahill, seeking to justify it, cites as telling evidence such peripheral measures as regulations for hawkers and peddlers.

McCahill's work contains several useful appendices. For this reason, among others, students of the era will wish to consult it. But since the book neither justifies the Tocquevillian sensibility of its author nor alters the accepted view of the peerage in Pitt's day, the rewards will be fewer than readers would wish.

REED BROWNING
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GORDON C. BOND. *The Grand Expedition: The British Invasion of Holland in 1809*. Athens: University of Georgia Press. 1979. Pp. viii, 232. \$17.00.

This monograph deals with the Walcheren expedition of 1809. It is already well known that this operation was a fiasco. It was conceived as an effort to destroy French naval forces and dockyards in the Scheldt River and to relieve French military pres-

sure on Britain's Austrian ally. What was required for success was imaginative and bold leadership. Alas, instead of Wellington and Nelson there were only Chatham and Strachan. These officers share responsibility for failure with the government at home. Portland's ministry was singularly inept in devising a workable strategy; they also failed to realize that the expedition was headed toward disaster no sooner than it arrived off Zeeland. It collapsed amidst mutual recrimination between commanders, the dissolution of the government, and the ravages of diseases that incredibly incapacitated nearly half of some important divisions.

Gordon C. Bond has built his story around careful reading of both French and British sources. His strength lies in reconstructing the course of events. The book's merit is that it deals in great detail with all aspects of the invasion as it unfolded. The reader sees what each protagonist did. Though hardly revolutionary, this is useful work.

Yet there are questions that remain unanswered. Clearly, Bond is more familiar with French than British sources, an unfortunate circumstance given his focus. He fails to understand the conflict between Castlereagh and Canning or at least does not develop it fully in the context of the invasion. The chapter on the parliamentary inquiry into the failure is uninspired. It largely recapitulates the debate from public documents. What really transpired behind the scenes in light of ministerial changes? Does one explain the consistent government majority (in a full house by contemporary standards, a sure indication of the importance of the issue) solely on the merits of its case? Surely these are considerations of greater import than the movements of troops in Holland.

This, then, is traditional military and diplomatic history. London and Paris do things rather than British and French leaders. Broader questions such as the problems posed by combined military and naval operations are set aside. The result is an interesting but narrowly defined book of greater utility to the military specialist than to those who hope to find interpretations of larger issues. Some stylistic infelicities and curious references (such as calling the Treasury the "treasury office") contrast sharply with excellent maps.

CHARLES R. MIDDLETON
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PETER ALLEN. *The Cambridge Apostles: The Early Years*. New York: Cambridge University Press. 1978. Pp. x, 266. \$29.50.

Throughout the nineteenth century, student societies, especially debating clubs, served as an alter-

native system of education at Oxford and Cambridge. The rigidity of the formal curriculum, its exclusion of contemporary and controversial issues, and the university authorities' uncritical belief in the paramount value of examinations and competition created the vacuum that these clubs filled. Controlled by students, they tended to represent interests and values antithetical to the official system. Perhaps the most famous of these clubs was the Cambridge Conversazione Society, popularly known as the Apostles. The society is best known as the Cambridge origin of the Bloomsbury group, but its earlier history also included an impressive array of Cambridge intellectuals, from Frederick Denison Maurice, John Sterling, and Alfred Tennyson in the 1830s to Henry Sidgwick, Bertrand Russell, and G. E. Moore in the late nineteenth and early twentieth centuries.

Peter Allen argues in this new book that the society has been characterized by an enduring tradition of intellectual liberalism. The "Apostolic spirit" included a belief in unrestricted debate and in personal friendship unaffected by doctrinal differences, although it also contained a strong tendency to view with disdain all persons outside the clique. *The Cambridge Apostles* traces in detail these tendencies in the society from its inception in 1828 through the 1830s.

The real founder of the "Apostolic spirit" was F. D. Maurice, and Allen relates his abhorrence of rigid ideologies to the bizarre religious extravagances of his family, buffeted between a muddled Unitarian father and hysterical Calvinist sisters. In reaction, Maurice developed a Broad Church theology that emphasized the importance of unity and sympathy and rejected sectarian narrowness. Allen argues that the liberal spirit of Maurice's thought pervaded the society and that this spirit connected his "Apostolic theology" to Henry Sidgwick's faith in "social science" and to the estheticism of Bloomsbury.

The Cambridge Apostles raises a troubling question: how convincing is Allen's conception of an "Apostolic spirit" that has endured throughout the one-hundred-and-fifty-year history of the society? Liberalism means very little when it is stretched to cover so many different intellectual movements over so long a period of time. Often it seems that Allen is either straining to make connections or merely chronicling the disparate careers and activities of a group of college friends.

Nonetheless, it is clear that the society was the most important part of many Apostles' Cambridge education. It created the personal friendships and intellectual contacts that were to last a lifetime. The society was also of considerable practical value in furthering the careers of many Apostles by bringing promising young men to the notice of more estab-

lished intellectual figures. In *The Cambridge Apostles* we have a valuable case study in the concrete working of the social network that created what Noel Annan has called "the intellectual aristocracy of Victorian England." One may be skeptical of the existence of an enduring "Apostolic spirit," but one must recognize the importance of the society in connecting the university to the greater intellectual world. It would be interesting to know if other clubs (especially political ones) at Oxford and Cambridge performed analogous functions and how twentieth-century changes in the formal curriculum have affected these institutions of student self-education.

ARTHUR ENGEL

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PETER MARSH, editor. *The Conscience of the Victorian State*. Syracuse: Syracuse University Press. 1979. Pp. viii, 257. Cloth \$16.00, paper \$6.95.

"There is nothing in the whole world so unbecoming to a woman as a Nonconformist conscience," observes a character in Oscar Wilde's *Lady Windermere's Fan*. This volume suggests that not only Nonconformists and Gladstone but also other Victorians had a conscience and that, on balance, it was most becoming to them.

The contents of some of the essays, admittedly, throw doubt both upon that assumption and upon the title of the collection. As Joseph Hamburger observes in "The Whig Conscience," one finds in Thomas Babington Macaulay, the best-remembered Whig spokesman, a disdain for religiosity and "a fundamental rejection of public conscience" (p. 22). "Was the Nonconformist conscience rooted in a coherent view or philosophy of society and politics?" asks Richard Helmstadter in a related essay. By the 1890s, "clearly the answer must be negative" (p. 139). Yet, the leaders of British dissent had upheld a distinct ethos from the 1830s to the 1880s, a set of values based upon a theology of evangelicalism, a tradition favorable to liberty of conscience, and a record of significant if piecemeal political achievement. Paradoxically, at the very time that dissent became commonplace and that the dissenting bodies became most effectively organized, both their theology and their political *raison d'être* faded, and only the (for Helmstadter more trivial) dangers of wine, women, and gambling continued to rouse a united turn-of-the-century "Nonconformist conscience."

Helmstadter's is one of the two best essays in the volume. The other is David Roberts's "Utilitarian Conscience," a well-written, well-organized, and persuasive account of the parliamentarians, civil servants, and journalists who in the 1830s and 1840s sought to apply the "greatest happiness principle"

to politics. As Roberts demonstrates, that principle was at once potent, simple, and vague, and in practice the Utilitarians proved to be not Gradgrinds with an account-book mentality but men sufficiently concerned with "justice," with a hatred of tyranny, and with the possibilities of a reforming state to "impose on the Victorian ruling classes a most formidable public conscience" (p. 69). Roberts's essay is also well documented, in contrast to several others in which the documentation is curiously erratic.

The essay by John Cell is less concerned with "The Imperial Conscience" than with the manner in which the groups that, in the early nineteenth century had abolished slavery and the slave trade and had sought to impose the concept of trusteeship upon the government of India continued to influence relationships that, in an ultimate sense, rested upon military force and exploitation. Marsh's "Conservative Conscience" is taken up largely with Peel and Salisbury. For the latter, the defense of property and of the Established Church was "permeated with ethical considerations," and he "reinvested Conservative party loyalty with moral spine" (p. 232). Deryck Schreuder's "Gladstone and the Conscience of the State" is the longest and most ambitious but least successful of the essays, although the author illustrates the manner in which the statesman supplanted his youthful ideal of a confessional state with the vision of a public conscience that resided in "the Christian people of England at large" (p. 101).

Despite Marsh's illuminating introduction, some of the contributors find their subject elusive, and readers will remain unconvinced that "the Victorian state" had a conscience. They will be more readily persuaded that scores of Victorians indeed possessed consciences and that when they pondered the implications of public morality they were as likely to think of politics as of sex.

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KEITH ROBBINS. *John Bright*. Boston: Routledge and Kegan Paul. 1979. Pp. xvi, 288. \$21.50.

Keith Robbins has written a fair and informative biography within the word limit he or his publisher has set, but he does not explain fully all of the paradoxes in John Bright's career. An independent study based largely on original sources, it pays rather less attention to the secondary literature that has appeared in the field in the last few decades. No substantial biography of Bright has appeared since G. M. Trevelyan's official life was published in 1913. Some years ago Robbins wrote a new life of

Sir Edward Grey, a rather more scholarly study than the official biography also by G. M. Trevelyan. The present task is more difficult, however, for Trevelyan's life of Bright was much better than his *Grey of Fallodon*. Trevelyan may have been a little too partial to Bright, who was a friend and a parliamentary colleague of his father, but he made good use of the Bright papers and had great literary skill, regarding history, in his great uncle's tradition, as a branch of literature. Robbins's claims in the latter respect are more modest, and he is also at the disadvantage that Trevelyan used almost twice the space and gave his reader far more in the way of quotation from Bright's letters and speeches. There are, however, some areas in which Robbins has gone further, perhaps because his research was wider. Thus we learn more from Robbins about Bright's second marriage, the curious courtship, the long separations, the tendency for each partner to go his or her own way (although Bright, to the historian's advantage, was a most faithful correspondent). He was always protesting his loneliness in London but seemed to have had no compunction in traveling without Mrs. Bright on numerous fishing trips to Scotland, on some Continental tours, and in later life on visits to the homes of some of the great aristocrats whose order he had attacked so severely. Robbins also has a fuller account of Bright's breakdown in the 1850s and of the steps he took to recuperate. On the other hand, on most important topics Trevelyan's treatment is fuller and includes more detail.

There are two areas where one might have hoped a modern biographer to go beyond Trevelyan: first, Bright's business affairs and their relationship to his career and, second, Bright's role in the history of the Liberal Party and particularly of its radical wing. Neither aspect is ignored, but in each case one might have hoped for a fuller treatment. When Bright first entered Parliament it was clear that he regarded himself virtually as outside of party. Yet in 1875 Bright chaired the meeting that elected Hartington Liberal Party leader in the Commons and fully agreed with the decision. More attention might have been paid to the transformation that took place in the 1860s and also to Bright's relationship with other elements in the radical wing of the party. There are references (pp. 138, 164, 173) to Bright's concern for the party in the years 1858, 1862, and 1863, but they are quite incidental and bear development. Robbins might also have explored further the circumstances that led Bright to accept office from Gladstone in 1868, for there is evidence that Bright put pressure on Gladstone to change his mind on the ballot.

One might quibble about a few minor points. The India Act of 1854 (p. 98) did meet Bright's criticism of patronage as Trevelyan notes; Palmerston

introduced an India Bill in 1858, but it was never passed (p. 135); Gladstone won his Greenwich seat before he was defeated in Lancashire (p. 219). On the whole, however, the book is a well-researched and balanced but not unsympathetic interpretation of a unique Victorian, and we may agree with Robbins's final conclusion that whatever Bright was he was undoubtedly "his own man."

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NORRIS POPE. *Dickens and Charity*. New York: Columbia University Press. 1978. Pp. xi, 303. \$16.50.

Norris Pope's title misleads. Those seeking further information about Dickens's attitudes toward charity as *carietas*, and those wanting details of Dickens's own charities or of his close association with leading philanthropists like Angela Burdett Coutts on Urania Cottage and in Columbia Square, should consult Dickens's letters and speeches, the Coutts archives, the work of Humphry House and K. J. Fielding, and Edna Healey's recent biography of the baroness. Nor will one find in Pope's monograph an examination of the role of charity in Dickens's philosophy or fiction: there is no extended discussion, for instance, of the satire on the poor laws in *Oliver Twist* or of the complex vision of private and public responsibility fundamental to *Bleak House* and *Little Dorrit*.

Instead, Pope's "charity" is Evangelical philanthropy, and his focus is on Dickens's ambivalent response to Exeter Hall. Pope divides the topic into five sections: an overview of Dickens's quarrel with narrow, presumptuous religionists; Dickens's lifelong opposition to sabbatarianism; his attacks (especially in *Bleak House*) on the misguided efforts of missionary societies; his qualified endorsement of the evangelically dominated ragged school movement; and his useful if limited propaganda for sanitary reform and improved housing. Each chapter is densely packed with information about prominent evangelicals, their reformist objectives and strategies, and the institutions they founded, such as the Lord's Day Observance Society, the Ragged School Union (RSU), and the Society for Improving the Condition of the Labouring Classes. Juxtaposed to the flourishing and waning of these bodies are Dickens's public speeches and *Household Words* articles (particularly those denouncing Lord Ashley's Sunday Post Office Address and supporting the Board of Health and its public relations agent, the Metropolitan Sanitary Association), his letters to influential friends like Coutts, Macvey Napier, Southwood Smith, and his brother-in-law Henry Austin, and key passages from a few novels.

Pope's extensive research into the archives of the

RSU and the pages of the leading Low Church journal, the *Record*, yields dozens of helpful quotations about these mid-Victorian voluntary efforts to ameliorate specific social evils and illustrates both the hopeful beginnings and the mixed consequences of attempts to deal symptomatically with urban poverty, ignorance, vice, overcrowding, and disease. Dickens's ardent concern for the poor often led him to support these evangelical causes while steering clear of narrow sectarian emphases. For example, he frequently praised ragged schools like the one in Field Lane, even after Mayhew's sharp attack in 1850 on the RSU in Dickens's old paper, the *Morning Chronicle*. Dickens differed from ragged school activists in deploring the time wasted in classrooms on deadly dull theological controversies or flopping in prayer, and he always thought of philanthropic education projects as transitional until comprehensive national programs could be enacted. On the ragged school movement, as on health and housing, Dickens's involvement was extensive, discriminat-ing, and developmental.

The later chapters of this book contain more of interest than the former (where Dickens's attitude is predominantly critical and satiric) and better integrate historical narrative with Dickens's response. An important question raised by this study is the extent of Dickens's influence on mid-Victorian social reforms. *Dickens and Charity* does not attempt to settle the issue, but it opens and closes with quotations that define the poles of Victorian opinion about it. The first, from a nonconformist preacher to his congregation, asserts that "there have been at work among us three great social agencies: the London City Mission; the novels of Mr. Dickens; the cholera." The last, from Shaftesbury's diary, affirms public awareness of Dickens's social conscience: "God gave him . . . a general retainer against all suffering and oppression." But the old veteran, battle-scarred, went on to confess sadly, "And yet, strange to say, he never gave me a helping hand—at least, I never heard of it."

ROBERT L. PATTEN
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RAYMOND G. COWHERD. *Political Economists and the English Poor Laws: A Historical Study of the Influence of Classical Economics on the Formation of Social Welfare Policy*. Athens: Ohio University Press. 1977. Pp. xvii, 300. \$15.00.

No one can complain that the English poor laws have been neglected by historians. Scholarly studies of the theory and practice of poor relief continue to proliferate, making ever more daunting the task of a new synthesis to replace that of the Webbs. Ray-

mond G. Cowherd's study is concerned with the intellectual background of the New Poor Law of 1834, and covers much the same ground as J. R. Poynter's *Society and Pauperism* (1969). Comparisons are thus inevitable, with results that are clearly in favor of Poynter. Cowherd's book is marred by structural and interpretive defects that render it less useful to the specialist and sometimes misleading to the unwary general reader.

There is much valuable biographical information here on both the major and minor figures of eighteenth- and early nineteenth-century Britain who grappled with the thorny problems of poor-law reform. Indeed, the title of the book does not adequately convey the range of thinkers covered. Not only are classical economists like Malthus, Ricardo, and McCulloch discussed but also a number of evangelicals, utilitarians, and political leaders. A central problem, however, is that the author has a passion for attaching labels to virtually every figure he discusses. The generation or so before 1834 is seen as a great debate among humanitarians (led by evangelicals), natural-law reformers, and philosophical radicals. An attempt to adhere too rigidly to this classification obscures the real complexity of the debate in which many thinkers, as well as the public, combined influences from all three groups as well as from other value systems and schools of thought.

The same problem is evident in the author's attempt to label the reforms of the period. Thus Gilbert's Act of 1782 and Pitt's reforms of the 1790s are described as humanitarian, Sturges Bourne's Act of 1819 is said to proceed from the natural-law reformers, and the New Poor Law is treated as a piece of applied philosophical radicalism. Such a classification ignores many elements of continuity linking those statutes, like the grouping of parishes and the shifting of power from the smaller ratepayers to the major tenants and landowners. There was undeniably a growing spirit of deterrent rigor in the nineteenth century, cumulating in the workhouse test and the principle of less eligibility, but the author exaggerates the shift. This is compounded by a factual error (p. 276)—that the New Poor Law stopped all outdoor relief effective June 1, 1835, when actually this provision was deleted by the House of Lords. From the author's account of the 1834 statute, a general reader would surmise that only in dire emergencies did the able-bodied poor receive outdoor relief—a gross distortion, as anyone familiar with the recent literature on the act's implementation can attest.

There are, indeed, a number of significant omissions from the author's bibliography, most notably on the subject of the "revolution in government." If Cowherd had taken into account other scholars' contributions to this subject, he might have avoided

the rigid categories and distortions that detract from an otherwise useful book,

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ANNE DZAMBA SESSA. *Richard Wagner and the English*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1979. Pp. 191. \$13.50.

In the closing pages of *Richard Wagner and the English*, Anne Dzamba Sessa concludes, "The fact is that Wagner was a cultural phenomenon; his music dramas seemed to offer philosophic and psychic inspiration to late Victorians in a changing and challenging era." The pages that precede this conclusion attempt to illustrate the kinds of inspiration that Wagner's music dramas offered to the British between the 1850s and 1914 and the kinds of admirers, of widely varying talents, who were duly inspired. Bernard Shaw gets a chapter to himself, as does an obscure and pretentious pseudo-philosopher named David Henderson Irvine. The "literati" are squeezed together in another chapter, so that Swinburne, Beardsley, George Moore, and Arthur Symonds are tossed in among the likes of John Payne, John Davidson, and Theodore Wratislaw, an English decadent poet of Polish origin. Finally, there is a chapter on the English response to *Parsifal* in light of the late-nineteenth-century search for surrogate religions to replace the Christianity undermined by science and Biblical scholarship.

Along the way there are some valuable observations. The author rightly emphasizes that Wagner represented different things to different people—prophet of socialism to Shaw, champion of sensualism and eroticism to Beardsley, critic of the conservative establishment to Irvine, and, to some Theosophists, chronicler of man's evolution toward perfection. Sessa explains clearly the importance of myth in Wagner's work and makes a persuasive case that Wagnerism must be included among the numerous forms of escape from scientific materialism that flourished in the late nineteenth century.

These nuggets, however, are unfortunately wrapped in an unprepossessing package. The author's favorite stylistic device is the list, which she employs at every opportunity. There are lists of authors who cite Wagner in their writings, lists of Wagner citations in the works of individual writers, and a lengthy list of the contents of the *Meister*, a short-lived English journal devoted to the study of Wagner. All too frequently, lists replace genuine analysis; an author's indebtedness to Wagner is "explained" merely by listing all references to Wagner in that author's work. And when analysis is ventured, the author tends to rely heavily on clichés of

late Victorian intellectual history. We are told, for example, that the age was "increasingly uncertain and dissatisfied with itself" (p. 49) and that it suffered from a "spiritual vacuum" (p. 134), but scarcely any effort is made to put fresh meaning into these tired phrases.

JANET O. MINIHAN
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HAROLD ISSADORE SHARLIN. *Lord Kelvin: The Dynamic Victorian*. In collaboration with TIBY SHARLIN. University Park: Pennsylvania State University Press. 1979. Pp. xiv, 272. \$16.00.

William Thomson, Baron Kelvin, was a seminal figure in Victorian science. His six hundred and sixty-one published articles (at least two annually from 1841 through 1907) touched on nearly every significant physical problem of the age and much else besides. His skill at applying abstract mathematical physics to practical technology made transoceanic telegraphy possible and greatly improved marine navigation. His *Treatise on Natural Philosophy*, written with P. G. Tait, transformed the teaching of physics in Britain and much of the rest of the world. But more important than any of these, his personality and the extraordinary fertility of his ideas provided inspiration for three generations of physicists. As an eminent younger contemporary remarked, an account of nineteenth-century physics that failed to deal with Kelvin "would be like the play of *Hamlet* without the Prince of Denmark."

Kelvin's reputation, unparalleled in his own time, has suffered a severe eclipse in ours. He contributed significantly to many fields of science but produced no lasting synthesis in any. His contributions to electricity and magnetism were overshadowed by those of Faraday and Maxwell, his work on thermodynamics by that of Clausius and Joule. And he has never been forgiven his "wrong-headed" opposition to the theories of Maxwell and Darwin. Rather than the Prince of Denmark, he has been relegated to the role of Polonius—necessary for the plot, sometimes quaint, but ultimately an obstacle to the central characters.

Harold Issadore Sharlin's admirable aim is to change this state of affairs. "I have tried," he says, "to integrate the subject's life and work and to place the result in a broader historical context that would illuminate the history of science and technology in the nineteenth century" (p. ix). Unfortunately the effort fails. Readers of this book will find little to explain Kelvin's reputation among his contemporaries and still less to clarify his position within the larger framework of Victorian science. By basing his account almost exclusively upon Kelvin's own correspondence and published works, Sharlin has put

blindness on his perspective. The world outside of Kelvin's immediate circle is ignored and only the most obvious of his interactions with other scientists are examined. No mention is made of the scientific organizations that played such a large part in Kelvin's life and in which he played so important a role. In the few instances where Sharlin attempts to deal with Kelvin's contemporaries, the selection is erratic and is rarely focused sharply on the issues at hand.

Even when dealing with Kelvin himself, Sharlin paints a curiously unbalanced picture. A third of the book is devoted to Kelvin's life before he became professor at Glasgow at the age of 22. (He lived to be 83.) More space is devoted to the political wrangling of Kelvin's father in securing the Glasgow chair for his son than to Kelvin's seminal work on thermodynamics or his revealing objections to Maxwell's electromagnetic theory. The correspondence with Tait over the writing of their textbook is quoted in detail while years of intense professional activity are skipped over with hardly a notice. The treatment of Kelvin's mature life is fragmented and two dimensional—an occasional vignette inserted in a stream-of-consciousness organization—while the heart of the book, the analysis of his scientific work, is often obscure and confusing.

To Sharlin's credit, he has tried to avoid hero-worshipping his subject. He is at his best in narrative accounts such as his treatment of Kelvin's activity on the Atlantic cable project and in dealing with integrated bodies of correspondence such as that between Kelvin and G. G. Stokes. But these virtues cannot save this disappointing book. When we understand Kelvin's role in nineteenth-century physics, we will have greatly enlarged our knowledge of Victorian science, but this book contributes little toward that end.

JOE D. BURCHFIELD
Northern Illinois University

ROBERT STEVENS. *Law and Politics: The House of Lords as a Judicial Body, 1800-1976*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1978. Pp. xviii, 701. \$30.00.

This is a history of the House of Lords extending over almost two centuries and centering on the judicial function of that house. Robert Stevens has taken a broad view of his subject, considering not only the House of Lords as the final court of appeal in the United Kingdom but also the ways in which the development of the law was shaped by the political and social outlook of the law lords. The outcome is a substantial volume that tells much about the law and the law lords, but less than one would

like to know about the relationship between their views of their judicial function and the major turning points in the history of the House of Lords. That is, *Law and Politics* is more successful when Stevens writes about the law than when political history pre-empts the stage. Unmistakably, however, this is a major piece of work, representing a vast expenditure of time and effort, and Stevens has discussed in considerable depth an area of legal history that was until now unmapped and uncharted.

The main theme is that the House of Lords—after it became clear in 1844 that the law lords alone could vote in judicial matters—gradually ceased to be a political body and became increasingly “scientific” in exercising its judicial function. The more political phase, which culminated in the Parliament Act of 1911, was in the late nineteenth and early twentieth centuries when Salisbury, Balfour, and Bonar Law led the Conservative Party and Lord Chancellor Halsbury guided the law lords. It came to an end with the advent of the Liberal Lord Chancellors Herschell and Loreburn, who for their own party reasons started the House of Lords on the path to what is called “substantive formalism.” The term means that the law lords acted as if the courts should be thoroughly subordinate to the sovereign legislature. Lord Chancellor Haldane carried the trend further in the name of scientific objectivity, and thereafter judicial restraint was increasingly the order of the day. Only in the last twenty years have the law lords proved receptive to the idea that judicial decisions constitute a legitimate form of law-making. Two law lords, according to Stevens, were of special consequence in this connection. A major effort was made to arrest the decline of the House of Lords as a judicial body during the chancellorship of Lord Kilmuir (1954-62); and, in the subsequent reversal of substantive formalism, Lord Denning played a key role when he “trumpeted the lawmaking potential of the judiciary from the rooftops” (p. 490).

An obstacle to this kind of study rises out of the dearth of scholarly monographs that would permit greater precision in interpreting the history of the House of Lords. As a result, Stevens tends to rely excessively on the histories of the period associated with R. C. K. Ensor, Trevor O. Lloyd, and Arthur Marwick—works too general for his purposes. To be sure, he has undertaken his own research at certain points, as in an interesting description of the retention of the lords' appellate jurisdiction in 1876. Yet there are places where more information would be helpful. For example, the statement that the mixed constitution was effectively at an end by 1830 requires more elaborate explanation than is given here. After all, Olive Anderson, writing in the *English Historical Review* (1967) about the Wensleydale peerage case of 1856, attributes the failure to in-

roduce life peerages at this time in large measure to the great popularity of the theory of the mixed constitution. The proposed creation by Queen Victoria was opposed as a threat to the vaunted balance among king, lords, and commons. Under these circumstances Stevens's language is hardly a preparation for Anderson's description, and the discrepancy should be ironed out. But these are relatively minor matters in a work of this scope. As earlier noted, *Law and Politics* is a welcome addition to modern histories of the House of Lords, especially for those who believe that that house merits a larger place in the literature of parliamentary institutions than it has hitherto received.

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ARTHUR DAVEY. *The British Pro-Boers, 1877-1902*. Cape Town: Tafelberg. 1978. Pp. 220. R 13.00.

Arthur Davey states that his purpose is "to give a comprehensive history of the British pro-Boers, not only in the major phase of 1899-1902 but also in the preceding quarter century during which the phenomenon evolved." He defines the British pro-Boers as "those citizens of the United Kingdom who by their utterances, writings or other actions showed recognizable signs of sympathy for or understanding of the South African Dutch or Boers" (p. 2). His preface carefully relates the work to other scholarship. There is a very full bibliography, a note on sources, and an excellent index. The extensive footnoting is conveniently at the bottom of the page. Dutch quotations in the text are translated in the footnotes. South African archival materials were thoroughly exploited and relevant private papers in Britain and in South Africa, as well as the most useful newspapers and periodical sources, were consulted. For someone interested in the Boer War period, the book, therefore, has interest and value.

Workmanlike the study certainly is, but there are limitations imposed by its task. The obligation to be comprehensive leads to an almost encyclopedic style. Fascinated as he is by the activities per se of the pro-Boers, Davey eschews theory and does not really analyze ideas. The excitement of debate is absent from the book. What is new and interesting, consequently, is to be found in the richness of detail and not in the interpretation. The author assumes moreover, that his readers have considerable knowledge of the Boer War and British history. The general reader is told too little to comprehend many of the events animating the pro-Boers, which are referred to but not explained (for example, the Jameson Raid and inquiry, prewar diplomacy, even the conduct of the war itself).

Davey does draw comparisons between 1877-81, when retrocession did occur, and 1899-1902. Missing in 1899-1902 was a figure of Gladstone's stature to restrain rampant jingoism. Anti-Boer propaganda was skillful; the Boer ultimatum and early invasions and annexations of British territory were hard to excuse. Some pro-Boer activity was extreme and counterproductive. The pro-Boers thus failed to moderate British prewar diplomacy or to prevent the annexation of the Boer republics. They had little effect on the course of the war, and only after the burning of farms, the concentration camps, and the frustrating prolongation of the war had worked on public opinion could Henry Campbell-Bannerman and others wear down the policy of unconditional surrender. The pro-Boers may, nevertheless, have underlined for Britain the anti-imperialist lessons of the war, and their concepts of conciliation and their faith in self-government were ultimately applied by Campbell-Bannerman when he became prime minister in 1906.

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GREGORY D. PHILLIPS. *The Diehards: Aristocratic Society and Politics in Edwardian England*. (Harvard Historical Studies, number 96.) Cambridge: Harvard University Press. 1979. Pp. viii, 288. \$18.50.

Until recently the British right in the Edwardian period has, with the exception of Bernard Semmel's pioneering work, been sadly neglected or shallowly dismissed. Controversies about the emerging labor movement and the state of liberalism and the Liberal party have absorbed the energies of most historians. Of late, works by Springhall, Ramsden, Searle, and Steiner have, in various ways, perceptively treated aspects of the right on both the ideological and the active political levels. We are treating more seriously than before the influence of such organizations as the National Service League and the Boy Scouts as well as examining in detail the Germanophobe press. Gregory D. Phillips's book on the Diehards is a valuable, meticulous addition to this growing if still sparse literature.

Phillips is the arch revisionist, attempting to render respectable the most maligned group of aristocrats in modern British history—the so-called "Backwoodsmen" who voted against the 1911 Parliament Bill and whose rhetoric added to the "sense of impending clash" that pervaded Britain in the period 1911-14. By arguing that the Diehards were hardworking, serious politicians acting from considered intent and not "instinctive reaction," Phillips goes against most contemporary accounts as well as the interpretations of historians such as George Dangerfield and Roy Jenkins.

The most valuable and original parts of the book are those chapters that marshal data and arguments to show that the 112 Diehards were, on the whole, active politicians who better than most of their class had adapted successfully to the economic and political challenges of the late Victorian period. Phillips argues convincingly that their intransigent behavior in 1911 cannot be attributed to frustration at the local level, where they still held sway, and must be attributed to their attitudes and activities on national and imperial affairs. In these areas, the Diehards differed from fellow aristocrats primarily "in their persistence, the strength of their commitment and the leading role which many of them took in military and imperial controversies." Even here Phillips claims that the Diehards were selective and not absolute in their resistance to change.

When describing Diehard ideological beliefs, opinions, and prejudices, Phillips is less convincing. The litany of their faith in tariff reform, conscription and "efficiency" and of their abhorrence of democracy, socialism, and liberalism does not supply the much needed reflective work on conservative thought in prewar Britain. Were not the Diehards responding to immediate issues arising from the Liberal victory of 1906 rather than a school of thought? Phillips attempts to give ideological coherence to the Diehards by classifying them under the term "Radical Right," which Lipset and Bell in the 1950s tried to apply to groups at the extreme right in the America political spectrum. Milner and a few other Diehards might qualify for the label "Radical conservatives rather than simple reactionaries" but most of the 112 certainly do not. Moreover, leaders such as Milner and Selborne were Liberal Unionists and not Conservatives.

On a methodological level problems arise from Phillips's concentration on the 112 peers while neglecting their Diehard comrades in the Commons. He might also have noted how effectively the industrialist Bonar Law tamed the Diehards where the aristocrat Balfour failed. Finally, while acknowledging Diehard willingness to accede to civil war before 1914, particularly over Ireland, Phillips underplays the destructive impact they had on British political life by 1914 in his eagerness to convince us that the Diehards were adaptable and farsseeing on some issues.

RICHARD A. REMPEL
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JOE GARNER. *The Commonwealth Office, 1925-68*. Exeter, N. H.: Heinemann. 1978. Pp. xix, 474. \$60.00.

Joe Garner joined the Dominions Office in 1930—five years after Leo Amery had created it in an attempt to persuade South Africans, Irishmen, and others that, as they were no longer administered like "subject peoples" under the Colonial Office, they

were indeed fully autonomous nations freely associated in the British Commonwealth. Garner retired from the Commonwealth Relations Office in 1968 on the eve of its merger with the Foreign Office. He is therefore in an unusual position from which to write an insider's "house history."

Compared with the official histories of British government departments that come to mind—Hall, Parkinson, and Jeffries on the Colonial Office, for example—Garner's book is a large and ambitious undertaking indeed. Here to be sure are the in-house anecdotes, the brief portraits of ministers and fellow colleagues, the discussion of office routine that one expects from such a work. Garner, however, has taken seriously the task of setting his office in the context of what it was doing: to try to keep the Commonwealth together through delicate, painstaking, and necessarily colorless diplomacy with a group of extremely sensitive and aggressive junior partners. Up to the termination data set by the thirty-year rule, he has explored in some detail the documents on such central problems as Ireland, the abdication crisis, imperial preference, wartime cooperation, and the transformation of the white man's club after World War II. This material has been covered thoroughly and well by Mansergh and others; quite rightly Garner frequently cites secondary as well as primary sources; and there are few surprises.

Of greater interest is Garner's inside perspective on more recent topics such as Suez, relations with the independent African countries, and Rhodesia. Here again he mainly confirms the contemporary official statements: he agrees with the late Iain Macleod that there was no planned acceleration of the march toward independence after the election of 1959; he argues that the very mistaken belief that Ian Smith was a "man of straw" who would be toppled "within weeks" by a bit of economic pressure was based on the assessments of Sir Roy Welensky and other Rhodesian political veterans. For some reason Garner chose not to exercise the privilege that is often accorded to retired officials of using documents not yet available to outsiders, a decision for which professional historians should be grateful.

Though hideously expensive, this book is indispensable for research libraries if not for individual purchase. It was high time for a house history of the Dominions Office, and Garner has conceived and written it well.

JOHN W. CELL
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ASA BRIGGS. *The History of Broadcasting in the United Kingdom*. Volume 4, *Sound and Vision*. New York: Oxford University Press. 1979. Pp. xiv, 1,032. \$65.00.

Asa Briggs's latest volume in *The History of Broadcasting in the United Kingdom* deals with a crucial mo-

ment in the development of the mass media in Britain: the advent of commercial television. Parliament's decision to allow the BBC's monopoly to be broken by the passage of the Television Act of 1954, which set up a public authority with powers to allocate franchises and oversee program quality, came at a moment when, as he makes clear, the BBC was still coming to terms with television. In fact, the men who ran BBC television were engaged in a frustrating and often petty struggle with a central bureaucracy that thought of television as an adjunct to radio: "radio with pictures," as one BBC mandarin put it.

One consequence of this attitude was the resignation of the head of BBC television, who proceeded to become one of the moving spirits of the commercial television lobby. The story of that lobby and of the early days of commercial television inevitably takes up much of the present volume. Commercial television, as Briggs makes clear, was an issue that divided the two main political parties internally. Thus there was strong opposition to commercial television within the Conservative Party because it ran counter to the patrician tradition of public service; within the Labour Party there were advocates of commercialism who felt that the BBC was too firmly establishmentarian.

Briggs's account, in spite of interviews with prominent participants of the events he describes, indicates one of the problems that will face future volumes in the series. Although the BBC and the Independent Broadcasting Authority, which now controls both commercial radio and commercial television, have put their archives at his disposal, there is no such easy access to the papers of the commercial companies that have been involved in a venture that, after a shaky start, became in the words of Lord Thomson (later proprietor of the *Times*) "a licence to print money."

In spite of a noticeable reluctance to make a posteriori judgments, Briggs has provided a well-balanced account of a historical episode, which throws light not only on the nature of the EBC and British commercial television but also on the workings of the British political machine. It is in that sense a study of how the role and functions of the mass media are controlled within a parliamentary democracy.

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P. W. J. RILEY. *King William and the Scottish Politicians*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xi, 194. \$27.75.

This is a fascinating and in some ways provoking book. It elaborates on views P. W. J. Riley has ex-

pressed before (see his "The Structure of Scottish Politics and the Union of 1707" in T. I. Rae, ed., *The Union of 1707: Its Impact on Scotland* [1974]) and provides the first detailed account of the machinations of Scottish politicians during the reign of William II and III, "a study of political chicanery at the highest levels . . . not calculated to lighten anyone's spirit" (p. 10). It is also a study in failure at all levels; no one emerges with any credit save possibly the first marquess of Tweeddale, who, unlike his fellow politicians, "concerned himself seriously about the wellbeing of Scotland" (p. 50) and the earl of Seafeld, a far-sighted man who had something of the outlook of a permanent civil servant. The king cared nothing for Scotland; all he wanted was that it should cause him no trouble. He left its management to his favorite Portland and the returned exile William Carstares. According to Riley, they all misunderstood the nature of the Scottish problem. They imagined that issues were involved, specifically a religious issue between presbyterians and episcopalians, when in fact the only thing that mattered was office. William understood enough about the tactical requirements of Scottish politics to resist the destruction of the committee of the articles. What he failed to understand was that once the committee was gone, and the independence of the Scottish parliament assured, that body would become unmanageable because there would not be enough places and pensions available to create a working majority. Nor did William and his advisers properly assess the place of the magnates in Scottish political life. There were four such men, so powerful that no government could survive without the support of at least one of them: the dukes of Hamilton and Queensberry, the marquess of Athol, and the earl of Argyll. If it was impossible to govern without them, it was also impossible to govern with them, because none of them would share power with any of the others, much less with any less important man. After William's early attempts to govern without them failed, a ministerial reconstruction in the winter of 1695-96 resulted in a government in which all four interests were represented. The result was an alliance between Queensberry and Argyll that drove the other two out; this accomplished, Queensberry began to intrigue, successfully, against Argyll. At the end of the reign Queensberry stood alone, facing a parliament that his insatiable greed for dominance had rendered unmanageable. The only solution, as William came to realize (though probably, says Riley, for the wrong reasons); was union.

Riley tells his sordid story artfully; his research is thorough and his conclusions convincing—though for reasons that are not wrong but incomplete. He has become so immersed in his tale of political chicanery that he deals with events only as they affected the struggle for office, and he sees them too

much in that light. Of the massacre of Glencoe, for instance, he writes, "the outcry was more against Dalrymple than the crime" (p. 84). This outlook leads him unduly to discount the impact of public issues on the politicians themselves. William may have exaggerated the importance of the religious issue, but the nature of the religious settlement did matter to people, and it is worth pointing out that each of the shifting coalitions of officeholders operated in the presbyterian "interest." If Riley's approach had been a little less cynical than that of the Queensberrys and Dalrymples, this would have been a better book. But it is a very good one, an important contribution to our understanding of the antecedents of the Anglo-Scottish union.

MAURICE LEE, JR.
Rutgers University

MALCOLM GRAY. *The Fishing Industries of Scotland, 1790-1914: A Study in Regional Adaptation*. (Aberdeen University Studies Series, number 155.) New York: Oxford University Press, for the University of Aberdeen. 1978. Pp. 230. \$19.50.

The importance of fishing as an additional support for the Highland economy during the years of severe population pressure has long been recognized. Despite this no specialized study of fishing has ever been written nor has the role of fishing elsewhere in Scotland been investigated. Malcolm Gray's book thus fills a gap in the literature on Scottish economic history. The first five chapters describe and explain the remarkable transformation that lifted the Scottish fisheries from their eighteenth-century backwardness to a position of preeminence within Europe by the mid-nineteenth century. Public policy sought to develop the fisheries, but Gray allows this little weight in his explanation of the dramatic change in the industry's circumstances. Rather, it was "tiny private enterprises" (p. 6) that tapped the abundant, readily accessible stocks of herring, haddock, cod, and ling, in response to demand from three main markets: Ireland, the West Indies, and Continental Europe.

The book is described as being primarily an economic analysis, but in fact there is plenty to interest the social historian in Gray's analysis of the fishing communities. Three broad geographical regions, each with its own distinctive social organization, are identified: the East Coast (with Caithness as the pioneering area in the development of herring fishing), the West Coast (with the Firth of Clyde treated separately because of the preponderant influence of the Glasgow market), and the Northern Isles. In the East, fishing became a fully professional, year-round activity divorced from farming. On the West Coast, land hunger, poverty, and lack

of landlord support left fishing as a part-time activity providing some extra income and subsistence. In the Northern Isles, land provided a less than full subsistence, but, unlike on the West Coast, landlords were active in promoting and profiting from fishing. The continuing influence of these characteristics is traced in later chapters and is fundamental to Gray's view of fishing as a series of industries rather than a single industry. This may not accord with the economists' definition of an industry, but it undoubtedly enables Gray to make fascinating observations about the fishing communities. Thus, for example, the custom of equal shares, common on the East Coast in the 1790s, created a community that "was almost class-less within itself" (p. 24). Such equality was not present elsewhere nor did it survive the growing intensification of the industry as steam-trawling (from the 1880s) and the steam-powered drifter (after 1900) radically altered the conditions of participation in the industry. This theme and the part played in it by the fish salesmen, a small but wealthy and influential group who took over marketing from the curers in the 1880s, dominate the rest of the book. With their multiplicity of shore-based business interests, the salesmen provided market services that made Aberdeen the most rapidly growing city in Scotland between 1880 and 1911. Not all of the expansion in fishing capacity before 1914 proved financially rewarding, and with a heavy dependence on export markets (some 80 percent of the herring catch in 1913 went to Europe) the industry was to suffer severely during the interwar period.

RON WEIR
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CORNELIUS O'LEARY. *Irish Elections, 1918-77: Parties, Voters, and Proportional Representation*. New York: St. Martin's Press. 1979. Pp. 134. \$19.95.

Irish politics is proving increasingly fruitful for testing concepts of comparative politics. As theories about nation-building, elites, leadership, and political parties have been used in recent studies of Irish political culture, psephologist Cornelius O'Leary is just the person to make an appraisal of its system of proportional representation (PR). Well versed in the Anglo-American experience with single-membered constituencies where the winner takes all and with the theory and practice of PR on the Continent and in Ireland (having written *The Irish Republic and Its Experiment with Proportional Representation* in the Notre Dame series of national election studies), he should be able to test opinion and theory about Irish polling against experience and reality.

O'Leary shows that Ireland's system of proportional representation has performed contrary to Anglo-American opinion about elections. Ireland has a

system of multimembered constituencies where electors can exercise the single transferable vote, thanks to Lloyd George's efforts to preserve minority representation during the fight for independence. Irish elections have not worked against responsible, representative government despite the Republic's social and political differences. In fact, the originally diverse and fragmented group of parties was reduced to a system of two-and-a-half parties by the 1960s, a process that effected stable government. In achieving stability, parties, leaders, the media, and voters all played a role. Although O'Leary admits that there is a complex, interdependent relation between parties and electoral systems and that the Irish experience has undercut some of the dogmatism about majority systems, he does not believe that the Irish system *per se* has contributed much to government stability.

The trouble with O'Leary's explanation is that it does not give Irish PR its due, thanks apparently to his past dogmatism about majority systems. Despite his claim, the first half of the present volume is little more than a rehash of the earlier work, warts and all. For example, in the midst of describing how Irish elections helped achieve a stable party system, O'Leary asserts, "Apart from all value judgments PR has, in fact, acted as a disintegrating force and the majority system as an integrating one" (p. 50). PR's supporters, like J. A. Costello, are pictured as little better than fools and knaves although they appear to have been more right about the future and about what might happen if PR were eliminated than people like David Thornley. The roots of O'Leary's dogmatism seem to lie in the assumption that elections are meant only to create stable governments and in a bias toward Ulster and Unionism. Electoral systems, like parties and other institutions, have many functions, including mobilizing and educating citizens so that they can trust their leaders and their policies. O'Leary should have explained these other functions of Irish elections, including local ones, against a backdrop of the horror that the majority system has helped create in Northern Ireland. The Stormont regime had such overwhelming majorities that it lost the confidence of friend and foe alike.

The Irish tragedy is too complex and confounding for O'Leary's electoral preferences.

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BRIAN P. COPENHAVER. *Symphorien Champier and the Reception of the Occultist Tradition in Renaissance France*. New York: Mouton; distributed by Walter de Gruyter, New York. 1978. Pp. 368. DM 92.

The "enchantment" and "disenchantment" of the Western world is a topic that has increasingly con-

cerned both intellectual and social historians. D. P. Walker and Frances Yates have shown that, although occultism existed in antiquity, in early Christian times, and throughout the Middle Ages, it took on greater respectability and force during the Renaissance, largely because of the influence of Marsilio Ficino and Giovanni Pico della Mirandola.

Brian P. Copenhaver has contributed a worthy study to the growing corpus dealing with the rise and decline of occultism, focusing his considerable scholarly talents on Symphorien Champier, the Lyonnais humanist-physician of the early sixteenth century. Duke Antoine of Lorraine's *primarius medicus* was, according to Copenhaver, a scholar of "un-deviating unoriginality." Yet his very limitations make him a valuable object of study, for Champier reveals the mental space of similarly educated intellectuals of his day.

Eschewing the social scientific approaches to the study of Renaissance occultism found in the works of Keith Thomas and Erik Midelfort, Copenhaver investigates the "intellectual history of the occultist tradition through the examination of individual thinkers." This examination allows Copenhaver to answer the question posed in his "introductory essay": "to what degree and for what reasons did the occultist tradition possess 'cognitive authority' [i.e., intellectual respectability] in the late medieval and early modern period?"

It would at first seem strange that Copenhaver has studied Champier in order to answer this question. After all, Champier was well known as an enemy of occultism, especially of the astrologers and of the platonizing medieval Arabic physicians. (Champier's *Dyalogus . . . in magicarum artium destructionem*, appended to this study, is an example of Champier's attack on the occultist tradition.) An examination of Champier's writings and of his sources demonstrates, however, why he transmitted the occultist tradition at the same time that he was its critic and enemy. Champier feared and criticized magic in its various forms because of its tendency to negate human free will and God's providence. Yet this medical humanist's acceptance of the *prisca theologia*, the Greco-Roman and Islamic medical authorities, and the medical advice of Plato's *Charmides* (156d-157a), as interpreted by Ficino, necessitated his reception of their emanationist and demonological world-view, which he should otherwise have attacked were he to have mounted a consistent critique of occultism. Consequently, while he mocked contemporary theories of demons, Champier was unable to deny their existence in an absolute way.

This study does indeed show why the occultist tradition enjoyed respectability and why it could not easily be dispatched: its premises were too much

a part of the sixteenth-century intellectual's *Weltanschauung*. While Champier and others might decry the *magi* and their demons, they could not imagine a universe devoid of Satan's agents. They could only trust, as Champier did, that Christian piety and faith would harness the demons and overcome occultism's popularity, which the pre-Reformation institutional crisis of the Church had helped cause.

In sum, Copenhaver's sensitive study of Champier shows the inexorability of the sixteenth-century reception of occultism. He also shows that the motivation for trying to "disenchant" the world was, at first, religious. The new science eventually overthrew the respectability of the occultist tradition but only after having expurgated the occultism inherent in its own sources.

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EMMANUEL LE ROY LADURIE. *Le Carnaval de Romans: De la Chandeleur au Mercredi des cendres, 1579-1580*. (Bibliothèque des Histoires.) Paris: Gallimard. 1979. Pp. 426.

Again Emmanuel Le Roy Ladurie has focused attention on a single community as a means of coming to a better understanding of the structures and dynamics of a preindustrial society. The subject is Romans, a small city in Dauphiné that during the late 1570s became the foremost center of a widespread insurrection. There an artisan-led coalition of lower orders in 1579 forcibly broadened the base of municipal government at the expense of an oligarchy that had dominated the political life of the city for decades. Noteworthy as this Porcnev-type revolt is, the manner in which the rebels were put down is even more remarkable. The pre-Lenten festivities of 1580 provided a scenario for dramatic confrontations that culminated on the eve of Mardi Gras in a bloody counterrevolution that in turn touched off a general repression of the popular movement throughout the region.

Le Roy Ladurie's demographic, social, economic, political, and cultural analysis of Romans and the surrounding countryside is highlighted against a background of long-developing provincial disputes, the most significant of which were directly related to military and fiscal operations of the Italian and the civil wars. He recounts the main issues of a century-long *procès des tailles* that dated from the reign of Francis I. This set of controversies, concerned primarily with the assessment of direct taxes, produced a common front of *tailles*-paying *roturiers* who were determined to restrict the *taille* exemptions of the privileged orders of provincial society. The Third Estate of Dauphiné was exceptional in developing a

sophisticated theoretical framework for grievances, briefly examined here with reference to some of the *cahiers des doléances* and pamphlets of the period. Also as part of the background Le Roy Ladurie traces the formation of popular "leagues" in the area from 1578 on. Two currents of this originally rural protest movement are identified according to religious orientation. Both were directed against troop depredations, war taxation, and communal indebtedness, both became involved with uprisings in cities of the province, and both led to successful campaigns by combined rural and urban forces against brigand strongholds in the region.

In central chapters on the Romanais revolt and counterrevolt, Le Roy Ladurie presents a fine critique of the principal sources, the most informative of which is an account attributed to the royal judge of the city, a fascinating character who masterfully manipulated pre-Lenten activities to bring about the defeat of his enemies. The political use at carnival time of demonstrations and celebrations by occupational organizations, confraternities, "Abbeys of Misrule," youth groups, and neighborhood associations are described in unusual detail. Most interesting of the carnival events were the *reynages*—contests involving animals which became the symbols under which "kingdoms" were set up that parodied the established order and served to structure both festivities and intrigue. The bold, often brilliant, interdisciplinary interpretation of the symbols, codewords, dances, customary rituals, and spontaneous happenings associated with these "kingdoms" is the most original feature of the work.

Specialists in the history of Dauphiné can quibble with details and argue that a more thorough and systematic treatment of some topics is needed. Even so, Le Roy Ladurie has written one of the best of the monographs on the province, and his study is a valuable addition to the growing body of literature on the uprisings of early modern Europe.

LLEWAIN SCOTT VAN DOREN
Boston College

PETER N. STEARNS. *Paths to Authority: The Middle Class and the Industrial Labor Force in France, 1820-48*. Urbana: University of Illinois Press. 1978. Pp. 222. \$12.95.

Peter N. Stearns's new work is a study of the social attitudes and paternalistic actions of the French middle class during what Stearns considers its formative period. He wants to "get at the entrepreneurial mentality" through a study of the relationship between the industrial middle class and workers, because "employers, even those who hoped to view workers only as a factor in production, could not help thinking about them" (p. 107). Stearns there-

fore divides his manufacturers into three "types," corresponding to the degree of their "modernization" as entrepreneurs: "(I) those who refused to change in method or goal, (II) those who changed but imitatively and reluctantly, and (III) those who innovated with a certain degree of zest" (p. 16). The book is primarily about "type III" manufacturers in three industries, who, Stearns contends, improved conditions for their workers out of a combination of entrepreneurial sensibility and concern and thereby laid the foundations of the modern middle-class welfare state.

This is a very mixed book, a somewhat strange combination of solid research and unsubstantiated generalization, of some good ideas and insights joined with ragged conceptualization. Stearns has dipped into an important subject. We need to know more about the bourgeoisie of that period and in general. Like the early utopian socialists, Stearns lumps the industrial middle class with the "producers." Unlike later socialists and many historians, he is their booster, applauding their energy and their efforts to rationalize production. He sympathizes with their difficulties, telling us that "some industrialists had hard lives, even by proletarian standards" (p. 5) and that a few even suffered malnutrition (presumably these were of "type I"). Stearns helps revise the old saw about the non-innovative French entrepreneur, presents an able summary of the development of paternalism and the attitudes of middle-class industrialists toward workers (the best part of the book), offers some provocative comparisons with English cases (however vague), and capitalizes on some interesting sources, such as biographies of industrialists and records of the *conseils des prud'hommes*.

Yet Stearns's paths more often lead us to the present than they take us into the world of class relations during the *monarchie censitaire*. His study bogs down in a framework of crude "modernization." Stearns never really tells us how many "factory" workers there were nor offers a suitable definition of what that entailed in the early nineteenth century. The "factory setting" was not unprecedented and had, with the whole question of industrial discipline, important eighteenth-century antecedents. In any case, as the author implies, factory workers constituted a small minority of workers and in many cases worked alongside traditional artisans. Nor were factory bosses any more representative of the middle class than factory workers were of the labor force. The arbitrary typology is not helpful: there is something unreal about a family "on the border between type I and type II, as entrepreneurs," with a son with "type III ideas" (p. 18). Southern industrialists who opposed the 1841 child labor law (the subject of an interesting discussion) become "non-adapters." Modernization here be-

comes a dynamic that assumes a rigid and rather ahistorical inevitability in its own right.

Furthermore, it is unlikely that the views of manufacturers ultimately "shaped the nature of workers' unrest itself" (p. 178) more than changes in the structure of work and resultant shifts in worker attitudes. Stearns tells us nothing of the impact of the Revolution of 1830 on employers and workers. Even his contention (elaborated in an appendix) that conditions were improving for factory workers is suspect because he overestimates the degree of work stability and depends on wage data, an unreliable index of prosperity if unaccompanied by information on voluntary and involuntary *chômage*.

As with the documentation, the writing is uneven. Parts of the book seem hurriedly written, if not directly dictated to the printer. The bibliography of secondary literature is out of date. *Paths to Authority* will satisfy readers desiring an introduction to the evolution of labor relations during this important period of French history, and it may please those students of contemporary society who care less about understanding the past than uncovering fleeting glimpses of our own time. One can only hope that Stearns will take the time to carry through a full study of class relations and that this study will inspire other research.

JOHN M. MERRIMAN
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MAURICE CRUBELLIER. *L'enfance et la jeunesse dans la société française, 1800-1950*. (Collection U.) Paris: Armand Colin. 1979. Pp. 389.

This work is an ethnography by a cultural historian who uses materials drawn from anthropology, sociology, psychoanalysis, memoir, biography, and popular culture, as well as pedagogy and public record, to demonstrate the impact of the development of a national educational system on childhood and youth in France. In Maurice Crubellier's romantic view of childhood in traditional society, younger generations learned manners, morality, and the means to a livelihood from family, parish, employers, and peers through a slow, spontaneous, organic process. Crubellier asserts, however, that children managed to escape enough from surveillance to form a sort of autonomous youth culture, marked especially by secrecy from and opposition to the adult world. During a century and a half of industrialization and urbanization, solicitous adults discovered childhood, invented "puériculture," and created a school system within which to "recycle childhood . . . [and] integrate it into the cultural circuits of urban-industrial society." The traditional youth culture disappeared, to resurface in part, however, in urban adolescent gangs and the con-

temporary "culture sauvage" of motorcycles, movies, pop music, and blue jeans.

In an engagingly written, richly documented text, Crubellier examines many aspects of the French educational system, including nursery, primary, secondary, and vocational schools, clerical and lay instruction, legal requirements and classroom practices, bourgeois and working-class responses, boys' and girls' schools, sports and games, provinces and Paris. The author believes with Ivan Illich, whom he frequently cites, that all elements of the "schooled society" most assiduously taught a hidden curriculum, the "morale de Franklin," which turns play into work, children into industrious adults, and imposes scientific order on folkways. Purporting to offer the masses access to a higher culture and opportunity for upward mobility, educators of republican as well as religious persuasion in fact presented a profoundly conservative message. Emphasis on a classical course of study and a standardized language of discourse served to maintain the cultural hegemony of the elite. Through the "systematic enclosure of youth" in this "hard world" where new norms in language, manners, and even personal hygiene were taught by rote and sanction, traditional popular culture was diminished and no adequate substitute developed. Educational leaders created only a "museum of values." Deprived of meaningful work and social involvement, youth rebelled and contributed to the state of crisis in contemporary urban society.

Crubellier's work belongs to the literature that calls for reconstruction of educational processes to use rather than repress the "Luddite tendency" of children. It also implies that resolution of the many contradictions between industrial society and humanistic values may be possible, especially with increasing leisure opportunities. Partisans of Illich may use Crubellier to document their call for disestablishment of the schools. Marxists may cite him as evidence of the class struggle in education. Feminists will appreciate his interest in the education of girls and in changing sex roles. All students of the history of childhood, history of education, and French culture will find his new book interesting and useful.

MARILYN J. BOXER
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RAY NICHOLS. *Treason, Tradition, and the Intellectual: Julien Benda and Political Discourse*. Lawrence: Regents Press of Kansas. 1978. Pp. x, 270. \$16.00.

Although well known for his provocative *Treason of the Intellectuals*, the *homme de lettres* Julien Benda has never excited much interest among American students of modern French thought. Benda was per-

haps too narrowly concerned with French issues, and the tradition of the *clerc*, which he defended so vociferously, has no counterpart in American experience. Robert J. Niess (*Julien Benda* [1956]) and now Ray Nichols have tried to convince us of the continuing interest of Benda as a thinker and as an exemplary life. They see Benda as having written the twentieth century's most acute reflection on the permanent tension between the demands of universal reason—man's highest quality—and the irrationality that dominates life in the real world. In this analysis of Benda's thought as expressed in his many published works (his papers, like those of Léon Blum and other Jewish intellectuals, were seized by the Germans and lost), Nichols has relied on Benda's own account of his life. The scholar's analysis of Benda's works largely confirms the writer's self-image, a testimony to Benda's intellectual honesty. To be sure, Nichols is more aware than his subject of certain inconsistencies, of unresolved questions, of polemical exaggerations, and he has given us a nuanced picture of a complex mind.

Nichols is especially strong in explicating *Treason of the Intellectuals* and showing its relation to Benda's other work. After Niess and Nichols there can be no more excuse for simplistic misinterpretations of this celebrated—but more often cited than read—work. Though he defended the ivory tower, Benda was by temperament more polemicist than philosopher, and Nichols is right to conclude that he can best be understood by recognizing that "he was, after all, and above all, a critic" (p. 181), as were so many French Jewish intellectuals.

Nichols's chronological approach allows us to see both the consistency and the development of Benda's thought more clearly than Niess's topical organization; but the older work is not superseded, and some readers will find Niess's humanistic learning more appropriate to the subject than Nichols's social science. Despite their different approaches, they come to remarkably similar conclusions about this irascible gadfly of twentieth-century culture, this "idéologue passionné."

Although the virtues of this well-researched study clearly outweigh its defects, it is unfortunate that Nichols (or his publisher?) felt compelled to begin with an examination of the problem of the "intellectual," an examination that, fortunately, does not prove necessary for the understanding of Benda, who is effectively put into context in the remaining chapters. There are occasional turgid passages throughout, but Nichols's analysis of Benda sheds more light on the question of the intellectual than his introduction leads one to expect. Better editing would also have eliminated some inconsistencies of style in the notes, would have made the author explain the important difference between the com-

mon usage of "pathetic" in English and "pathétique" in French, might have eliminated some minor inaccuracies concerning Péguy's Librairie Bellais, and could have made a fine work out of what is certainly a more than adequate one.

WILLIAM LOGUE
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JEAN-NOËL JEANNENEY and JACQUES JULLIARD. "*Le Monde*" de Beuve-Méry ou le métier d'Alceste. Paris: Editions du Seuil. 1979. Pp. 376.

Le Monde was born of the need in 1944 to fill the "quasi-institutional" gap left by the defunct and compromised *Temps* with a serious, impartial, reference newspaper standing above the plethora of special-interest organs spawned by the liberation. Its founding editors, all approved by de Gaulle himself, included the MRP *pur et dur* Hubert Beuve-Méry, paradoxically a prewar *Temps* correspondent in Prague where he had broken with the newspaper over Munich. In the meantime he had passed through the crucible of the École des Cadres d'Uriage and its redoubtable Pierre Dunoyer de Segonzac, whose style of a reformed monk-soldier was the touchstone of national renewal for many of his followers. Beuve and his companions were given the material resources (including the offices and printing plant of the *Temps*) to guarantee their "moral" independence.

As *Le Monde* grew in readership, it became the mirror of France's most important issues. Rapidly dominating his colleagues on moral and intellectual grounds, Beuve resolutely set the newspaper against the war in Indochina and the alignment of France in the Atlantic bloc. No sooner had the "neutralist" issue of 1950 been calmed, than the "crisis of 1951" broke. René Courtin, one of the original founders, mounted the most dangerous assault on Beuve's authority during the entire history of *Le Monde*, and only the joint persuasion of Maurice Duverger, Jean Monnet, and de Gaulle himself caused Beuve to abandon resignation and vanquish his opponent. In comparison, the attempt in 1956 of an old guard rival with a new organ, *Le Temps de Paris*, to dislodge *Le Monde* was a farce. When de Gaulle came to power in 1958, Beuve forced his approval of the general as "le moindre mal" on his reluctant subordinates, despite his acute awareness of the moral illegality of the coup and his later opposition to Gaullist plebiscitary politics and Maurrassian nationalism. On the twenty-fifth anniversary of his editorship in 1969, Beuve retired, leaving Jacques Fauvet in charge of the newspaper, which was shortly to become the third largest in France and the only one to be entirely controlled by its personnel. *Le Monde* had become what Beuve intended it

to be: nationalist, parliamentary, and neo-Jansenist.

The authors have used a wide variety of sources, ranging from the archives of and interviews with their protagonists to secondary literature critical of Beuve and his newspaper. They have far more than merely journalistic credentials: Jean-Noël Jeanneney, author of a much remarked study on the de Wendels, is professor of contemporary history at the Institut d'Études Politiques and Jacques Julliard is a professor at Paris-Vincennes. It is thus all the more unfortunate that they should have uncritically adopted the fashionable and tiresome French intellectual shibboleth of an always unspecified American "imperialism" to which France is continually being "inféodé." This myopia does not just prevent them from treating the moral side of a scandal like the publication in 1952 of the sensitive Fechteler report. It also causes them to be less than completely frank on Beuve's desertion of Pierre Mendès-France in 1954, his disregard for the plight of the *pièdes noirs* in Algeria in 1958-62, and his guilt-laden handling of the student revolt in 1968.

Can *Le Monde* as an institution become decrepit? Not as long, the authors seem to conclude, as the organ remains faithful to the "conception of information giving" laid down in its first issues. One can only point out that vigilance is necessary, for this certain conception of journalism, like a "certain idea" of France, is not perennially hardy but rather terribly vulnerable to external and internal corruption. Two annexes are included, the more interesting of which portrays the readership of *Le Monde* by sex, age, and socioprofessional category.

PHILIP C. F. BANKWITZ
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JANINE FAYARD. *Les membres du conseil de Castille à l'époque moderne (1621-1746)*. (Mémoires et Documents publiés par la Société de l'École des Chartes, number 26.) Geneva: Librairie Droz. 1979. Pp. xxx, 611.

This massive study, based almost exclusively upon exhaustive archival research, is devoted to the social group formed by members of the *Consejo de Castilla* in the period between the *siglo de oro* and the Enlightenment. With the implicit assumption that "le conseil de Castille était le premier corps de l'État" (p. 125), Janine Fayard ignores the judicial, administrative, and political functions of the *Consejo* and examines the family origin, career, wealth, and social rank of its members. The thesis of this examination is that there did exist in Spain—Antonio Domínguez Ortiz to the contrary—a *noblesse de robe*.

From one perspective, Fayard proves her thesis convincingly. Drawn from the *noblesse moyenne* of

northern Castille, the members of the *Consejo* had, by the end of the seventeenth century, been transformed into an endogamous group that had high social status and that perpetuated itself on the basis of family, university, and regional solidarity. In describing and analyzing this group, Fayard provides a fascinating picture of society based upon considerations of "la honra," whose members' lifestyles were dominated by "le besoin de paraître." Although the discussion of the importance of *colegios mayores* only marginally adds to the work of Richard Kagan, that of "le cursus administratif" is excellent. To Fayard—and to the reader—the members of the *Consejo* emerge vividly "comme d'honnêtes praticiens du droit" (p. 538).

But is an endogamous group of jurists with high social status a *noblesse de robe*? By formulating the problem of the existence of a *noblesse de robe* in Spain, Fayard explicitly calls for comparison with the corresponding development in France in the early eighteenth century. Thus, to answer the question affirmatively, one would have first to examine the judicial, administrative, and political power of the *Consejo*, those areas Fayard deliberately ignores. Whatever powers the *Consejo* had in the sixteenth century and was to have again in the early nineteenth century, and whatever power *individual* members of the *Consejo* may have had, it seems clear—as Fayard herself indicates—that the growth of *Juntas* and *validos* in the seventeenth century and that of *secretarios del estado* in the eighteenth century relegated the *Consejo* to a position where its political powers were not equivalent to its social status. And it is only with regard to this last criterion that Fayard can accurately describe the *Consejo* as "le plus beau fleuron" in the administrative system of Spain.

MICHAEL D. GORDON
Denison University

JOSÉ ANDRÉS-GALLEGOS. *El socialismo durante la dictadura, 1923-1930*. (Colección Historia Política.) Madrid: Tebas. 1977. Pp. 636.

According to the author, who is professor of contemporary history in the extension division of the Spanish university system, this book began as a series of university lectures; consequently, it represents an attempt to synthesize the existing literature rather than to present the results of original research. Such efforts can be useful when the synthesis is the product of thoughtful and prolonged analysis of both primary and secondary materials. Unfortunately, this volume provides little in the way of real synthesis. Instead, it is basically a compendium of facts, documents, and contrasting points of view, most of which have not been sub-

jected to critical evaluation. Nor is there a coherent interpretive framework. As a result, the most valuable part of the book is the four-hundred-page appendix, which conveniently assembles relatively inaccessible official documents and newspaper articles from the period.

José Andrés-Gallego's failure to draw conclusions from his material is not inadvertent but rather grows out of his conviction that the history of the recent past is badly in need of a "dose of positivism" to stimulate "enthusiasm for verifiable facts" (pp. 250-51). It is indeed true that Spanish historiography has often suffered under the twin burdens of political partisanship and a cavalier disregard for hard data. But a significant number of works recently published in Spain overcompensate in favor of a naive positivism in which "the facts" and the documents are left to speak for themselves. The enthusiasm for archival work—so long missing in Spain—is welcome. But the painstaking analysis and synthesis that distinguish history from antiquarianism seem currently to be receiving short shrift.

Despite the author's reluctance to interpret his material systematically, the book nevertheless betrays a certain sympathy for the "collaborationist" orientation of Spanish socialism during the 1920s and an even clearer sympathy for the attempts of the dictator, General Miguel Primo de Rivera, to domesticate the Spanish labor movement. The text deals most thoroughly with the controversy within the Socialist Party and its trade union affiliate, the UGT, over the union's decision in 1924 to accept official positions within the regime as the representative of the Spanish working class, although some of the interesting—and controversial—points raised are too weakly substantiated to be accepted without further research. One example is the author's assertion that the UGT rank and file remained close to the "syndicalist" position maintained by Francisco Largo Caballero, even after he and the rest of the Socialist leadership abandoned it in favor of an "opportunistic" policy of opposition to the dictatorship in August 1929. Others have argued that it was precisely a shift in the mood of the UGT masses that forced the leadership to repudiate its former policy of collaboration.

This controversy highlights the need for more research on the history of the Spanish working classes, as opposed to the history of labor organizations and their leaders. As this volume demonstrates, our knowledge of those organizations remains sketchy. Nevertheless, their impact and significance will only be completely understood when political history begins to be supplemented by the new social history.

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SAMUEL J. MILLER. *Portugal and Rome c. 1748-1830: An Aspect of the Catholic Enlightenment*. (Miscellanea Historiae Pontificiae, number 44.) Rome: Università Gregoriana Editrice. 1978. Pp. xii, 412. L. 20,000.

This thoroughly researched and ably prepared study has two major themes. The first involves a careful tracing of the frequently tense diplomatic relations between Portugal and the papacy from 1748 to 1830. The second is an analysis of the religious and intellectual aspects of the contest between the "regalist" state, determined to enlarge its role in society, a still powerful papacy, seeking to maintain and increase Roman control, and a third group of moderates (whose position was as varied as it was uncomfortable), who sought to create their own vision of the Church.

Samuel J. Miller moves back and forth between these two themes, seeking to show the truly complex relationships between them. When José I came to the throne of Portugal in 1750, the issues of control of the legal business of the papacy, the management of missions in Portuguese territory, and the complex matter of appointing church officers, had been simmering for years. Moreover, the crown was much aggravated by the intransigence and equivocation of the Jesuits toward the Anglo-Portuguese boundary settlement of 1750, in the opinion of many merely the latest and most obvious example of the suspicious independence of the Jesuits.

To these ingredients must be added the nearly simultaneous ascent to power of the future *marquês* do Pombal and Clement XIII, both powerful and determined men. The result was a clash whose immediate focus was the Jesuit order and which ended in their expulsion in 1759. Within a year, Lisbon broke off official relations with Rome. This situation prevailed until the accession of Clement XIV, in 1769, when there was an abrupt reversal in papal policy, amounting to a substantial victory for Lisbon.

Diplomatic and legal relations were resumed in 1770, but on terms that tacitly recognized the king's rights of *Placet* and appointment. The result was a Portuguese church of decidedly nationalist flavor. Even the death of José I and the fall of Pombal had no immediate effect, and the dominance of regalism was overcome only after the shock of the French Revolution and Napoleonic invasion had frightened and weakened the monarchy and reversed the flow of sentiment.

On the intellectual side, Miller clearly delineates the existence of a moderate Catholic Enlightenment in Portugal—a movement that then and later was carelessly labeled Jansenist but that, in reality, was far more complex. One of the major contributions of Miller's work is to show how inadequate and deceptive it is to label the movement Jansenist. In the

Portuguese context, the Catholic Enlightenment was perhaps principally composed of regalism, episcopal jurisdictionalism, antischolasticism, anti-Molinism, and anti-Jesuitism. The heterogeneous and negative character of the movement is at once apparent, and it was this characteristic that opened it to exploitation by the senior partner, the regalisists. Nonetheless, almost no Portuguese reformer, not even Pombal, wished for any total or permanent break with the papacy.

It was just such moderates, backed by Pombal's political power, who carried through the reform of the University of Coimbra, who manned the redirected Board of Censors, and who, in time, were named to high ecclesiastical positions, giving stability and direction to the Portuguese church.

It was the tragedy of all of these parties, however, that in their struggle with each other they lost sight of the real enemy, the antireligious wing of the Enlightenment, that burst on the scene in 1789.

Miller is to be praised for his exhaustive use of a large number and variety of archival sources, for the careful and usually clear delineation of a complex problem (which he knows how to situate within a broad historical context), and, above all, for an ample, judicious, and fair vision of a problem that has been badly obscured by polemics and special pleading. I think this will be the definitive work on the topic.

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PHYLLIS MACK CREW. *Calvinist Preaching and Iconoclasm in the Netherlands, 1544-1569*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1978. Pp. 221. \$19.95.

The author of this work has recognized that historians need to know more about the role of the Calvinist ministers in the Netherlands in the years immediately preceding the outbreak of the Dutch Revolution. Did Calvinist preachers or Calvinist ideology inspire the Dutch Revolt and the Troubles (1566) that preceded it, or were other, perhaps more important, factors involved?

In this book, which is a study of the relationship between ideology and social behavior, Phyllis Mack Crew analyzes the attitudes and character of Calvinist ministers in the Netherlands in the mid-1500s as well as their effect on the popular religious riots and outbreaks of iconoclasm that occurred during the summer of 1566. Crew strives to relate the Troubles to the larger context of the international Calvinist movement and to iconoclastic violence in other countries. In so doing, she questions whether the Netherlands preachers who claimed to be part

of the Reformed community in this period were really as clearly "Calvinist" as historians in the past have supposed, concluding that "the ministers who preached during the Troubles had virtually nothing in common" (p. 100).

Crew maintains that the violence of 1566 was the result of a desire to revitalize communal and personal authority in the face of a collapse in the central political jurisdiction. When that political authority was restored in 1567, the popular movement in turn broke down. Contrary to some older views, she asserts that the Troubles in the Netherlands was neither a symbolic expression of socioeconomic conflict nor the result of a mass conversion to Calvinist doctrines.

Crew has employed the best sources for the proper study of mid-sixteenth-century Netherlands history. Yet, despite her spirited discussion of this oft-neglected corner of Dutch history, several weaknesses make her treatise less than conclusive. First, it is not always clear how she is using certain key words and phrases. For example, what exactly was a "Calvinist" in 1566? This critical definition is absent from her discussion at several points (for example, pp. 40, 47, 57, 65-68, 100, 134, 175). Calvinist ministers, moreover, may have become "a band of revolutionary fanatics" after 1566, but by whose definition (p. 181)? Nor is it always clear against which kind of Calvinism the hedge-preachers of 1566 are being measured—the moderate brand espoused by Calvin himself or the more severe variety of Beza (both are mentioned on page 100). Also lacking clear usage are the important terms "Reformer," "fanatic," "Anabaptist," and "sect."

Second, Crew lacks a firm grasp of the dynamics and basic beliefs of the Anabaptist movement. This is most apparent when she claims that "the theological doctrines of the Mennonites would not have impressed outsiders as radically different from those of other Protestant groups" (p. 148). On the contrary, there were fundamental differences in theology between Anabaptists and other reformers on such issues as believer's baptism, regenerate church membership, and religious liberty. What is more, Anabaptists did not believe in "the idea of spiritual regeneration by adult baptism," as Crew alleges on the same page.

Third, she tends to exaggerate those historical viewpoints she wishes to refute, especially when she maintains, "Both contemporaries and historians have perceived this relationship of ideas and behavior as an either/or situation: either Calvinist ideology inspired the Troubles and the Dutch Revolt, or it did not" (p. 176). Furthermore, she does not identify the historians to whom she alludes.

I am in substantial agreement with Crew's overarching conclusion that, instead of being a rigorously doctrinaire movement, Calvinism contained

more diversity of expression than historians hitherto have generally noticed. Even so, her work, like most doctoral dissertations, is more suggestive than definitive. I, for one, will look forward to her continuing contributions to our knowledge of sixteenth-century Netherlands Calvinism as she hones the interpretations she has put forward here.

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RICHARD W. UNGER. *Dutch Shipbuilding before 1800: Ships and Guilds*. (Aspects of Economic History: The Low Countries, number 2.) Assen: Van Gorcum. 1978. Pp. xi, 216. f 32.50.

This slim volume of 118 pages of text traces the development and organization of the shipbuilding industry and the rules laid down by the Dutch shipbuilders' guilds from the Middle Ages to 1800. Richard W. Unger employs the word "Dutch" to refer primarily to the activity of shipbuilding in Holland and Zeeland rather than to that in the Low Countries as a whole. The use of wind power and design made Dutch ships more effective than those of other countries and laid the foundations for the wealth of Holland and Zeeland in the seventeenth century.

The author traces the rapid growth and development in ship design in the sixteenth and early seventeenth centuries and then the stagnation of the industry in the eighteenth century. Major improvements in shipbuilding practically ceased after 1630. One might wish that the author could state more specifically why this phenomenon took place around this date. The rigid rules of the shipbuilding guilds undoubtedly contributed, as Unger sets forth, but this explanation still leaves one with the feeling that something else must have been involved.

Little is included of any contributions made to the industry by the admiralties of Holland and Zeeland. In the design of ships intended for naval warfare some advances must have been made. The question can be raised whether the Dutch East India Company (VOC), in its construction of ships for the Oriental trade, did not make significant improvements in ship design. It would have been interesting to learn more about the materials used in ship construction, the type of wood, where it came from, and how it was seasoned and prepared.

The chapters dealing with guild organization and regulations are particularly good. In the early period, ship carpenters' guilds were remarkably free from restrictive rules and did not oppressively limit personal freedom. By the eighteenth century, as the author shows, the political activity of the town councils came more and more to regulate the ship

carpenters' guilds. The guilds of the fifteenth and sixteenth centuries on balance fostered growth; the guilds of the late seventeenth and eighteenth centuries hedged innovation in order to preserve jobs. It was the town councils and not the central government that encouraged this type of regulation.

The appendixes include regulations, letters of organization, and bylaws of ship carpenters' guilds from the fifteenth century to 1800. An extensive bibliography of the shipbuilding industry and very helpful footnotes are presented. More illustrations or drawings of various types of ships, sails, and rigging would have been helpful.

This book is highly recommended to anyone interested in studying Dutch maritime history between the Middle Ages and 1800.

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GERALD STRAUSS. *Luther's House of Learning: Indoctrination of the Young in the German Reformation*. Baltimore: Johns Hopkins University Press. 1978. Pp. xii, 390. \$20.00.

There was a greater concern with education in the Renaissance and Reformation period than at any time in history other than our own. In this volume a leading Reformation scholar directs his attention to the question of the Lutheran reformers' efforts to transmit their values to the younger generations during the course of the sixteenth century. Gerald Strauss examines their pedagogical assumptions and expectations, their doctrines and techniques of indoctrination, and assesses their successes and basic failure. The basic assumptions of the reformers were that one must start with the young, that indoctrination is necessary for religious and moral improvement as opposed to academic education, that this indoctrination must be done in public schools and not entrusted to parents apt to be negligent, that teaching will be more effective if based upon a humane fellowship between child and adult, and that religious knowledge and understanding are attainable through the efficient functioning of a well-ordered school system.

The study is based upon the reading of a staggering number of educational treatises, histories of the schools and sermon books, and a very impressive, detailed examination of the reports made by the visitors commissioned by the consistories of the territorial principalities to determine the spiritual and educational conditions in the land. The visitation reports provide less information on the larger cities than on the towns and villages of the countryside, but they contain a vast amount of material of keen interest to the social and religious historian. The author concludes that, in the light of the lofty aims of

the reformers, the evidence of actual accomplishment in improving the common people reveals that their pedagogical venture was a failure. Such success as the reformers enjoyed was possible only because of the support of the state. But that success was so limited when measured against the reformers' purpose of leading all people to think, feel, and act as Christians—to imbue them with a Christian mind-set, motivational drive, and way of life—that their effort must be judged a failure. The antiheroes of this scenario are the Lutheran churchmen, preachers, theologians, and superintendents who lost contact with the common people and their folkish religiosity and promoted abstract theological formulas instead. The Reformation lost its spontaneity after 1530 and became progressively ineffectual as the century wore on. The burden of proof now rests, the author asserts, upon those who claim that the Reformation aroused a widespread, independent, meaningful, and lasting response to its message.

This revisionist thesis makes this book a very important contribution to our picture of the Reformation era. It is rich in interpretive insight and quite unconventional analyses. What can be said in response by a critic who believes that what is said is very true but that the account does not tell the whole story or properly evaluate the positive achievements in religious education? The visitation reports are almost by virtue of their purpose apt to emphasize abuses and failures, "viewing with alarm." A vast body of other literature—devotional booklets for families, aids for catechetical instruction in the household (not just by the state), hymnbooks, prayerbooks, well thumbed in extant copies—suggests a vital religious life among the common people. One hundred thousand copies of Luther's catechism, with its table of duties, were published by 1562 and learned by acolytes and midwives. Parish preachers came from the lower classes and were, in fact, close to the people. The authors of the electoral Saxon constitution urged pastors not to "shame the boys and girls but to treat them in a friendly brotherly manner so that they may look forward to the (confirmation) examination with heartfelt desire and joy rather than fear." In a day such as ours of waning commitment to public education, this superb book conveys a somber message.

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HANS-CHRISTOPH RUBLACK. *Gescheiterte Reformation: Frühreformatorische und protestantische Bewegungen in süd- und westdeutschen geistlichen Residenzen*. (Spätmittelalter und Frühe Neuzeit: Tübinger Beiträge zur Ge-

schichtsforschung, number 4.) Stuttgart: Klett-Cotta. 1978. Pp. 290. DM 88.

PAUL MÜNCH. *Zucht und Ordnung: Reformierte Kirchenverfassungen im 16. und 17. Jahrhundert (Nassau-Dillenburg, Kurpfalz, Hessen-Kassel)*. (Spätmittelalter und Frühe Neuzeit: Tübinger Beiträge zur Geschichtsforschung, number 3.) Stuttgart: Klett-Cotta. 1978. Pp. 232. DM 66.

These studies of sixteenth-century Germany, one conducted on an urban and the other on a territorial base, observe the German Reformation from two unaccustomed angles.

In *Gescheiterte Reformation* Hans-Christoph Rublack investigates the failure of Protestantism to establish itself in eight south and west German cities that served as episcopal sees: Würzburg, Bamberg, Trier, Mainz, Salzburg, Passau, Freising, and Eichstätt. Which "presuppositions, conditions, and supporting circumstances" (p. 4) that were present in those cities where the Reformation succeeded were absent in these bishops' residences? Rublack asks this question both in reference to the early reform movement of the 1520s and in reference to the Protestant minorities that existed in many of these same cities during the second half of the sixteenth century.

The answers are not unexpected. Early reform efforts stagnated at the level of a preachers' movement and were never "politicized." City councils were too weak to provide effective resistance to the bishops who exercised both ecclesiastical and secular power. No permanent links could be forged between longings for urban autonomy and the Reformation. Protestants could thrive only as long as the prince-bishops allowed, and, in the first stage, they tolerated the nascent reform only until the Peasants' War of 1525 supplied a good occasion to suppress it. The tantalizing question, *why* the preachers in these cities could not mobilize the citizenry and the councils against the bishops, is not answered. Rublack hints that the answer might lie partly in the timidity of the preachers and partly in the submissiveness of the citizenry. One might also ask what social and economic conditions were lacking that sparked the urban revolutions accompanying the successful breakthrough of the Reformation in many north German cities.

Zucht und Ordnung investigates the rise, structure, and sociopolitical function of the Reformed church orders in Nassau-Dillenburg, the Electoral Palatinate, and Hesse-Kassel. These German Reformed churches did not adopt a completely Calvinist structure like their kindred churches to the west but retained constitutional remnants from their earlier Lutheran phase. The historical development of the Reformed orders and a comparative study of their elements lead to the same conclusions. First, the conversion of these territories from Lutheranism to

Calvinism did not curb the tendency of the secular ruler to exercise control over church affairs. Second, questions of polity did not play a decisive role in the changeover to Calvinism; rather, dogmatic concerns more vital to the theologians and liturgical practices more visible to the people dominated.

In his analysis of the socio-political function of the orders, Paul Münch elevates the concepts of *decorum* (which he translates as *Zucht*?) and *ordo* to components of a theory of social control and concludes that Calvinism promoted more discipline than democracy. This theory deserves to be tested on a broader textual basis than the preface to the Church Order of Hesse (1566). If true, it would reinforce what Münch himself suspects (p. 191): Calvinism's contribution to democracy had more to do with the external situation of its churches under persecution than with its internal theory.

Taken together, these studies document an important and sobering ingredient in the total Reformation picture: in spite of its epochal character, Protestantism failed to penetrate major centers of Germany. Even where it did succeed, it often failed to alter political and social realities, although it passed through phases where such alterations theoretically should have resulted. The abundant, informative material presented in these works, partly based on archival sources and overflowing into extensive appendixes, still does not fully explain these failures.

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WINFRIED SCHULZE. *Reich und Türkengefahr im späten 16. Jahrhundert: Studien zu den politischen und gesellschaftlichen Auswirkungen einer äusseren Bedrohung*. Munich: Verlag C. H. Beck. 1978. Pp. xvi, 417. DM 88.

This excellent monograph assesses the impact of an external threat, the great Turkish "menace," on the internal history of the Holy Roman Empire during the latter half of the sixteenth century. While the temporal focus is justified both by the intensity of the military danger and by a relative neglect of the period in previous research, it is no limitation on the author, who casts a wide analytical net over the entire "long" sixteenth century. No work on the Turkish threat is more important than Winfried Schulze's, precisely because he has had the imagination to realize the full potential of the subject. Furthermore, the book exemplifies the current conception of constitutional history in Germany, firmly anchored in careful legal and institutional analysis but also comprehending the power struggles and social relations behind formal political structures. Though the school has not moved close to the "new" social history in its attempt to understand group behavior or mentalities, it has developed a

genuine concern for lower social orders as well as elites. More impressively, it has infused the study of public life with a new sense of political reality derived from a full recognition of the role of social conflict in politics and from analysis of the actual *functioning* of institutions. Schulze is a leading representative of this approach, and he has chosen a theme that elucidates many issues in early modern German history.

The Turkish danger was a real one, of course, but was most easily perceived by the governing elites, who led the fight against it and had the most to lose from defeat. They saw popular indifference to the Turks as an "internal threat," which they sought to overcome by creating a horrifying image of the "hereditary enemy" and by preaching a united front to save the Empire and Christendom itself. This ideology sought to stabilize the internal political and social order at a time when it faced not only a military threat abroad but also religious enmity and serious popular unrest at home.

The interplay between external and internal considerations is seen throughout Schulze's analysis of imperial politics, an analysis that confirms—even for the late sixteenth century—the emphasis in recent research on the viability of the Empire. His discussion of the Reichstag, for example, shows the political astuteness of the Habsburgs and reveals that even the most dissatisfied Protestant leaders wished to preserve imperial institutions during this period of acute danger. Unable to gain redress of their religious grievances, the Protestants were nonetheless willing to authorize "voluntary" contributions to campaigns outside the Empire. Schulze devotes most attention to imperial finances and demonstrates Habsburg success in developing a well-functioning financial administration and in collecting over 80 percent of the money appropriated for defense against the Turks. This whole issue gives Schulze a forum for a penetrating analysis of taxation at both the imperial and territorial levels over the late fifteenth and sixteenth centuries, the most intelligent summary available. The discussion of the transfer of the tax burden from the territorial lords to their subjects is yet another example of the analytical richness here. Schulze's fine book is essential reading for all students of early modern Germany, indeed of sixteenth- and seventeenth-century Europe.

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RÜDIGER SCHÜTZ. *Preussen und die Rheinlande: Studien zur preussischen Integrationspolitik im Vormärz*. Wiesbaden: Franz Steiner Verlag. 1979. Pp. vi, 263. DM 56.

When Prussia acquired its extensive Rhenish territories at the end of the Napoleonic wars it inherited

a serious political and administrative problem: how to integrate this western area, which had lived for two decades under a French administration that stressed civic equality, into the Prussian kingdom, which, despite some modernizing reforms, was still organized and governed in the spirit of the old regime. The Prussians were shocked when their well-meaning attempts to introduce good old Prussian laws and institutions into the new provinces were resisted by a populace grown fond of the equalitarian aspects of French rule. How were they to create a real union, a Prussian nation, without some consistent policy regarding the legal status of Prussian subjects? They listened, sometimes, to Rhenish objections. They delayed and temporized, and they made concessions. But in the end they extinguished the most significantly divergent elements of Rhenish multiformity. They eventually Prussianized the Rhineland.

Rüdiger Schütz sets out to investigate this process of Prussianization. He examines the office of provincial *Oberpräsident*, an anomalous third level of administration that might have developed the strength to defend provincial autonomy but never did. He traces the heated arguments over local government from the Rhineland's rejection of Stein's *Städteordnung* up to its grudging acceptance of the *Gemeindeordnung* of 1845, which contained some major concessions to Rhenish peculiarities—concessions that were largely nullified a few years later. And he examines how the king of Prussia backed off of his promise to create a Prussian national parliament after he discovered that Rhenish conditions would not accommodate the *altständisch* foundations upon which he hoped to base it.

Schütz's account of these matters is disappointing. In each case he briefly describes the initiatives from Berlin, and then he reports at great length the negative reactions of Rhenish officials, mostly the *Oberpräsidenten* and the *Regierungspräsidenten*. Finally, he tells the reader how it all came out in the end, the policy ultimately adopted in Berlin. But he makes little attempt to explain why the Rhenish officials adopted their negative positions. What were the experiences, the concrete political pressures that led them to defend the Rhenish status quo against Berlin's centralizing tendencies? Schütz never gets far enough outside of the bureaucrats' offices to be able to tell us. His history is strictly *innerbehördlich*, as he says, and that approach has obvious and severe limitations. Furthermore, in most cases it appears that Rhenish arguments had little if any impact at all on Berlin. The ministers seem to have made their decisions usually on the basis of other considerations. It is not clear why one should care particularly about what the Rhineland officials were saying when nobody was really listening to them.

The book can best be understood as a companion to the same author's *Grundriss zur deutschen Verwal-*

tungsgeschichte, 1815-1945 (1978). That work is basically a collection of documents illustrating the administrative history of the Rhineland. This book, too, is primarily a set of documents—namely, the memoranda and reports of the Rhenish officials. These documents are not simply reproduced, of course, but a large and important part of the text consists simply of paraphrases of them, sometimes in stupefyingly superfluous detail. No doubt this provides valuable raw material for a penetrating study of Prussian Rhineland policy in *Vormärz*, but that study has not yet been written.

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DIRK BLASIUS. *Kriminalität und Alltag: Zur Konfliktgeschichte des Alltagslebens im 19. Jahrhundert*. (Kleine Vandenhoeck-Reihe, number 1448.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 95. DM 9.80.

During the first half of the nineteenth century Prussia experienced the onset of modernization. The economic and social values of capitalistic individualism were increasingly accepted by the governing elites; the state was enlarging its demands on the population, and its bureaucracy was growing in both scope and effectiveness; economic life entered that awkward transitional phase between the downfall of traditional structures and the full arrival of modern industrialism. It is within this context that criminality in Prussia—understood as the number of recorded criminal investigations—rose dramatically, well beyond what might be attributable simply to more efficient law enforcement. In an earlier work Dirk Blasius examined the phenomenon primarily from the standpoint of the reform of criminal law and the judicial bureaucracy. This new essay recapitulates the essence of the earlier argument but focuses more on the perpetrators of crime and their motivations.

Blasius is interested chiefly in petty criminality—small thefts, especially thefts of wood, evasion of taxes or customs, insubordination, arson, and so on—and how it illuminates the condition and mentality of the masses. In general, two types of linkages can be posited: the economic and the political. Economic causality has often been stressed and is easily demonstrated. Pauperism and deprivation were widespread in Prussia in the decades after the Napoleonic wars. That petty criminality, especially theft, could be the product of economic desperation is readily believable. The correlation between the price of grain (rye) and the number of criminal investigations between 1836 and 1865 is very high (Pearson's r exceeds +0.90).

But Blasius finds political causality more per-

suasive in this case. Indeed, he argues that petty criminality during these years should be seen as popular rejection of the norms of the emerging capitalist industrial state and as a primitive form of proletarian class consciousness and solidarity. Evasion of taxes or customs duties and insubordination when confronted by an official represent the unwillingness of the masses to recognize both the authority of the reformed state and the validity of the new legal situation. Similarly the extremely high incidence of thefts of wood—more than three-quarters of all thefts—represents the resistance of the rural poor to capitalist property rights that denied them their traditional rights of gathering scrub wood. Blasius also claims that, contrary to the views of other authors, thefts do not indicate an increase in the valuation of bourgeois property on the part of the masses but rather the growing resentment of inequality. Moreover, he argues that the importance of thefts as an indicator of the onset of industrial society has been overrated. In Prussia the linear trend of thefts does increase between 1836 and 1850, but then it decreases between 1852 and 1866, the period of industrial take-off. Meanwhile, the trend for crimes involving bodily injury increases during both periods. From this Blasius concludes that crimes of assault are the real characteristic of modern criminality in the urban, industrial age because they "reflect the psychic damage inflicted on the way to modern society" (p. 50).

The evidence presented here to support these assertions is rather meagre. Undoubtedly the essay's shortness precluded the inclusion of more details, and it is hoped that Blasius will document his interpretation more fully in a subsequent study. Nonetheless, the present work can be recommended as a stimulating and thought-provoking introduction to the social history of criminality in nineteenth-century Prussia.

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LAWRENCE SCHOFFER. *The Formation of a Modern Labor Force: Upper Silesia, 1865-1914*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xvi, 213. \$13.50.

This monograph traces the development of the labor force in the Upper Silesian mining and manufacturing region during the period of its most rapid growth. It examines the evolution of a population of unskilled ex-peasants into a force of disciplined, committed, "modern" industrial workers, divorced from the land and able to influence conditions of employment through organization and other tactics.

Though at first glance a short book, it is quite

rich both in material and in the variety of questions it touches upon. One of its principal themes is the chronic labor shortage that Upper Silesia experienced during the period under examination. Although situated atop one of the richest coal fields in Europe, this region was unable to compete for labor with the Ruhr and other German industrial areas. It drew its labor force from a surprisingly small area, essentially the immediate region itself. The area's predominantly Polish ethnic character apparently acted as a deterrent to immigration by German workers, while tight control of immigration from Russia and Austria-Hungary (due to concern about the national balance in Germany) prevented the tapping (except on a seasonal basis) of the large labor supply in the Slavic east. Despite this labor shortage, wages remained lower, hours longer, and conditions generally less favorable than elsewhere in industrial Germany; a response in the form of strikes and other union activities came only on the eve of World War I. Lawrence Schofer suggests that workers learned to exploit their relative scarcity earlier: he sees the high levels of absenteeism and job-hopping as tactical moves in this struggle and not as indications of a lingering anti-industrial ethos. But, in the absence of evidence of conscious intent to use such behavior strategically (for example, explicit statements by workers themselves), this interpretation must remain largely conjecture.

Schofer has ranged widely in a number of fields besides history, is familiar with the Polish and German literature, and has amassed an impressive amount of information. To be sure, his work has its share of shortcomings, the most serious of which is the general neglect of the national dimension. Among the variables that affect the applicability of developments in Upper Silesia to general labor history and theory, this is certainly the most obvious and important. Schofer largely ignores the problem of the ambivalent national identity of Upper Silesian Poles—the partial development of a Polish national-political consciousness (in competition with Catholic, socialist, and traditional Prussian world-views) in the midst of increasingly aggressive nationalist policies from Berlin, which dominated most aspects of life in the Prussian east at this time. There is also a regrettable absence of the "subjective" angle, a lack of first-person accounts by workers to compensate for the somewhat bloodless picture one creates when trying to reconstruct the life of any group from official statistics. As a result, due to the imperfect quality of the available evidence or its selective use by the author, one is left with a rather incomplete picture of the Upper Silesian work force. More information on the general state of the local economy—for example, the profit picture and other aspects of management's point of view—would also be helpful. There are frequent

suggestions that Upper Silesian entrepreneurs and managers were less than competent, as slow as their workers to adopt more rational forms of industrial behavior, but we are given little evidence with which to address such issues.

For the most part, Schofer is a social historian of the right kind: open to stimulation from the often rarefied world of social "science," willing to give its notions a try but faithful to the primacy of historical fact (as we know it) over abstract constructs. But there remains a disparity between the quite good material he presents and his conceptual framework. The latter consists of a somewhat ill-defined concept of a "modern labor force" and an idealized view of optimal labor-management relations toward which modern economies are presumed to be moving. On the one hand, the data remain too limited and incomplete to support satisfactorily the ambitious model; on the other, the model is inadequate to handle all of the material that is presented. This inadequacy leads to numerous tangents and causes the sources to determine the book's contents in many cases. Thus, it is not a particularly easy work to read, and locating the numerous footnotes at the back of the book makes reading as much an exercise for the hands as for the eyes.

Overall, historians will probably value this work more for its considerable informational content than for its conceptual thrust. It is certainly a solid research effort that future students of the Upper Silesian situation will be obliged to consult.

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SHULAMIT VOLKOV. *The Rise of Popular Antimodernism in Germany: The Urban Master Artisans, 1873-1896*. Princeton: Princeton University Press. 1978. Pp. ix, 399. \$25.00.

Shulamit Volkov cuts through a persistent ambiguity in studies of European antimodernism by concluding that, "as a reaction to modernity, antimodernism was itself a modern phenomenon" (pp. 326-27). She reaches this position through a careful and sustained examination of the history of German small independent urban master artisans practicing what she terms "traditional crafts" in the last quarter of the nineteenth century. Volkov blends the now familiar psycho-intellectual *Fragestellung* about German antimodernism with very solid social analysis, based on archival research. Following the lead of Heinrich August Winkler, she brings the discussion down to the shop floor. In so doing she renders a necessary criticism of earlier "intellectualist" studies such as Fritz Stern's and my own.

She has ransacked the appropriate archives at

Potsdam and Merseburg as well as looked at selected regional sources of interest. She compares English with German industrial history to illuminate her arguments about the special nature of the German development. And she operates on a level of theoretical sophistication unusual, and welcomed, in a historical work. All of this is to say that Volkov learned her craft in Hans Rosenberg's workshop and that the 1873-96 depression frames her analysis.

In the odyssey of the small urban artisans from the 1840s, when they were open to various political orientations, to the 1920s, when many aligned themselves with National Socialism, she argues that the period sometimes designated the "great depression" marked the crucial changes in their collective biographies. Modernization before the 1870s had distressed many masters; others, as she well points out, benefited from it. But all had to adjust in some ways to Germany's emerging industrial society. In the 1873-96 period, however, "depressed prices, a decelerated rate of economic growth, and considerable material hardship" (p. 60) seemed to reduce their ability to respond rationally to their circumstances, in Volkov's view. The master artisans felt themselves outside the new society: they were offended by the industrial capitalist social structure growing up around them; they were wounded by their journeymen treating them as bosses rather than as fatherly mentors. Until the 1870s the urban artisans looked regularly to the liberal bourgeoisie for political direction and alliances. The "great depression" broke this tie and introduced the "Appeal of the Extremes." Yet, the artisans did not simply give themselves up to some extremist group; rather, they turned inward, hoping to cultivate a sense of corporate identity—which, of course, deepened their political isolation. Attempts at playing the game of interest-group politics failed. Only the irrational options remained, according to this argument. Artisans in increasing numbers succumbed to the lures of anti-Semitism, authoritarianism, monarchism, nationalism (p. 302), anticapitalism, and antisocialism (p. 310). It seems to me inaccurate to characterize this complex dialectic of artisans' ideas, hopes, and fears as simply the inspiration of emotion rather than of logic, as she does (p. 310).

But, if the masters dealt with their reality irrationally, they were encouraged by "conservatives hoping to exploit the Masters' antimodernism for their own ends": namely, "the last effort of the Prussian agrarian aristocracy allied with the big business oligarchy to preserve a social supremacy long outdated and even more precarious" (p. 325). After the war this second-line defense of the elite passed to the lead—behind the leadership of the National Socialists, of course. Thus, in the ongoing debate about the role of fascism in advancing or retarding modernization, Volkov has taken the side of those

who, to paraphrase Arthur Rosenberg, see the Meister Johann Kleinermann as the culprit of National Socialism. He was driven to irrational extremes as a more or less incomprehending victim of "modernization."

Volkov reaches into an old social science portmanteau and finds the concepts of "traditional" and "modern" society. Since the concepts have served as receptacles for reductionist judgments about extremely varied metropolitan and peripheral societies, she might have been more cautious about applying them to the curious commingling of old and new social institutions and patterns in late nineteenth-century Germany—or, indeed, late nineteenth-century anywhere. They do not advance her argument; they paper over problems. It is true that most of the fascist movements of the twentieth century contained what she labels "antimodern" elements, but so indeed does a July 4 or 14 oration by a modern bourgeois politician linking himself to national traditions on the advice of a market research firm. Were the Italian Futurists or the students in the Iron Guard antimodern? Do present-day leaders of corporate capitalist planning owe nothing to the fascist era, to Johann Kleinermann's horror at the capricious play of the market?

Finally, we have to begin to exercise more caution before the temptation of using the "great depression" as a hanger upon which we place the important events of the 1873-96 period. Volkov admits that the aggregate small business sector grew at a rate equal to the German economy as a whole in the crucial period (p. 71). And, although she does take great care to correlate some artisan activity with smaller cycles within the large one, there seems more symmetry than explanation in the effort. For all of the excellent scholarship Hans Rosenberg's approach has engendered—and this is a book of excellent scholarship—it comes at a price. Volkov must systematically eschew the mechanical economic determinist temptations of the "great depression" theory. The theory operates in her book more as a classification scheme than as an explanation. For explanation we are proffered a poor "modernization" model.

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RAINER KOCH. *Demokratie und Staat bei Julius Fröbel, 1805-1893: Liberales Denken zwischen Naturrecht und Sozialdarwinismus*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz. Abteilung Universalgeschichte, number 84.) Wiesbaden: Franz Steiner Verlag. 1978. Pp. xiii, 298. DM 72.

Julius Fröbel belonged to that generation of Germans whose lives were bisected by the revolution of

1848. He was born in 1805, grew up in a Thuringian *Kleinstadt*, gravitated toward radical politics as a young man, and supported himself by working in journalism, publishing, and on the fringes of academic life. The great events of March 1848 seemed to open a new world of opportunity for Fröbel, his party, and his nation. During the revolution, he was active in democratic organizations, served in the Frankfurt Parliament, and finally was forced to flee into exile. After an adventuresome career in the United States, Fröbel returned to Germany, reconciled himself to Bismarck's Reich, and served for a time in the Consular Corps. When he died in 1893, he was an advocate of suffrage restrictions, social Darwinism, and Realpolitik.

Although Rainer Koch has some interesting things to say about Fröbel's life, his major concern is with his subject's ideas. Koch believes that these ideas—largely contained in a series of books and pamphlets written in the late 1830s and 1840s—have two claims on our attention. In the first place, Fröbel's attempt to build a theory of democracy on the "corner stones of freedom, equality, and participation" was more successful than any of his German contemporaries (pp. x–xi). Second, Fröbel represents an essential link in the long but elusive tradition of German democratic thought. In order to define this historical and contemporary relevance, Koch applies a method based on Max Weber's notion of the "ideal type." The product of this method is not a summary of Fröbel's thought but rather an abstraction of its most essential elements, to be presented with a fullness and consistency greater than Fröbel himself might have been able to accomplish (p. 175).

The basis of Fröbel's achievement as a democratic theorist, Koch argues, was his uncompromising demand that sovereignty be located in the people. Suffrage regulations, representative institutions, and the legal system must provide the broadest and richest involvement of the nation in the business of government. At the same time, Fröbel recognized the potential dangers involved in a democratic polity. To avoid these, he insisted upon the protection of individual rights and property as well as upon social reforms to reduce poverty and discontent. In sum, Fröbel sought a dynamic and creative synthesis of popular participation and the protection of individual rights (see Koch's summary on pp. 142–43).

While I have no doubts about the value of this account of Fröbel's ideas, I am uncertain as to how much Koch's accomplishments are due to his use of the "ideal type." Much of this book looks to me like old-fashioned summary and analysis. Moreover, I am uneasy about a method that seeks to present ideas without their inconsistencies and omissions—it is often precisely these elements that tell us the

most about the internal strains and tensions of an intellectual construction. Finally, I am not sure one can or should construct an ideal type of Vormärz democracy from a single individual's ideas. Typicality cannot be conferred on one's subject a priori but can only come from a careful comparison of his ideas with those of his contemporaries. Until Koch turns himself to this task, we shall have to be content with the more limited but still highly rewarding case study he has given us in this book.

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ROSEMARIE LEUSCHEN-SEPPEL. *Sozialdemokratie und Antisemitismus im Kaiserreich: Die Auseinandersetzungen der Partei mit den konservativen und völkischen Strömungen des Antisemitismus, 1871–1914*. (Politik- und Gesellschaftsgeschichte.) Bonn: Verlag Neue Gesellschaft. 1978. Pp. 347. DM 72.

This study of the German Social Democratic Party's position on the Jewish question in imperial Germany proceeds from the premise that theoretical formulations by Marx, Kautsky, Mehring, and others were less important formative influences than the ways in which local party activists and the popular party press dealt with the problem of anti-Semitism. Anyone familiar with Paul Massing's classic study, *Rehearsal for Destruction*, will find little here to change his mind about the official Social Democratic analysis of Judeophobia. What is new and insightful here is an examination of the attitudes and actions of lesser party members and publications.

Rosemarie Leuschen-Seppel shows that the unity of theory and practice displayed by the Social Democrats in their opposition to political anti-Semitism during the 1880s broke down after the anti-Socialist law was dropped and the Stoecker movement collapsed. Marxist theorists continued to condemn all forms of racism, but they belittled post-Stoecker anti-Semitism as a bourgeois affliction that left the workers untouched. Local and regional party leaders, however, carried forward their militant confrontation with anti-Jewish agitators into the 1890s. Of more lasting significance was the growing gulf between Socialist denunciations of the social consequences of anti-Semitism at the political level and occasional employment of unflattering caricatures of Jews at the cultural level. The latter is painstakingly documented in influential (but today frequently ignored) publications designed to amuse and entertain German workers and is illustrated with examples in the appendix. It is attributed principally to the party having underestimated the importance of evolving a true proletarian culture, which left workers and party members easy prey to

integration by state institutions into existing German culture, complete with its racial prejudices. Acculturation was aided by the Socialists' smug assumption that anti-Semitism was on the way to extinction, which encouraged a certain opportunistic toleration of racist symbols, and by the Socialists' unacknowledged nationalism, which deepened their identification with Wilhelminian Germany's economic and cultural achievements. The author concludes that this simultaneous defense and negative stereotyping of the Jews generated working-class ambivalence toward the Jewish question.

Ambivalence there certainly was, but not everyone will agree that its main roots were in proletarian acculturation. Social Democrats had reasons for criticizing Jews that were uniquely their own, not the least of which was annoyance over Jewish politics. They believed that a persecuted minority like the Jews had a special obligation to support Social Democracy, the party of all the oppressed, and it must have seemed to them a monumental case of ingratitude that most German Jews stubbornly remained outside the Socialist camp long after it had proved itself a mighty bulwark against anti-Semitism. That criticism aside, this study deserves high marks for its sensitive and thorough treatment of a difficult topic. It should help put to rest the myth of an anti-Semitic tradition in German Socialism, without in any way contributing to the equally misleading view that Social Democrats were uninfluenced by anti-Jewish sentiments.

DONALD L. NIEWYK
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ROLF WEITOWITZ. *Deutsche Politik und Handelspolitik unter Reichskanzler Leo von Caprivi, 1890-1894*. Düsseldorf: Droste Verlag. 1978. Pp. vii, 406. DM 48.

The author, who received his degrees from the London School of Economics where he worked under James Joll, has constructed a detailed account of the planning and negotiation of the Caprivi trade treaties, based on thorough use of archival materials in Bonn, Coblenz, Potsdam, Merseburg, and Munich. His investigations do not lead him to new political interpretations but rather confirm previous judgments. Caprivi's trade negotiations were an integral part of his "new course" policy of rational adjustment and reconciliation, and, as with his other policies, both domestic and foreign, the overall result was disappointing. Because no fundamental change in the political structure of the Reich was envisaged, Caprivi's reform gestures were doomed to failure from the outset. Lowering the price of bread by reducing the grain tariff and helping to provide jobs and higher wages by encouraging exports did not give the masses the political recogni-

tion that they sought. Rolf Weitowitz judges that the economic impact of the treaties was at best secondary and only a minor assist to a world boom from 1895 to 1914 in industrial production and trade that would have occurred anyway. German trade with the Triple Alliance powers, Austria and Italy, did not increase; the greatest advance in German trade was with Russia, where trade in items with continuing high import tolls increased as much as in those products with reduced rates. Socially, the trade treaties probably assisted in the "negative integration" of the Socialists into German society before World War I, but they also set the style among the ruling groups of looking for "ersatz solutions" to social and political problems instead of real ones. Also, and possibly most importantly, by alienating and provoking the ruling agrarian Junker conservatives, Caprivi not only brought about his own political downfall, but his rational trade policy encouraged them politically to develop an irrational "pre-fascist radicalism." This reviewer must add that Caprivi, as a military man, shared the technician's approach to politics with other civilian members of the bureaucratic German government; that is, he saw the trees but not the forest; his approach was essentially apolitical. Also, the authority of the government could have been brought to bear more effectively, more significant reforms might possibly have been accomplished, and the more rabid Junkers could have been held in check through the medium of a more genuinely liberal and responsible head of the authoritarian state structure, instead of the neurotically posturing William II whose personality accentuated most of the problems of the second German Reich.

J. ALDEN NICHOLS
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GERHARD PADDERATZ. *Conradi und Hamburg: Die Anfänge der deutschen Adventgemeinde (1889-1914) unter besonderer Berücksichtigung der organisatorischen, finanziellen, und sozialen Aspekte*. Hamburg: By the Author. 1978. Pp. vi, 298.

Gerhard Padderatz's volume written in German was originally a dissertation for his doctoral degree at Kiel University in Germany. It is an interesting and well-documented study of the Seventh-Day Adventist Church in Germany and the central role played by Louis R. Conradi in this endeavor. Unfortunately many of the church's documents were destroyed in Hamburg during World War II.

The volume begins with the Millerite movement of the 1840s in the United States, and it describes how the Seventh-Day Adventist Church grew out of this movement. Padderatz emphasizes the influence of Ellen G. White, one of the church's found-

ers, in the development of the early church in the United States as well as in Germany. The author explains how the church was built up in Germany, its triumphs and failures, and Conradi's importance to the movement as the major motivating force. The most important reasons for the success of Adventism in Germany were the distribution of Adventist publications and the holding of evangelistic meetings. Since the church was a new sect and made proselytes from other denominations, considerable opposition to it developed. Other problems that are treated in the book are the relationship of Seventh-Day Adventist young men to the military service, the problem of school attendance on Saturday, and opposition of the civil authorities to the holding of public meetings by Adventists in certain provinces. What is enlightening and shatters the lofty image most members had of Conradi was the revelation of his frailties and mistakes and the problems they caused to the church.

The book leaves many questions unanswered, has certain flaws, and therefore is an incomplete account of Conradi and the Adventist Church in Germany. Padderatz resorts to speculation when he offers no documentation to the charge that the European General Conference was dissolved because of Conradi's moral failure. His research is suspect when he says that no Adventist German bore arms before 1914 and that the only reason they got into difficulty with the military authorities was their refusal to work on Saturdays. This is erroneous and is refuted by authorities in the field.

The question is raised why the author ignored any discussion of the change of church policy concerning military service that took place once Germany was involved in World War I. This is disappointing since it would be very enlightening concerning the character of Conradi and the real nature of German Adventism. His mistake was to take unilateral action approving the bearing of arms for German Adventist men, which was contrary to the official position enunciated by the denominational headquarters in the United States. Conradi was later reprimanded for this unwise decision, and after the war he repudiated his action. German Adventists like to forget this unfortunate episode in their history.

The book is helpful to the novice in giving a broader understanding of the general nature of the development of the Adventist Church in Germany but does not add significantly to historical knowledge. Furthermore, it gives no penetrating interpretations of the significance of the various developments of the Adventist Church in Germany in relation to other denominations and to the communities in which the churches were located.

JACK M. PATT
San Jose State University

RÜDIGER ZIMMERMANN. *Der Leninbund: Linke Kommunisten in der Weimarer Republik*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 62.) Düsseldorf: Droste Verlag. 1978. Pp. 307. DM 64.

The shelf of books on factionalism and schism in the working-class parties of the Weimar Republic is growing long. The subject matter is certainly there: in Germany's troubled years from 1917 to 1932 there were at least eight significant splits in the workers' parties (four in the Communist Party alone). Establishing the particulars of the splits and of the new organizations growing out of them is often a formidable research exercise, with very high standards set by Hermann Weber and others. Yet with the present book—which treats the course and aftermath of a Communist schism of 1926–27—we now have detailed scholarly treatments of all these affairs except the short-lived Communist schism of 1921–22. It will soon be time for someone to draw an integrated picture of the phenomenon.

It is no accident that the Communist schism of 1926–27 should be one of the last to find its historian: it was one of the most confused and least promising. The group headed by former party eminences Ruth Fischer, Arkadij Maslow, and Hugo Urbahns, which was later to found the Leninbund, was forced out of the party at about the same time as a number of smaller "ultraleft" factions (led by figures like Karl Korsch), which never coalesced with the larger faction or with each other, but all criticized the KPD from the left. Then the Leninbund itself proved highly fissiparous, losing Fischer and Maslow (and much of the membership) immediately after its foundation early in 1928 and losing further cadres to the new Trotskyite movement in 1929–30. It had perhaps six thousand members at its foundation, which was also its peak, and fewer than a thousand in the years from 1930 to the end of the republic. Its ideology, much of the time, was conspicuously futile, and its impact on public affairs generally negligible.

Rüdiger Zimmermann has done the work of sorting out the facts carefully and well. In fact, though he writes with unusual (for German scholarship) simplicity and directness and offers pungent commentary as the narrative proceeds, the main part of his story is so detailed that even most specialists will probably use the work only for reference (though reference use is impeded by the lack of a subject index). Fortunately, the author has more on his mind than establishing the facts. In passages of more general interest—particularly the two evaluative chapters at the end—he presents his characters as representative of certain dilemmas of dissident Communists, particularly dissidents who remain loyal to the Leninist conception of a cadre-party

and to an ideal vision of the promise of the Soviet Union. The Leninbund faced, rather helplessly, the twists and turns of Stalinization in Russia and the Comintern in 1926–29; it also faced, sometimes with real penetration, the danger of fascism in Germany. From both confrontations there are general insights to be gained, and Zimmermann succeeds in bringing them out.

DAVID W. MORGAN
Wesleyan University

HANNSJOACHIM W. KOCH. *Der deutsche Bürgerkrieg: Eine Geschichte der deutschen und österreichischen Freikorps, 1918–1923*. Berlin: Ullstein. 1978. Pp. 487.

Another book on the Free Corps? In fact, as Hannsjoachim W. Koch notes, there have been only two major studies of the Free Corps since 1945, Robert G. L. Waite's *Vanguard of Nazism* and Hagen Schulze's *Freikorps und Republik*. But this is somewhat misleading, since other authors, most notably Harold J. Gordon and F. L. Carsten, have written extensively on this subject within the context of larger works. Nevertheless, an updated, comprehensive history of the Free Corps would be of value. Koch's attempt to provide such a history, unfortunately, succeeds only in part.

Der deutsche Bürgerkrieg consists of twelve chapters covering the formation of the Free Corps, their suppression of revolutionary unrest in Germany, Free Corps campaigns in the Baltic, Upper Silesia, and Carinthia, the Kapp Putsch and resultant Ruhr uprising, and, finally, the dissolution of the Free Corps and their subsequent underground activity, culminating in the violent events of 1923. Objecting to Waite's "tendentious" treatment of the Free Corps, Koch presents a more sympathetic—yet generally accurate and convincing—account of the Free Corps' mentality and the feelings of anger, disillusionment, and betrayal that eventually turned many members equally against the young democratic republic and Wilhelmine conservatism.

While providing a broad overview of Free Corps activity, the book on the whole adds little that is new or significant to what is already known. The chapter on the Austrian Free Corps in Carinthia provides some interesting material, but Koch's rather narrow diplomatic-military focus limits its value as a basis for a meaningful comparative analysis. Similarly, the new archival material introduced by Koch, since it originates primarily from diplomatic archives, helps to flesh out the international complications surrounding Free Corps activities but does little to augment the existing picture of the Free Corps themselves. Finally, although Koch, in contrast to Schulze, carries his story beyond the formal dissolution of the Free Corps, his

account of post-1920 events is sketchy and superficial.

With regard to the Ebert government's controversial role in the formation of the Free Corps, Koch takes the position that, given the constraints imposed by domestic political and Allied diplomatic pressures, the government had no choice but to follow the course that it did. Moreover, defeat, revolutionary chaos, and historically conditioned prejudices combined to make it virtually unavoidable that the unexpected alliance of Social Democrats, old military leaders, and newly formed Free Corps would be filled with the misunderstanding and mistrust that led eventually to the Free Corps' alienation from the republic and the army's faulty integration into it. Koch adamantly rejects the idea of a "third way" based on the council movement. Yet one need not accept the councils as a *deus ex machina* to argue that there were alternatives to the military policies adopted in 1918–19. Koch rejects but never systematically refutes the arguments in favor of such alternatives. The result is a somewhat one-dimensional, fatalistic interpretation of events that inhibits substantive discussion of many of the important issues surrounding the Free Corps. A truly analytical, comprehensive history of the latter remains to be written.

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RAINER BÖLLING. *Volksschullehrer und Politik: Der deutsche Lehrerverein, 1918–1933*. (Kritische Studien zur Geschichtswissenschaft, number 32.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 306. DM 48.

The author examines the sociopolitical environment of German public school teachers from the immediate pre-World War I phase to the coming of the Third Reich. In his fluently written monograph, Rainer Bölling devotes much space to organizational history, and in the center of his examination he places the German Teachers' League (*Deutscher Lehrerverein*), which comprised about 62 percent of all organized German primary school teachers in 1907 and 55 percent in 1927. After sketching the situation of teachers before World War I, Bölling moves on to a brief characterization of the main teachers' organizations during the Weimar republican era. An entire chapter is devoted to the sociopolitical and pedagogical objectives of the teachers' league. A further chapter deals with the interrelationship between the league and Weimar political parties. Appropriate attention is paid to the teachers' attitude concerning contemporary school legislation under the jurisdiction of the regional (*Länder*) governments, namely Prussia, and the criti-

cal issue of teacher training. In this latter area Bölling can report some progress for the teachers inasmuch as university norms were introduced in Thuringia into the training process. That this success was short-lived turned out to be one of several disappointments for primary teachers in the republic, who were striving to improve their academic qualifications, their material standard of living, and, consequently, their social status, especially vis-à-vis the highly educated upper school teachers who continued to look down upon their lower-school colleagues. The last chapters of the book are dedicated to the primary school teachers' struggle with adverse conditions during the economic depression at the end of the 1920s and the (resultant) attraction of the Nazi Party.

To this reviewer, the greatest merit of the study lies in the fact that Bölling has authoritatively dispelled the old myth of the German primary teachers' predilection for right-radical ideologies. As the author convincingly shows, the teachers were much more prone to adhere to bourgeois-liberal parties such as the DDP and, further to the left, the SPD. Yet one could argue that, since Bölling has only dealt with the members of the German Teachers' League, he has disregarded anywhere from 30 to 45 percent of the professional group as a whole. He has thus raised the question of the representativeness of his sample. Perhaps it would have been wiser to reverse main and subtitle in this case: this is really a work about the *Lehrerverein* that tells the reader much about "public school teachers and politics" in the period under discussion.

Without question, this book constitutes an important addition to the growing stock of sociohistorically oriented monographs whose task it is to focus on specific (professional) subgroups of German society and to examine in detail their sentiments and patterns of behavior with regard to the various political currents of the day. It is to be hoped that more studies of this nature, concentrating on upper school teachers, lawyers, physicians, and other closely knit interest groups will follow this important volume.

MICHAEL H. KATER
York University

ULRICH SCHÜREN. *Der Volksentscheid zur Fürstenenteignung 1926: Die Vermögensauseinandersetzung mit den depostierten Landesherren als Problem der deutschen Innenpolitik unter besonderer Berücksichtigung der Verhältnisse in Preussen.* (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 64.) Düsseldorf: Droste Verlag. 1978. Pp. 327.

After years of negotiation, costly law suits, and futile attempts at legislation, the disposition of the

property of the former princes remained an unresolved issue for the Weimar Republic. In 1926, the Communists and Social Democrats cosponsored an initiative and referendum on the expropriation of this property without compensation. Despite its popularity, the referendum failed, as did subsequent efforts to resolve these property disputes on the national level, and state governments were left to grapple with the problem. Historians generally interpret the *Volksentscheid* as marginally important, a reflection of a popular mood of negation or a strong republican demonstration without practical results. Only East German historians, for whom the referendum's widespread support represents a successful example of KPD united front tactics, view it as a particularly significant event. While Ulrich Schüren discounts the latter interpretation, he contends that the *Volksentscheid* was of greater consequence than previously recognized. He argues that in Prussia, the state confronted with the major Hohenzollern claims, the referendum was crucial in forcing a compromise settlement. After the referendum the Hohenzollerns and the German National Party in Prussia became more conciliatory; without this change of attitude the Prussian negotiations would have remained deadlocked. To Schüren this indicates that the procedures for direct democracy were more than "nice decorations on the republican constitution" and that initiatives and referendums provided a viable means through which the people could exert direct political influence.

The popularity of the referendum, Schüren explains, was due to "social grievances" and not KPD united front tactics. Ruined by the great inflation, faced with the economic insecurities of 1926, and incensed by potential lucrative compensation for the princes, many bourgeois voters joined the working classes in the demand for expropriation. But any hope of lasting cooperation between these diverse social forces, or even between the KPD and SPD, was an illusion harbored solely by the Communists. The SPD cosponsored the initiative and referendum only because it feared losing constituents to the KPD. Throughout the campaign, the SPD shunned the very notion of an alliance with the KPD and conducted independent political activity; always ambivalent, the SPD preferred a compromise law until the very end. KPD tactics were often counterproductive because they allowed the opposition to brand the referendum as a Bolshevik assault on private property.

Aside from Schüren's analysis of the referendum's impact on the Prussian compromise, little in his account is actually surprising. His book reads like a typical case study of Weimar party politics in which the reactions of various parties and factions are quite predictable. Schüren also seems reluctant to make generalizations that might widen the scope of

his study. Although he refers to the general phenomenon of direct democracy, for example, he does not actually develop this theme nor relate the 1926 *Volksentscheid* to other Weimar initiatives or referendums. Thus it is difficult for readers to maintain their interest through the lengthy descriptions of internal party debates and fruitless negotiations leading up to the referendum.

As a comprehensive study of this rather specialized subject, however, Schüren's work is a very competent piece of scholarship. The book is well organized, clearly written, and based upon extensive archival research and the more important secondary literature. Schüren's study is the most detailed and reliable history of the 1926 *Volksentscheid* to date.

JOSEPH W. BENDERSKY
Virginia Commonwealth University

KARL HOLL, editor. *Wirtschaftskrise und liberale Demokratie: Das Ende der Weimarer Republik und die gegenwärtige Situation*. Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 152. DM 15.80.

Economic crises constitute a severe challenge to the survival of the "liberal state," argues Karl Holl in his introduction, and many would agree. The specter of Weimar is omnipresent again, and the turbulent years of the world economic crisis still evoke enough fears among participants and non-participants alike to stimulate an apprehensive look at present and past conditions. It is quite reassuring, though, that all the contributors to this small volume of essays assure us in good historical manner that the particular circumstances in Germany were quite unique and that—as the only nonhistorian, H.-J. Rüstow, points out—the basic economic problems of the 1930s are solved by now. That may very well be. The historians themselves still struggle with finding out what the main problems in the world economic crisis and in Germany actually were.

The essays presented—on Brüning's policies by H. Mommsen, the *Staatsverständnis* of the left liberals by J. Hess, the panic of the middle classes by P. Wulf, industrial policies by G. Feldman, and the economic rationale behind work creation by H.-J. Rüstow—confirm first of all some of the worst suspicions about the republic and its politicians. The authors emphasize quite unanimously that none of the social and political groups and individuals under discussion truly cared for the Weimar Republic as a democratic republic. And even those who did (like the tiny group of liberals), did not fully understand the rules of the democratic game. They preferred to think in terms of a "predemocratic" ideology of the state beyond factional interests and of an ideal harmony of society rather than in terms of

conflict and a balance of interests. This perspective has been known quite well and has served as a convenient explanation for the decline of the republic. A more interesting consensus seems to be emerging on another theme, which might actually lead back to some of the above-mentioned fears: the very inability of the Weimar politicians, be they democratic or not, and, for that matter, of industrialists and trade unionists to solve any of the political and economic problems they faced during the crisis. Thus, Brüning not only pursued an authoritarian course, he actually worsened the crisis. The industrialists not only followed their own narrow self-interests—which after all might be expected—but also drove themselves and others even deeper into the crisis. Thus, the main conclusion of this book may be that the failures of the authoritarian policies as much as those of the democratic ones ran down the republic and made it an easy prey for the radical solutions of the National Socialists. The failure to perceive and understand the political, economic, and social issues at stake from an authoritarian vantage point seems to be at least as important as the peculiarly antidemocratic ideologies in Germany. One might conclude that the economic crisis showed more clearly the demise of authoritarian concepts than of the barely existent liberal democratic ones.

MICHAEL GEYER
University of Michigan

EDWARD W. BENNETT. *German Rearmament and the West, 1932-1933*. Princeton: Princeton University Press. 1979. Pp. xv, 569. \$35.00.

One of the more puzzling problems in the interwar period of diplomacy is why the Western democracies were willing to accommodate the anti-democratic governments of Germany in the 1930s when they refused similar concessions to the democratic governments of the 1920s. Edward W. Bennett's microscopic account of the rearmament negotiations from 1932 to the end of 1933 provides a complex answer. Bennett starts his study by stating his three-fold purpose: to assess German military intentions and plans; to evaluate Western knowledge of these plans; and, finally, to describe Western diplomatic responses. The organization of the book adheres to this pattern with most of the emphasis on the diplomatic maneuvers of the major powers to accommodate Germany's push for arms equality.

In assessing German military plans, Bennett detects a growing militarism in Weimar; military ends unduly determined, influenced, or closed out options available to the civil government. The German military sought to restore the nation's strength not only to revise the peace settlement but also to

prepare for the day when Germany could seize the initiative in international politics. Needless to say, this concept of an aggressive military preparedness included an array of antidemocratic ideas, including the destruction of socialism, suppression of dissent, and alterations of the structure of Weimar democracy. Unlike most German writers, Bennett argues that military considerations were "possibly the largest consideration" (p. 301) in bringing about the replacement of governments from Hermann Müller through Hitler. It was not that the German military was so keen on Hitler; it was more that he seemed like the best possible choice given a limited number of possibilities. The catch, according to Bennett, was that the military had done so much to limit these options.

British and French awareness of German military plans was quite good. The French especially had accurate, hard intelligence on the Germans, but both misjudged German intentions. The British thought the Germans wanted a small professional army along Seecktian lines that would be cheaper and hence more desirable, while the French thought the Germans were aiming for a mass militia army lead by a trained cadre.

The heart of the book deals with the author's masterful description of how the Western nations started down the road to appeasement with the issue of rearmament. The French were unwilling to fight, but they were willing to support a firm line on the Germans; their Anglo-American friends were less resolute. Bennett comes down hard on U.S. policy, which "bears more responsibility for smoothing the path for Hitler than has been generally recognized" (p. 510). He is equally as hard on the British, for they, too, erroneously rejected the principle of balance of power and permitted German rearmament. In the end the Germans won by default. The dialogue of the deaf, as the French phrased it, was over. The disarmament conference collapsed and with it went collective security and the league.

This is a superb book on a very narrow topic. Bennett's exhaustive examination of the sources, coupled with his usual clearly organized and written presentation, means that this study will remain the standard for a good many years. My major criticism is of the accent rather than the details of the study. Bennett's case for a marked upswing in influence by the German military at the end of Weimar is strong, but I am not completely convinced. Other recent studies argue that the military was not as responsible for the collapse of the republic as Bennett would have us believe. My second criticism of accent is that in his telling of the tale Bennett sometimes overlooks how important the depression was on international diplomacy. The depression mesmerized the leaders of the early 1930s, and no matter how important German rearmament later be-

came, it was not that important at the time. This does not come across as forcefully as it should in the book, especially considering the author's obvious mastery of the topic. But these are differences over accent or nuance; the study is a remarkable piece of work.

EDWARD L. HOMZE
*University of Nebraska,
Lincoln*

WILLIAM CARR. *Hitler: A Study in Personality and Politics*. New York: St. Martin's Press. 1979. Pp. x, 200. \$18.50.

The author intends his book, as he writes in his preface, to be "a contribution to the continuing debate about the historical role of Hitler in the National Socialist era"; his concern, "in a sentence, is with the interrelationship between the personality of Hitler and those social forces which made National Socialism possible." This goal of placing his subject in the context of his time is, as the author says, shared by most biographers and, in the case of a man who, like Hitler, has long been a center of historical scrutiny, is so modest that it would be difficult not to achieve. Hitler the politician, the dictator, and the military commander is portrayed; his intellectual world and his ailments, psychic and somatic, are explored and commented upon. The author is sedulously fair in assessing the manifold possibilities behind Hitler's decisions and performance—so fair that he leaves little opportunity for answering his rhetorical questions with anything but resigned acquiescence. For example, he asks in connection with the crisis that led Gregor Strasser to leave the party in 1932, "is it not conceivable that Hitler exploded with rage and frustration precisely because he was beginning to wonder whether Strasser might not be right?" (p. 35). Or discussing Hitler's anti-Semitism he asks, "can one automatically assume that hatred of the Jews was at all times the dominant strand in his political theory between 1933 and 1945?" (p. 126). Or again, "might it not be that when Hitler singled out the Jews as scapegoats for all the ills troubling Germany he was simply projecting onto them the guilt complex felt by most Germans?" (p. 161). The author's conjectures are certainly possible ones, but putting his questions in such a catch-all fashion makes answering as easy as asking them.

While most historians would doubtless agree with William Carr's premises, this is not true of some of his observations. He calls Ribbentrop, for example, "able but sycophantic." This is an overgenerous judgment of a man of conspicuous ineptitude. Ribbentrop's preparation for the foreign ministry came by way of doing odd jobs in Canada, selling liquor

as well as champagne from his father-in-law's firm, and supplying Hitler with news items taken from the foreign press. Even his wife was surprised when he was appointed foreign minister. So little did he comprehend the world outside Nazi Germany that after the war he wrote a groveling letter addressed to "Vincent" Churchill in which he referred to Hitler as a "great idealist," offering to send Churchill a copy of Hitler's political testament because, as Ribbentrop wrote, "I could imagine that its contents might be adapted to heal wounds . . . [and] in the perilous epoch of our world . . . bring about a better future for all people."

What the author has succeeded in doing is to present his own version of leading theories and speculations about Hitler, but, especially in his psychological section, he leaves the readers to make their own way among them.

EUGENE DAVIDSON
Santa Barbara, California

CHRISTOPHER R. BROWNING. *The Final Solution and the German Foreign Office: A Study of Referat D III of Abteilung Deutschland, 1940-43*. New York: Holmes and Meier. 1978. Pp. 276. \$21.50.

Among the German bureaucrats who took part in destroying European Jewry were members of the Foreign Office. Christopher R. Browning's book is intended as a case study of such civil service personnel, their individual motivation, and their role in the evolution of policy leading to genocide. He examines principally Undersecretary Martin Luther, von Ribbentrop's ambitious deputy in nazifying the ministry, and some half-dozen subordinates of the "Jewish desk" in the "Division Germany" established for that purpose. Whereas Luther was an "amoral technician of power" (p. 28), ruthlessly carving out a political empire for himself within the Nazi hierarchy, his "Jewish experts"—"a random cross-section of German bureaucracy"—were efficiently meticulous careerists who routinely performed the inhuman tasks assigned to them. Neither ideological fanaticism nor blind obedience motivated these "banal bureaucrats"; rather, they sought to preserve untarnished their reputations for reliability. Their sole error, one stated at a subsequent trial, was in not keeping their names off documents wherever possible! (pp. 178 ff). Precisely that failure enables the author to trace in detail "who initiated what" in implementing the Final Solution (or rather solutions), when and by whom information on each phase—from forced emigration through physical extermination—was known within the Foreign Office, and, especially, what "input" the office then provided in the expeditious res-

olution of the "Jewish problem." Its increasingly overriding significance among Hitler's aims meant that prewar marginal involvement gave way to the zealous initiation of measures (for example, regarding the deportation of stateless Jews within German-controlled territories) by these desk-bound killers in order to preserve the office's jurisdictional position vis-à-vis other participating agencies, notably the SS. The patchwork pattern of wartime Nazi international relationships and changing German military fortunes necessarily limited and varied opportunities for "diplomatic arm-twisting" (pp. 125 ff) where such was needed. Within these bounds, "Division Germany" all too successfully helped facilitate the "frictionless operation" of the machinery of destruction.

At least in its broad outlines and country-by-country development, much of this story was already known; nor do Browning's interpretations of persons and events generally depart from those of previous writers. (One exception is his favorable view of Luther's contribution to postponing a *judenrein* Denmark, pages 250-51). Furthermore, his self-imposed focus upon a single department: of the Foreign Office, albeit the key one, sometimes obscures the much wider participation of German diplomats—both high-ranking "conservatives," such as State Secretary von Weizsäcker (pp. 72, 79, 95, and so forth), and ambassadors like Mackensen in Rome—in promoting Nazi goals, evidence for which his account amply provides. Only one, Wilhelm Melchers of the Near East desk (Political Division), displayed "courage, invention and determination" in saving lives, in particular Turkish Jews abroad who were initially unprotected by their government and marked for internment and "resettlement" (p. 156). Among the nations, aside of course from the Danes, the Italians alone systematically and effectively sabotaged Nazi plans wherever they could: "in gratitude for Italian protection, the Jews of Nice [in 1943] . . . raised . . . three million francs to aid Italian victims of Anglo-American air raids" (p. 167). Browning's contribution emphasizes again that the resolve to murder far outstripped that to rescue Nazism's victims.

LAWRENCE D. STOKES
Dalhousie University

MANFRED OVERESCH. *Gesamtdeutsche Illusion und westdeutsche Realität: Von den Vorbereitungen für einen deutschen Friedensvertrag zur Gründung des Auswärtigen Amtes der Bundesrepublik Deutschland, 1946-1949/51*. Düsseldorf: Droste Verlag. 1978. Pp. 204. DM 48.

After Germany's collapse in 1945 the Allies divided the country into four occupation zones. By the end

of 1946, after the Council of Foreign Ministers had negotiated the minor peace treaties and before the zonal division of Germany had changed appreciably, the Allies were ready to take up the German peace treaty at a meeting scheduled to begin in Moscow in March, 1947. At about the same time, certain Germans—among them numerous former foreign service officers who had found their way into private research institutes and the local governments established by the occupation powers—tried to create an agency that could function in the absence of a foreign office, which had gone out of existence in May 1945. They were motivated by various things: the prospect of a peace conference without German participation, hopes that Germans might eventually be invited to participate, and the mushroomlike appearance of uncoordinated and often contradictory public statements on Germany's future by various German writers and officials. The book under review is a study of these people and their efforts.

Based on research in official records and personal papers, enriched by interviews and memoirs, the book is an original contribution. The emphasis is on the German Office of Peace Questions (*Deutsches Büro für Friedensfragen*), its history, and its eventual assimilation into the Foreign Office of the Federal Republic of Germany. Manfred Overesch is at his best on how the question of a "national representation" affected the German Office of Peace Questions. One group believed that the minister presidents of the *Länder* (states) should represent Germany to the outside world, the other that the political party leaders were the true spokesmen for the German people in the absence of an elected central government. The former were strongest in the American zone, where they drew support from Bavaria; the latter in the British zone, where they drew support from Kurt Schumacher and the Social Democrats. Unable to resolve their differences on who should control and instruct the proposed national Office of Peace Questions, the Germans failed to establish even a bizonal office, much less one for the four zones.

Despite its originality, the book is in some respects a disappointment. It is weak on the larger context of the Allied occupation, and it is burdened with much undigested detail on discussions about structure, organization, and jurisdiction. There is little about what the Germans proposed to do or say if they were to participate in the peacemaking. What did they want regarding the Saar? The Ruhr and Rhineland? Reparations? Were they prepared to give up Prussia without a struggle, if they could keep their western boundary?

Overesch accepts Wilhelm Kaiser's inaccurate chronology of the minister presidents' conferences

(p. 93), his English includes "sekret" and "Moscou," and his German includes "gehandikapt," "favorisierte," and "minimalisieren."

JOHN GIMBEL

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FRIEDRICH RENNHOFFER. *Ignaz Seipel, Mensch und Staatsmann: Eine biographische Dokumentation*. (Zeitgeschichtliche Bibliothek, number 2.) Vienna: Hermann Böhlaus Nachf. 1978. Pp. x, 800. DM 125.

It is in the nature of historical writing that there should rarely be a definitive book, particularly in the field of contemporary history: the arguments go on, prompted by the availability of new sources and, of course, by the changing perspectives of the historians. Although this reviewer put Ignaz Seipel on the map, so to speak, with a carefully documented "topical biography," which addressed itself to Seipel as a distinguished but controversial Austrian and, indeed, European statesman of the interwar period, he could not expect his work to be definitive. Responses from the Catholic as well as the Marxist side were inevitable; more important, someone might yet uncover the hitherto-missing papers of the Christian Social Party of the 1920s and 1930s.

The book under review fails altogether to take cognizance of the previous literature on Seipel, including the brilliant though quirky work by Ernst K. Winter. The questions for the reviewer and reader are, then, whether or not the discipline of history is so cumulative as to require the historian to consider previous research and interpretations and whether or not the volume under review has enough new materials and perspectives to justify its publication in the scholarly vacuum in which it clearly maneuvers.

Friedrich Rennhofer's new biography of Ignaz Seipel is a labor of love more than of scholarship. The new documentation on which it claims to be based is minimal. Moreover, some vital political materials have not been consulted: those of the official Austrian archives only spottily; those of the German Foreign Office not at all; the party archive, alas, has not been found. Furthermore the eight-hundred-page narrative is devoid of any attempt at analysis and interpretation. A study of Seipel simply cannot avoid, as this one does, dealing with the issues that make Seipel a statesman of distinction and importance: Seipel as a representative of political Catholicism confronted with the problem of accommodation to modernity and as a postimperial figure in a world of independent "successor states" jockeying for one or the other pattern of Central European order. Seipel's positions on monarchy, re-

public, democracy, the corporative state, National Socialism and his stand on Austria's independence, neutrality, the Anschluss, and Danubian confederation are all issues that must be faced head-on by anyone aspiring to make a scholarly contribution to twentieth-century Central European history with the focus on Seipel.

A mere "biographical documentation," which the volume under review aspires to be, is not enough, in particular since Seipel as a private person and as a mind would barely justify a major historiographical effort. Only the political intent elevated Seipel above the commonplace and the parochial. Nor is hagiography, the genre to which this volume in fact belongs, enough, since Seipel, far from being a saintly personage, made his mark in history by an essentially political vision and by his particular way of dealing with the temptations of political power.

The absence of analysis and critical approach is manifest all over this book. A case in point is the treatment of Seipel's wartime work, *Nation und Staat*, whose contents are taken for gospel by the author rather than analyzed for what they are: a poor man's Lord Acton and a poor man's Meinecke (both of whom Seipel, in fact, seems to have been unaware of) and also a political document—a significant critique of the modern nation state from the perspective of the Habsburg supranational idea and, moreover, a striking critique of the Ausgleich and its consequences.

Was the socialists' epithet for Seipel, the "prelate without mercy," merely "a dangerous slogan based upon some few words taken out of context" (p. 514)? And can Seipel's concept of "true democracy" be taken on face value? Does it not require semantic scrutiny that leads away from what is commonly understood by democracy? After all, at about the time that Seipel was lecturing about "true democracy," he engaged in a close exchange of ideas with none other than the chief theoretician of Catholic corporatism in Austria, Othmar Spann (whose mention is strangely altogether omitted in Rennhofer's book), and also came into close contact with the Heimwehr. Although this evidence does not make Seipel an "architect of counter-revolution," it certainly suggests a disillusionment on his part with the workings of parliamentary democracy and the failure of the policy of accommodation.

We have cause to be grateful to Rennhofer for having added to our knowledge of Seipel's day-by-day movements; but to the argument concerning Seipel's statesmanship and historical stature this volume has contributed close to nothing.

KLEMENS VON KLEMPERER
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GERHARD BOTZ. *Wien vom "Anschluss" zum Krieg: Nationalsozialistische Machtübernahme und politisch-soziale Umgestaltung am Beispiel der Stadt Wien 1938/39*. Introduction by KARL R. STADLER. Vienna: Jugend und Volk. 1978. Pp. 646.

As his choice of title already suggests, Gerhard Botz pursues a twofold objective, offering a history of Vienna under National Socialism until the outbreak of the war and analyzing National Socialist policy to illuminate through the concrete Austrian example the methods of government by the Third Reich. The book is a very detailed and competent study of developments in Vienna during a turbulent period, a study that leaves out no important aspect of life in the former Austrian capital. The author gives special attention to the seizure of power from "below," from "above," and from the outside," to the plebiscite of April 10, 1938, and to the early attempts to win over the Catholic Church, the working masses, and other groups; he also focuses on Nazi policy toward the bureaucracy, the Jews, and the churches and on the growing opposition to the Third Reich and the annexation. In spite of the apparent Austrian mass consent or acquiescence to the *fait accompli* of the Anschluss, Botz does not look upon the 1938 union as a voluntary link-up, such as many Austrians had previously desired. The author holds that in the interwar period the relevancy of the idea of union and of the movement toward Anschluss was at times indisputable. But the total dissolution of Austria and the extinction of its very name during the National Socialist domination of the country, in combination with repression and terror, became the catalyst for the self-discovery of Austrian nationalism and the development of Austrian state-consciousness and led to the creation of that minimal consent among former political opponents about democratic rules without which the country could not have experienced a rebirth. Botz freely admits the initial enthusiasm of many Austrians for the Anschluss, the widespread apathy of the population vis-à-vis the dictatorial methods of Nazi politics, and the absence of early political resistance—all matters about which thoughtful Austrians still display some uneasiness. But soon, he asserts, National Socialism lost this early mass consent.

This study is unquestionably the fullest treatment of the period in any language. The work of a younger Austrian historian, it is marked by a high degree of objectivity and strength of democratic convictions; Botz does not refrain from voicing his opinion about the basic "inhumanity" in theory and practice of the NSDAP and about its leaders and activists, though his purpose is to explain rather than to condemn. Equally important, there is no

trace here of the *grossdeutsch* infatuation that gripped not only German but also Austrian historiography of the interwar period and even survived the birth of the Second Austrian Republic.

The book should have included a short treatment at least of how the rest of Austria fared under the prewar Nazi occupation. Also, due to the structure of the study, some minor repetitions have apparently proved unavoidable: the author returns frequently to themes with which he has already dealt. Although there is little doubt that even during the limited time of a year and a half Nazi policy in Austria underwent some modifications in emphasis, methods, and tone, the basic objectives of National Socialism in most areas hardly underwent a radical change. Finally, an introductory account of the Anschluss movement, 1918–38, using recently published studies (such as those listed in footnote 13, page 562, but not in the bibliography) as well as some other books dealing with the First Austrian Republic, would have provided a needed historic perspective. But Botz's writing is lucid, his analysis penetrating if not always original, and his impartiality exemplary. Although he is preoccupied with the intraparty struggle of the leading Nazis in Austria—with the “institutional anarchy” of Nazi politics that left Hitler in the important role of “ar-biter”—he does not neglect social and economic problems of Austria's development.

Since the study focuses on “the relatively most successful and the least inhuman phase” of the domination of German fascism in Vienna, the author tries to balance a perhaps false impression the reader might gain by letting Karl R. Stadler write an introduction “Provinzstadt im Dritten Reich,” which sketches the fate of the city until 1945, dwells on Hitler's ambivalent though always hate-filled attitude toward Vienna, and summarizes the permanent human and material losses suffered by all of Austria. Stadler recalls that, as consequence of seven years of Nazi occupation, altogether half a million Austrians lost their lives: some died in concentration camps and Gestapo jails; fifty-one thousand five hundred Jews were deported and annihilated; and three hundred and eighty thousand Austrians never returned from the battlefield.

ALFRED D. LOW
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PIERRE CASPARD. *La Fabrique-Neuve de Cortaillod: Entreprise et profit pendant la Révolution industrielle, 1752–1854*. (“Recherches,” number 29.) Paris: Publications de la Sorbonne. 1979. Pp. 227. 95 fr.

This monograph analyzes a Swiss cotton cloth (calico) printing firm located in Cortaillod, near the

city of Neuchâtel. Pierre Caspard has found rich archival materials in the Neuchâtel State Archives, where the company's records now rest, and in other local repositories. He also has used a surprisingly abundant secondary literature on this industry (*indienne*) in Western Europe. Caspard suggests that the century-long history of the Fabrique-Neuve illustrates the growth of a leading industry in the later eighteenth century, that it reveals how a large manufacturing enterprise operated in this period, and that it shows the traditional vitality of Swiss capitalism, always seeking opportunities for its skilled labor and capital.

In the early eighteenth century the over twenty-five thousand inhabitants of the principality of Neuchâtel, then a dependency of Prussia, lived largely from agriculture, but in the towns and villages a vigorous commerce flourished, based on the export of wine and import of manufactures and foodstuffs. Caspard argues that the economic elite, disgruntled by the low rate of return on farm land, sought to develop manufacturing in order to draw more people into the area, which would increase demand for farm produce and consequently raise prices and profits, and to find a more remunerative outlet for capital. Two new industries had already appeared in the principality in the late seventeenth century: lacemaking and watchmaking. Calico printing arrived just after 1700, favored by prohibitions on its manufacture and sale in France and England. In common with the other new activities, it dealt with a product of low bulk but high value. This business developed slowly at first, employing but two hundred and fifty persons in the principality by mid-century. At this point Claude-Adam Dupasquier established the Fabrique-Neuve, which began printing calico in 1752. Four generations of his family operated the factory for a century. A peculiarity of this firm was that for most of its life it engaged only in manufacturing. A wholesale house in the city of Neuchâtel obtained the cotton cloth, contracted with the Fabrique-Neuve to print it, and then sold the product. Close financial and personal ties existed between the manufacturing and selling firms, and in 1818 they finally merged.

In several interesting chapters Caspard analyzes the operations of the factory, discussing its capital, the evolution of production, costs and selling prices, and profits. He includes many tables, charts, and maps as well as photographs of some of the buildings that still remain. The calico printing industry in the region employed a peak of some two thousand workers in the 1780s. The apogee of the Fabrique-Neuve's success came in the following decade when its output was the largest in Continental Europe, and it employed some six hundred workers. Difficulties ensued thereafter, first the commercial

problems occasioned by the Napoleonic Wars and then tariff protectionism in France and elsewhere. Labor costs rose to higher levels than in neighboring countries. The Fabrique-Neuve tried to develop markets overseas but met serious competition from large producers in major countries. The company did not play the card of heavy investment in new technology. Management reluctantly dropped cloth printing in 1854, shifted the factory to watchmaking, and then in 1879 turned to cable manufacturing in which it is still engaged. Caspard does very little with social and labor matters in this study but has published his findings on these subjects elsewhere.

His book should be added to that of F. Jequier on the Fleurier Watch Company and the articles of R. Darnton on the Société Typographique de Neuchâtel as another important recent contribution to the fascinating economic history of the principal city and canton of Neuchâtel.

JAMES M. LAUX

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ERICH GRUNER, editor. *Die Wahlen in den Schweizerischen Nationalrat, 1848-1919: Wahlrecht, Wahlsystem, Wahlbeteiligung; Verhalten von Wählern und Parteien; Wahlthemen und Wahlkämpfe*. Volume 1, *Erster und zweiter Teil*; volume 2, *Anmerkungen*; volume 3, *Tabellen, Grafiken, Karten*. Assisted by GEORGES ANDREY *et al.* (Helvetia Politica. Series A, number 6.) Bern: Francke Verlag. 1978. Pp. 1189; 300; 540.

Erich Gruner's *Elections to the Swiss National Council, 1848-1919* is a monster of a work. The first volume, divided into two separately bound parts, consists of some eleven hundred pages of small print. Volume 2 contains the "footnotes," three hundred pages of them, packed with additional information; the oversized volume 3 has hundreds of tables, charts, graphs, and maps.

The work covers the first seven decades of the modern Swiss state. It begins in 1848, when the new constitution created a federal legislature with two houses, roughly on the American model. The lower house, the National Council, was elected on the basis of a modified majority system according to the number of people living in the Swiss cantons. One never would have suspected that such a harmless sounding statement would necessitate two thousand pages of print to explain all of its ramifications, peculiarities, and meanings. 1919 is a natural ending point for the work since in that year the majority system of electing national councillors gave way to the system of proportionate representation according to party strength. The work is a detailed history of the first decades of the modern Swiss con-

federation as well as a detailed study of a system of representative democracy, probably the most thorough in existence in any language about any country.

Historical approach (the story of what actually happened during the elections in all twenty-five Swiss cantons) and political science methodology (the analysis of underlying issues) are roughly balanced. This dualism marks and at times mars the work. There is little internal cohesion. Chapters usually begin with a review of the literature on the subject, but discussions of pertinent secondary works and sources also turn up frequently in the text and the footnotes. Technical dissertations about computer analyses of statistical data are interspersed with narrative segments. Parts of the work are written in French; a French résumé of the German-language chapters provides a convenient short-cut to the materials.

From 1848 to 1917 there were twenty-four general elections at three-year intervals. The history and analysis of each one of these elections form a major part of the work. The elections to the National Council are seen as a means for an established political system to adapt itself to changing economic and social circumstances. The adaptation took place in small steps across a series of elections and through varying party alliances. The Swiss political culture is characterized as having been able to soften the impact of painful conflicts or to prevent them altogether. Yet elections during this period never degenerated into mere routine acts of voting. They often were almost direct plebiscites on the decisions of the government and closely paralleled referendum balloting.

During two dynamic phases of economic expansion and social change (1849-73 and 1894-1913) the elections helped to bridge differences and balance conflicting interests, largely under the umbrella of the dominant Radical Party. Only when this power of adaptation was lost, due to the pressure of World War I and growing class strife, did the majority system collapse and have to make room for elections under a system of proportionate representation.

It is obviously impossible to give even an overview of the unbelievable wealth of information provided by Gruner and his collaborators. Whether they deal with the constitutional and legal bases of the system, the election process, voter participation, parties, political background of the formation of electoral districts, election themes and struggles, preparation and execution of elections, or peculiarities of Swiss elections under the majority system in plural electoral districts, they always seem to exhaust the subject. Their work is the definitive treatment of an important aspect of recent Swiss history.

Thanks to the expertise and methodological thoroughness of its authors, it will become a model for similar studies in other countries.

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JEAN-CLAUDE HOCQUET. *Le sel et la fortune de Venise*. Volume 1, *Production et monopole*. Lille: Université de Lille III. 1978. Pp. 352.

Venetian commerce was based on salt. Already before the year 1000, the Venetians found salt to be the most profitable export from their lagoons. They brought salt inland to exchange for grain and other necessities. This important book describes the production of salt and the mechanisms of Venetian control over the supply of salt throughout the Venetian empire from the mid-thirteenth to the early seventeenth centuries.

Initially, Jean-Claude Hocquet defines the metrology of salt and describes the techniques and areas of its production in late medieval Mediterranean Europe. The Venetians dealt in sea salt, produced along the Mediterranean and Adriatic, where salt was obtained by the evaporative action of the sun on salt water. Several different qualities of salt were produced from the Istrian peninsula, the coasts of Dalmatia, Albania, Apulia, and the Ionian islands. Venetian ships also brought back different kinds of salt from the Aegean, Syria, Egypt, Libya, Sardinia, and the Balearics.

Venice attempted to monopolize the supply of salt in its trading area, but, as the author admits, in the end the Venetians maintained only a very imperfect monopoly. Venice controlled the supply of Adriatic salt by concentrating production at certain locations along the upper Adriatic. This salt could be carried by sea only to Venice, making Venice the staple port. Those salt-producing areas owned outright by Venetians were forced to return a percentage of the harvest to the Venetian state. Other salt-makers had to sell most of their salt to the Venetian Salt Office at a fixed price. The remainder of the salt was consumed locally or sold at a higher price to muleteers bringing foodstuffs from the hinterland. Venice obtained an alternate supply from its fleets returning from farther voyages. With these imports, Venice became the sole source of supply of salt for the Veneto and Lombardy.

Hocquet demonstrates that the quantity of salt produced in each locale was subject to extreme variations from year to year, month to month, and even week to week. He argues that in the short run this variation was caused by the climate. For the *longue durée*, government policies influenced the supply more than the weather. The Venetian monopoly re-

stricted salt production on the Adriatic during the thirteenth and fourteenth centuries in order to encourage its fleets to bring Mediterranean salt back home. By the sixteenth century, however, the Turkish expansion and the decline of Venetian overseas commerce caused Venice to support once more the production of salt on the Adriatic coast.

The themes and methodology of this fine book are typical of the *Annales* school, to which the author belongs. Twenty years of archival research have produced this monograph with its exhaustive fifty-seven pages of bibliographies, index, table of contents, and three excellent maps. The author promises other volumes on maritime commerce in salt, salt production in Venice itself, and the markets for Venetian salt and its *gabelles*. This book succeeds admirably in extending our knowledge and understanding of Venetian commercial leadership during three hundred and fifty critical years.

LOUISE BUENGER ROBBERT
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GIORGIO PORISINI. *Bonifiche e agricoltura nella bassa valle padana (1860-1915)*. (Studi e Ricerche di Storia Economica Italiana nell'Età del Risorgimento.) Milan: Banca Commerciale Italiana. 1978. Pp. 436.

Land reclamation in the lower Po Valley may not sound like a topic likely to generate much controversy. This is precisely what has happened, however, in the context of Italian history where land reclamation in that region is closely tied to developments of national scope. It is a merit of this book that it discusses its narrow topic in a broad context marked by the consolidation of agricultural capitalism and by the evolution of a special relationship between government and private initiative. Another important dimension of the topic, namely, the emergence of an organized agricultural proletariat, is reserved for separate treatment in another volume in this series that is scheduled for publication in the near future. Although the decision to deal with labor separately may leave some readers with an incomplete understanding of the social ramifications of land reclamation, it is probably justified by the complexity of the issues discussed here by Giorgio Porisini and by the vast documentation that he incorporates. Indeed, with more than half the book devoted to statistical tables, we are given what amounts to a small portable archive. The rest is well written, clearly organized, and easy to follow.

Porisini is most interested in the evolving relationship between public and private initiative. The turning point in this relationship occurred in 1876, when the liberal left came to power with a vaguely

defined program of meeting popular needs. Ironically, in the case of land reclamation such a program enabled well-off private investors to reap huge profits by buying at low prices extensive tracts of marshy, underutilized land and reselling the same land at inflated prices once the government had committed itself to reclaiming that land for reasons of public health and economic progress. As the author points out, "the distinguishing trait of Italian legislation in matters of land reclamation was the introduction of hygienic and sanitary considerations as determining and justificatory grounds for extending government support to private property interests" (p. 76). The author's documentation appears to be most convincing precisely when he analyzes the web of private interests that inspired and guided the appropriate legislation through parliament.

More puzzling and less convincing is the author's assessment of the impact of land reclamation on the agriculture of the Po Valley. On this point he wavers between positive and negative views, with the emphasis definitely on the latter. For, while he discusses at length several instances of newly formed enterprises that operated efficiently with the most up-to-date techniques, he also insists that on the whole land reclamation led to the formation of speculative enterprises more interested in short-term profits than long-term productive investments. His conclusion is that extensive public support for private initiative in the form of outright subsidies and tax incentives did not produce sustained economic growth and that sizable private profits were not reinvested in agricultural enterprises. Thus the author reiterates familiar themes of current economic analysis in Italy.

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MEIR MICHAELIS. *Mussolini and the Jews: German-Italian Relations and the Jewish Question in Italy, 1922-1945*. Oxford: Clarendon Press, for the Institute of Jewish Affairs, London. 1978. Pp. xii, 472. \$39.00.

Fascist Italy's "Manifesto of the Race" was promulgated in July 1938. Given Mussolini's earlier barbs directed at the anti-Semitism of "that idiot in Berlin," his conversion to racism comprises one of the trickier conundrums of fascist historiography. Above all, it must be asked how much it was due to the Rome-Berlin Axis. In the book under review the themes of Italian Jewish persecution and Italo-German relations are intertwined—a successful enough design save for a weak chapter on the outbreak of World War II that loses sight of the Jewish question.

The strength of Meir Michaelis's work lies in its careful delineation of Mussolini as a covert anti-Semite from the beginning, albeit one who never degenerated into a rabid executioner. The Duce's ambivalence is ascribed to a desire to mediate between international Jewry (he believed such a force existed) and the genocidal fanatics—rather in the way he aspired to mediate diplomatically between the Western powers and Germany. His goals were moderate anti-Semitism and selective revisionism. Freedom of maneuver on both counts, however, vanished by 1938. The search for a "dynamic ideology" thereafter led straight to an imitation of the Nuremberg Laws; "Mussolini had all sorts of grievances against the Jews but only one reason for persecuting them as a 'race'—his ill-fated alliance with a Jew-baiter" (p. 125). It follows that the Fascist regime's claim (given credence lately by Luigi Preti and Gene Bernardini) to an indigenous racism, nurtured in Ethiopia and based on peoples and nations rather than race, receives short shrift here. Yet, while affirming the "Aryan-Nordic" character of fascist racism, Michaelis is at great pains to circumscribe Nazi Germany's role. German encouragement there certainly was, but Nazi influence was always "indirect." In the final analysis, "the Duce's sudden declaration of war on the Jews" was "unrequested and unexpected" (p. 190).

Fascist Italy's embrace of racism was determined by Mussolini alone. It was not forced on him by Hitler nor by the radical racist wing of the PNF. Interlandi and Preziosi spoke for Mussolini, not he for them. With this contention Michaelis refutes Renzo De Felice, *Storia degli ebrei sotto il fascismo* (1961). Michaelis is also somewhat kinder to the Vatican than De Felice.

Inevitably, *Mussolini and the Jews* invites comparison with De Felice's earlier work. Both books are organized in two chronological sections with the rise of the Axis serving as a watershed, and basically the same narrative ground is covered. But Michaelis's source material is far richer; whereas De Felice relied almost exclusively on Italian documentation, Michaelis has made splendid use of non-Italian archival records recently accessible in Israel, Germany, and Britain. De Felice was a pioneer, moreover, in the controversy over Fascist anti-Semitism; Michaelis, conversely, has profited by taking into account fifteen years of scholarly debate, which lends his own interpretations a sense of judicious consideration. In consequence, *Mussolini and the Jews*, although we may be sure it is not the last word on the subject, affords a comprehensive and sensitive treatment that will be very hard to match.

ALAN CASSELS
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LAJOS DEMÉNY *et al.*, editors. *Răscoala secuilor din 1595-1596: Antecedente, desfășurare și urmări* [The Szeklers' Uprising of 1595-96: Antecedents, Evolution, and Results]. Summary in German. Bucharest: Editura Academiei Republicii Socialiste România. 1978. Pp. 336. 19 L.

In the sixteenth century the spread of the so-called second feudalism in Eastern Europe reached the Magyar-speaking mountain people who lived in the Transylvanian Carpathians. These Szeklers were free villagers who had preserved egalitarian, communal forms of organization from their tribal past and who played an important role as defenders of the Hungarian kingdom's eastern frontier. The Szeklers resisted the change, even as their traditional social forms were weakened by internal differentiation into new classes. A nobility was formed from those who could provide armed horsemen, a large class of commoners was relegated to the ordinary infantry, and the poorest, without draft animals or sufficient land, fell into serfdom.

Transylvania was in a state of endemic war among the Habsburgs, Ottomans, and the virtually independent (after 1526) local princes as well as among the Poles and Vlachs. The Szeklers were a vital military resource for the participants in these conflicts, who tried to win them over as best they could. Domestically, the free Szeklers were fighting to prevent further encroachments on their traditional rights and to curb their nobility. In 1562 there was a large-scale revolt, which was severely repressed by the Transylvanian government. But resistance continued, and there were uprisings in 1595-96. Szekler discontent was cleverly exploited by the Wallachian prince Michael the Brave in the late 1590s, when he briefly conquered Transylvania during his challenge to the Ottoman Empire. He guaranteed the common Szeklers their freedoms; and, even after his death and the return of the Magyar princes, the authorities bowed to the inevitable and allowed this situation to persist. Szekler society was too independent and warlike to be subjugated by a nobility, and it provided too important a resource for the princes of Transylvania to be tampered with.

This first-rate work of Magyar scholarship in Rumania was written by fourteen different authors. It traces not only the diplomatic and international imbroglios of the century but also the social and economic changes among the Szeklers. Much of the material might be put to use by comparative historians studying the survival of similar free, warlike mountain communities throughout Europe, particularly in the Swiss Alps and the Pyrenees. The book also shows that during this period, at least, ethnic particularisms were weaker than they were to be-

come. While the nobles, princes, and emperors shifted alliances and scrambled to increase their revenues and powers, the ordinary Szeklers were just as opportunistic and joined those who promised them the most freedom and security—no matter what their religion or native language.

The reader is given a good sense of why the various actors behaved as they did, what their interests were, and how they tried to satisfy them. A flexible use of Marxist categories is combined with a good dose of common sense and skillful inspection of the documents. The Rumanians occasionally issue translations of their scholarly works, and one can only hope that they choose to do so with this book. It now has a six-page German summary, but this is too brief to help the Western scholar who might not read Rumanian. This piece of scholarship deserves a wider audience.

DANIEL CHIROT
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MIRCEA MUȘAT and ION ARDELEANU. *La vie politique en Roumanie, 1918-1921*. Translated from Rumanian by RADU CREȚEANU. (Bibliotheca Historica Romaniae, Monographies, number 19.) Bucharest: Editura Academiei Republicii Socialiste România. 1978. Pp. 259. 20 L.

The Rumanian original of Mircea Mușat and Ion Ardeleanu's *La vie politique en Roumanie, 1918-1921* was the first of a number of very basic historical studies to appear on twentieth-century Rumania before World War II. (Among other such works are Mircea Iosa and Traian Lungu, *Viața politică în România, 1899-1910* [1977]; Anastasie Iordache, *Viața politică în România, 1910-1914* [1972]; Mihai Ruseanu and I. Saizu, *Viața politică în România, 1922-1928* [1979]; and Emilia and Gavrilă Sonca, *Viața economică și politică a României, 1933-1938* [1978].) The Mușat and Ardeleanu book is the only one to be published so far in a Western language. It is based on Mușat's dissertation of a decade ago, and the present French version is a translation of the greatly revised and expanded second Rumanian edition of 1976.

The book's virtues are a straightforward sorting out of the various political groups and their activities during these three formative years in the development of Greater Rumania. The book's major limitation is an understandable tendency to tread very cautiously with respect to interpretation.

Each chapter of *La vie politique* treats one party, beginning with a short history of the group at hand, proceeding to an interesting analysis of the class affiliation of its leaders and supporters, continuing with an exposition of party ideology, and ending in

a review of party activities during the years 1918–21. All of this is competently, and sometimes quite imaginatively, done. Given that the book is entitled *La vie politique*, however, rather than *Les partis politiques*, one might wish for a separate chapter on King Ferdinand, who played a central part in the political drama of these years, and for a fuller account of his very close working relationship with Liberal Party leader, Ion I. C. Brătianu. Likewise, the study says virtually nothing about the personalities of such key political figures as Brătianu, Iuliu Maniu, Ion Mihalache, and General Alexandru Averescu, although the characters of the political protagonists were often crucial determinants of public policy in interwar Rumania. Nonetheless, a good many questions of fact are clarified and some central issues, deserving further serious research, are raised. Particularly illuminating, for example, are the discussions of the successive reorganizations of the National Liberal Party and of the composition of Averescu's People's League.

The authors make good use of a wide variety of published sources—newspapers, diaries, books, journal articles, and pamphlets—as well as a certain amount of archival and mostly unpublished material, such as the memoirs of Constantin Argetoianu. The bibliographical information provided will certainly prove invaluable for future researchers. A list of cabinet members in successive governments and a chart on parliamentary representation are useful too, though unfortunately there is no index whatsoever.

The publication of *La vie politique en Roumanie, 1918–1921* is a most encouraging development. The book makes a very substantial contribution to our understanding of the workings of interwar Rumanian political parties. It is to be hoped that other books of this sort will also appear soon in translation.

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BIANCA VALOTA CAVALLOTTI. *Nicola Iorga*. (Gli Storici, number 7.) Naples: Guida Editori. 1977. Pp. 312. L. 4,500.

Bianca Valota Cavallotti's bio-bibliographic study of the Rumanian historian and politician Nicolae Iorga is a succinct if somewhat superficial volume, apparently designed to acquaint Italian readers with the prodigious career of a man whose intellectual formation and activities were somehow related to his work and life in Italy.

The author rightly depicts Iorga as a Renaissance figure, as a giant among the historians and in-

tellectuals of late nineteenth- and early twentieth-century Rumania. She also rightly emphasizes the innovative character of his historical and political thought prior to his becoming, early in the twentieth century, a less than innovative exponent of nationalist ideology and, after the establishment of Greater Rumania in 1918, a less than innovative politician and historian of national heroes and nationalist themes. Cavallotti's study is uncritical chiefly because it is largely based on the written work about and oral accounts of Iorga the man and historian and of his place in Rumanian history and historiography by contemporary Rumanian historians and political leaders. Consequently, a somewhat idealized and distorted image of Iorga and of his work emerges from Cavallotti's volume.

Iorga's significance as a historian rests in the imaginative and often incisive interpretations of major historical problems that transcended the narrow confines of traditional, national, Rumanian history. Iorga's brilliant intellect, fertile mind, and great facility in expressing complex ideas orally and on paper allowed him to use historical data for a variety of purposes not necessarily related to rigorous historical scholarship. His early work was only indirectly related to Rumanian history, and it seems fair to say that his *Geschichte des Osmanischen Reiches* (5 vols., 1908–13) was his best scholarly contribution. His later work, mostly connected in one way or another with Rumanian political problems, was gargantuan in size and scope but often lacking in objectivity and clarity. His reputation among Rumanian intellectuals was generally greater than that which he enjoyed among fellow historians, who could find fault with his methodology, inaccuracy of bibliographic references, and generally polemical style and conclusions.

It is noteworthy that Iorga, the man and his work, were in eclipse for many years after World War II until the nationalist tendencies of Rumanian communism became manifest in the late 1950s. The gradual development of a new Iorga cult during the last twenty years is intimately related to contemporary interpretations of the historic legacy of the Rumanians in relation to traditional foreign enemies and to the place of the Rumanians in universal history.

Cavallotti pays scant attention, if any, to these aspects of Iorga's career. Her work is essentially an Italian version of works on Nicolae Iorga by contemporary Rumanian historians that will allow Italian readers to acquire a sense of the stature and significance of Nicolae Iorga in the history and historiography of Rumania, past and present.

STEPHEN FISCHER-GALATI
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Boulder

DAVID P. DANIEL. *The Historiography of the Reformation in Slovakia*. (Sixteenth Century Bibliography, number 10.) St. Louis: Center for Reformation Research. 1977. Pp. 50. \$2.50.

Despite the unpromising parochialism of its title, this slender pamphlet is surprisingly stimulating to read. Part of its provocativeness lies, of course, in the sheer novelty of its subject. American historians know all too little about the role of the Reformation in Eastern Europe; to be led by a thoughtful scholar through its bibliographical complexities in an area as important as Slovakia is a welcome experience. But more interesting still is the way in which the historiography of Slovak Protestantism or, more properly speaking, Lutheranism seems to mirror the history of the Slovaks in general. If ever there was a people to have been caught up in the multiform political and ideological vagaries of Eastern Europe, it is they. Hungarian expansionism, Habsburg dynastic imperialism, nationalism, fascism, communism—all have had their day in Slovakia (northern Hungary until the twentieth century), and all have developed characteristic historiographical approaches to the Slovak Reformation. Seeking to justify the post-World War I amalgamation of the Czech and Slovak territories, partisans of that cause treated Slovak Protestantism as an offshoot of the earlier Czech Hussite movement. For their part, Slovak autonomists have downplayed the significance of Protestantism in the area in order to deny the existence of that very same relationship that the Czechoslovaks were promoting. Hungarian historians have traditionally viewed Slovak Lutheranism as simply one part of the Reformed Church in Hungary, which was Helvetic in its general orientation. Since the end of World War II, both Czechoslovak and Hungarian writings on the subject have reflected the views of the current regimes and therefore have minimized confessional concerns altogether.

Both in mind and heart David P. Daniel is with those who try to understand the Slovak Reformation as a specifically Slovak phenomenon, although he does not lose sight of works that place the movement within the social, economic, and political context in which it occurred. As a result, he sustains a tone of cool professionalism throughout the essay. The only moment in which his sympathies run away with him occurs when he speaks of the Slovak Lutherans as "the custodians of the Protestant tradition among Czechs and Slovaks" (p. 32). To this reader at least, this distinction is the property of the Czech Brethren, who endured through centuries of persecution never experienced by Slovak followers of the Wittenberg reform.

In general, the materials chosen for comment and

the comments themselves bespeak a scholar thoroughly at home in the field. At only two points are there gaps in Daniel's reading worth mentioning. One appears in his discussion of the Fugger-Thurzó mining ventures in northern Hungary, where one misses any reference to Götz Freiherr von Pölnitz's massive work on the Augsburg banking dynasty. Another is hinted at in the author's confident reference to Rudolf II's "absolutist inclinations." A reading of R. J. W. Evans's *Rudolf II and His World* (1973) might have forced Daniel to judge a bit more cautiously there. These lapses are very minor, however, and in no way do they impair the usefulness of Daniel's work.

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FRANTIŠEK ČERNÝ and LJUBA KLOSOVÁ, editors. *Dějiny českého divadla*. Volume 3, *Činohra, 1848–1918* [History of the Czech Theater. Volume 3, Drama, 1848–1918]. Prague: Academia. 1977. Pp. 657. Kčs. 160.

In this massive, richly illustrated book a team of twelve Czech scholars, headed by co-editor František Černý of Charles University, depicts a living, multifaceted art that inspired individual creativity while serving the cause of national advancement. As the third volume in a major history of Czech theater, this work ranges from the 1848 revolution to the founding of the Czechoslovak state. The theater surveyed is drama (*činohra*), not the musical or opera, which a future volume will handle. Two volumes that covered theater from antiquity to 1848 appeared in 1968–69.

The underlying concept, rather unobtrusively interwoven in the various sections by different hands, is that the Czech stage, while influenced by West European and Russian models, evolved in close conjunction with internal socioeconomic and political developments. This is hardly a startling hypothesis. In mid-nineteenth-century Austria, theater suffered the constraints of private patronage and official censorship. During the constitutional awakening of the 1860s the assertive Czech middle classes and intelligentsia began to underwrite the stage and to review plays in the press. An urban phenomenon, theater flourished in large cities such as Prague and Brno but also had a small-town following. The sons and daughters of the revolutionary generation entered the profession as the actors, scenic designers, and producers who brought ideas to realization. Indeed, the professionalization of the art and the specialization of its skills, significant features of the era, come across graphically in these pages.

After the 1860s theater had to compete increasingly with other forms of entertainment, eventually and most potently cinema; but it always held a special place in Czech hearts, as witness the patriotic campaign to complete the National Theater building in Prague (1868–83). The authors skillfully trace Czech dramatic motifs from their mid-century romantic phase through the realism and naturalism of the 1890s and symbolism and expressionism before World War I. The powerful links between Czech culture and avant-garde Western trends are apparent. A valuable contribution of the work is to show how emigrant Czech actors practiced the native art in such diverse locations as Vienna, St. Petersburg, and Chicago. We are informed that cabaret, puppetry, and chanteuse, modes in which Czechs were second to none, were tinged from their inception with antiestablishment sentiments, as was the “theater of commitment” (*angažovanost divadla*), which appealed to socially critical audiences.

In their conclusion, the authors note some distinctive characteristics of Czech theater: a concrete portrayal of people in realistic circumstances, an effort to reach viewers not through philosophical abstractions but via satire and parody, and the influence exerted on the actual performances by sovereign directors such as Josef Kajetán Tyl and Jaroslav Kvapil.

This book is the result of herculean research and expert synthesis. It assumes that the reader already knows the dramatic plots, the playwrights' careers, and the controversies that animated critics and aestheticians. Those who lack such knowledge would do well to approach the subject first through the more elementary *České umění dramatické*, (2 vols., 1941) or a comparable work. There is no bibliography, but references from Czech and world literature are provided in text and notes, and the two indexes are comprehensive and exact. Over 360 illustrations, many in color, with captions in Czech, English, and German, delight the eye.

This imposing book immerses us in the original and abundant dramatic art shaped by the Czech people in their nation-building era. One awaits with anticipation other volumes in the series.

STANLEY B. WINTERS

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BRUCE M. GARVER. *The Young Czech Party 1874–1901 and the Emergence of a Multi-Party System*. (Yale Historical Publications, Miscellany, number 111.) New Haven: Yale University Press. 1978. Pp. xv, 568. \$20.00.

The study of Czech history in the Dualist era has attracted a great deal of attention in Czech-

oslovakia. Most of the writings, however, have emphasized the remarkable achievements in the cultural and economic spheres. Insofar as they have dealt with Czech political history, political parties, and strivings, they have tended to view them from the perspective of the T. G. Masaryk-led struggle for national independence during the First World War and the establishment of the Czechoslovak Republic. Not much interest has been shown in those political parties, such as the Young Czechs, who guided Czech politics under Dualism, but aimed, virtually until the end, to reform and not to destroy the Habsburg Monarchy. In the West few monographic studies have been devoted to Czech history of this period, and works on the Dual Monarchy have generally viewed Czech politics from the perspective of imperial interests. In any event, these works as well as the general surveys of Czechoslovak history have paid only scant attention to the Czech political system and parties.

Bruce M. Garver's book provides the first comprehensive examination of the Young Czech (National Liberal) Party, a leading force in Czech politics from its founding in 1874 until the First World War. For the most part, the work is organized topically: the first two chapters analyze respectively the national and the Habsburg contexts of Czech politics; the following three are devoted to the founding of the party, its institutional bases in Czech society, and its rise to the leading position in Czech politics by 1891. The study concentrates on the 1890s, and four chapters detail and discuss the predominant party's behavior toward the crises of this critical decade in the history of Austria-Hungary. The last chapter examines the decline and disintegration of the Young Czech party, the rise of successor and rival parties, and, thus, the democratization of Czech politics.

Throughout the discussion Garver stresses the party's importance and its “constructive” role in Czech and Cisleithanian political life. He does not see the Young Czechs as “radical troublemakers,” as they are usually perceived in Western works on the Dual Monarchy, but rather as pragmatic and responsible politicians, as “able political technicians and formidable political polemicists” (p. 318). He emphasizes the party's contribution to the cause of liberalism and anticlericalism and its defense of civil and national liberties. Undoubtedly, the party made a lasting imprint on Czech politics and “its important place in Czech and Cisleithanian history remains secure” (p. 319). Garver leaves one, however, with the impression that he has been a bit too positive and uncritical in his overall evaluation of the legacy of this “elite party of notables.” He touches upon, but does not give equal weight to, the party's many conceptual and tactical shortcomings,

failings, and, in most respects, very limited success, all of which help explain why "few Czechs have ever held the Young Czech party in great affection or esteem" (p. 319).

Although Garver focuses on the Young Czechs, his lengthy work is not merely a history of a party or of a party system. His scope is much wider. He has drawn extensively on the many specialized Czech scholarly works, especially by Marxist historians, and has also produced a very fine survey of Czech social and political development in the second half of the century. All in all, Garver's clearly written book, which is based on an exhaustive investigation of archival and printed primary and secondary sources, makes a major contribution to our understanding of later nineteenth-century politics in the Czech lands and in Cisleithania. Students of both will await with interest Garver's separate work on the period from 1901 to 1914.

ANDREW ROSSOS.
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DAVID W. PAUL. *The Cultural Limits of Revolutionary Politics: Change and Continuity in Socialist Czechoslovakia*. (East European Monographs, number 48.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press. 1979. Pp. x, 361. \$18.50.

Historians read more than they write, but because of their pedantic proclivities their labors often give birth only to pretentious minutiae. Political scientists seem to write a lot more than they read and produce confident but ill-informed generalizations. David W. Paul belongs to that rare species, a political scientist endowed with an abiding sense of history. To judge from the present volume, he reads prodigiously and his generalizations are splendidly informed. He tells the interested reader almost everything he or she ever wanted to know about Czechoslovakia's political culture. The thread that runs through his work is that the history of the Czechs and Slovaks reveals enduring patterns of political culture that even the not-so-gentle Communist masters, lodged in power since 1948, have been unable to obliterate. One of the dominant traits in this culture, according to the author, is political pluralism; the experience of the Dubček interlude of 1968 shows how persistent the pluralist impulses have been.

In academic writing on Czechoslovakia, the Czechs have tended to steal the show: the Slovaks have usually been lumped together with the Czechs without getting their own billing; at best they have appeared as a third-rate appendage to the Czechs. Paul performs something of a tour de force by giving the Slovaks a distinctive profile. In this he has had few precedents in the historical literature on

which to build; because he has to break new ground, some hesitations and ambiguities still remain. In the main, Paul does not quite capture the influence and the dynamism of the Slovak Catholic movement of the 1930s and early 1940s. His observation that the Slovak (Catholic) People's Party remained a "distinct minority" is, strictly speaking, correct but, on close inspection, proves not very helpful. First, that party received approximately 39 percent of Slovak votes in 1929. This may not be a majority, but the figure acquires its proper significance if one realizes that practically nowhere in Europe at this time did a political party win this degree of support from its potential constituency. Even Hitler had to be content with 37 percent of the popular vote at the height of his popularity in Germany in 1932. Moreover, the Slovak People's Party was unquestionably the largest Slovak party, with the next largest party, the Slovak Agrarians, trailing behind with about 26 percent of the vote. This position gave the Slovak People's Party particular dynamism and force that went considerably beyond the numerical support it received at the polls. It was, in fact, very much a party of the political mainstream, and, to this reviewer at least, Paul does not quite confront the implications of this fact. Similarly, he mentions but does not really reflect upon the experience of the independent Slovak state of 1939-45. The Slovak state heightened the national awareness of the Slovak people and in that sense constituted an important stage in the development of Slovak nationalism. That this state was at the same time governed along the lines of Catholic authoritarianism only underscores the difficulty that confronts those wishing to draw up the balance sheet of this experience. How does all this fit into the Slovak political culture?

In the penultimate chapter the author makes an imaginative and original attempt to explore what he calls the "pathos of political non-violence." Few will quarrel with his conclusion that nonviolence has been characteristic of Czech and Slovak history during the last five centuries. One might add that this sets the two peoples apart from most of the nations of Eastern Europe, with the possible exception of the Slovenes.

Whether nations have souls or merely social and political characteristics, Paul's volume will make it possible for us to understand better these aspects of Czech and Slovak history. His work should serve as an invitation to others to attempt such a political profile for the other nations of Eastern Europe.

STANLEY Z. PECH
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HANS HENNING HAHN. *Aussenpolitik in der Emigration: Die Exildiplomatie Adam Jerzy Czartoryskis, 1830-1840*.

(Studien zur Geschichte des Neunzehnten Jahrhunderts, number 10.) Munich: R. Oldenbourg Verlag. 1978. Pp. 316. DM 67.50.

This book is an important addition to the growing literature on the multifarious diplomatic endeavors of Adam Jerzy Czartoryski. Originally prepared as a doctoral dissertation at the University of Cologne, Hans Henning Hahn's study of the Polish prince's diplomacy in the 1830s complements the study published two years earlier by the Warsaw historian Jerzy Skowronek, who treated the subsequent, largely Balkan phase of the activities of the Hôtel Lambert.

Hahn's examination of the first decade of Czartoryski's diplomacy in exile is essentially analytic and topical rather than narrative and chronological. In assessing the aims, programs, and results of the Czartoryski camp, the author also explores the broader issue of the possibilities and limitations of representing a "stateless" nation and the problems of the statesman in exile.

Hahn sees Czartoryski's principal aim in the 1830s as an effort to posit a manifold "polnische Präsenz" (Polish presence)—juridical, diplomatic, and military, no less than national—notwithstanding partition and the setbacks of 1831. Although Western sympathy for Poland's misfortune stopped short of risking war for Poland's restitution, Czartoryski's success in securing British and French support for his interpretation of Russia's violation of the 1815 Vienna accords "furnished the foundation—alternatively the pretext—for the Western powers to meddle regularly in Polish matters whenever it seemed expedient to them" (p. 92).

Czartoryski's hope in the 1830s to establish a unified Polish legion, a military "presence" reminiscent of the one established in the Napoleonic era to further Polish national aspirations, was thwarted not only by the dispersal of the émigrés but also by the desire of several Western states to acquire Polish mercenaries and their reluctance gratuitously to incur St. Petersburg's wrath. More successful, Hahn argues, were the efforts of Czartoryski and his followers to propagandize on behalf of Poland's plight and to foment anti-Russian sentiment in Western public opinion.

Hahn sees the years 1839–40 as a critical turning point in Czartoryski's activities in exile. (The older accounts of Handelman and Kukiel put little emphasis on this caesura.) The renewed Eastern crisis of this period, which led to France's diplomatic isolation and England's rapprochement of sorts with Russia, prompted Czartoryski to reconsider his exclusive dependence on the Western powers, especially Britain. The failure of short-term initiatives or *faits accomplis*, such as the Vixen incident of 1835,

convinced the prince that a long-range policy was necessary. The belief that a European war leading to a reversal of Poland's status was imminent—a tenet of the 1830s—yielded to the realization that renewed independence would require a fundamental realignment of Europe's balance of power. Accordingly, the prince set about establishing an independent diplomatic apparatus with his own agents. A pro-Slav posture and particular attention to the Balkan area characterized Czartoryski's activities in the next decade.

Appended to Hahn's fine study are several previously unpublished memorials by the prince from the Czartoryskis' Library in Cracow, a comprehensive bibliography, a useful summary of the book in Polish, and an unfortunately garbled one in English.

LAWRENCE D. ORTON
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JAN TOMASZ GROSS. *Polish Society under German Occupation: The Generalgouvernement, 1939–1944*. Princeton: Princeton University Press. 1979. Pp. xviii, 343. \$20.00.

It is always encouraging to find a pure "social scientist" concentrating his labors upon a subject normally considered to be the monopoly of historians and publicists. The sociologist Jan Tomasz Gross provides an excellent example of the rich harvest awaiting historian and social scientist alike with his masterful analysis of the German occupation of Poland during World War II. He has produced a sorely needed, objective, and reasonably dispassionate account of an extremely emotional and vital topic in contemporary history.

The author's main thesis is that a society cannot be destroyed forcibly by an alien group, unless that society's population is totally annihilated. After perceptively analyzing the theory and practice of Nazi occupation policy, Gross examines the Polish population's response in terms of alternative collective behavior patterns that developed to replace those prohibited by the Germans. Several interesting and important points emerge from this scrutiny. First, far from being the coldly rational, calculated, monolithic approach popularly ascribed to any feature of the Nazi regime, German occupation policy was vague, hazy, and often undefined, depending for much of its essence on momentary arbitrary decisions by its executors. This lack of a clearly delineated program was both cause and effect of sharp internal rivalries and resultant splits within the occupation leadership, which mirrored the situation within the Nazi movement itself. Second, Gross shows how the Germans managed the Polish econ-

omy so incompetently as to turn a potentially advantageous element of the Nazi war machine into a major burden. Third, there existed a very high degree of corruption throughout all levels of Polish life—corruption that the Germans knew about and, surprisingly, tolerated, thus encouraging it to flourish. Here the Germans made a serious mistake, for corruption soon became one of the most important alternative bonds holding Polish society together after normal integrating forces had been obliterated. The final point worth discussing is probably the most important, especially for scholars accustomed to looking at World War II resistance movements as counteroffensive forces directed against the domineering conqueror-occupier. The author argues that the Polish underground was not primarily an anti-German conspiratorial operation, “but rather a substitute for Polish society in all its functional diversity and plurality of organizational forms” (p. 283). With the traditional social structure in a shambles and normal living impossible, the underground apparatus directed its attention and activities mainly toward the Polish population and only secondarily against the Germans: “It was essentially a norm-creating institution for those who joined it and also for those who remained outside of its network” (p. 256).

The many merits of this book notwithstanding, several shortcomings somewhat hamper its total effectiveness. Despite the enormous amount of research material available in both English and Polish, let alone in German, the source base is quite narrow. Although use of recently published primary and secondary materials on prewar and wartime Poland may not have undermined the author's hypotheses and supporting proofs, their absence does open his findings to serious question. This is especially true of the rich archival and printed sources available in the Polish archives. This in turn may account for some curious omissions in the discussion: the Roman Catholic Church, such a preponderant force in Polish life, is scarcely mentioned; and the large Belorussian populace of eastern Poland is ignored completely in the examination of the relationship between ethnic Poles and the national minorities, despite the fact that the Belorussians played an important part in this facet of the occupation and could have been profiled as readily as the Ukrainians. Finally, the book's organization is awkward and cumbersome. It seems more logical to place the final two chapters, “The Underground as a Social Movement” and “The Underground as a Polity,” earlier in the work, to be followed by specific discussion of key facets of both dimensions of the occupation. This would seem to provide a more coherent, free-flowing account and in turn would enhance the book's usefulness.

But these reservations are mild when measured against the pedagogical value of the book. With occasional minor lapses, Gross has succeeded in avoiding the inane jargon that so many social scientists affect, and the result should be counted among the standard studies of any resistance movement for some time to come.

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V. STANLEY VARDYS and ROMUALD J. MISIUNAS, editors.
The Baltic States in Peace and War, 1917-1945. University Park: Pennsylvania State University Press. 1978. Pp. viii, 240. \$12.50.

This volume of essays, which emerged from the papers presented during the 1973-75 period at several conferences on Baltic studies in the United States and in Europe, is saved from the fate of being a “non-book” mainly by two considerations. The first is its introductory chapter, which successfully places the varied contributions to the history of the Baltic states in general, and of Estonia, Latvia, and Lithuania in particular, within a broader chronological and functional context. Secondly, the four-part chronological grouping of the essays within the established 1917-45 framework is also helpful in this regard. Significantly, there is no general concluding chapter.

Three essays—by Olavi Arens, “The Estonian *Maapäev* during 1917”; by Charles L. Sullivan, “The 1919 German Campaign in the Baltic: The Final Phase”; and by Aba Strazhas, “The Land *Oberost* and Its Place in Germany's *Ostpolitik*”—deal together, as it were, with the aspects of the Baltic road to independence, pointing to the failures of German policy in the East and the concomitant successes of the local Baltic peoples. Two essays—one by V. Stanley Vardys, “The Rise of Authoritarian Rule in the Baltic States”; and the other by Michael Garleff, “Ethnic Minorities in the Estonian and Latvian Parliaments: The Politics of Coalition”—present aspects of Baltic political and constitutional development, pointing to the rather mild characteristics of the regimes established, particularly considering the Hitlerite and Stalinist variants operating at the time in their vicinity.

Four of the essays—by Alexander Dallin, “The Baltic States between Nazi Germany and Soviet Russia”; by David M. Crowe, Jr., “Great Britain and the Baltic States, 1938-1939”; by Julius P. Slavenas, “General Hans von Seeckt and the Baltic Question”; and by Edgar Anderson, “The Baltic Entente: Phantom or Reality?”—analyze aspects of the international position of the Baltic states in Europe between the two world wars, discuss their role

in the calculations of the great powers, and also deal with their own unsuccessful efforts to stay independent. And, finally, four essays—Boris Meissner's "The Baltic Question in World Politics"; Dennis J. Dunn's "The Catholic Church and the Soviet Government in the Baltic States, 1940–1941"; David Kirby's "Morality or Expediency? The Baltic Question in British-Soviet Relations, 1941–1942"; and Romuald J. Misiunas's "Soviet Historiography on World War II and the Baltic States, 1944–1974"—attempt to show the impact of World War II, together with the goals of the Soviet Union in the Baltic area. They deal with the international reactions to the Soviet occupation and incorporation of the Baltic republics in 1940 and with the views and positions of Churchill, Eden, and Roosevelt on this question in 1941–42. The essay by the junior editor gives a useful overview of the kaleidoscopic changes in Soviet presentations of the history of World War II, showing that the old adage, "history is current politics extended backwards," still accurately describes current Soviet practices.

All of the essays are competently written by experienced and well-informed scholars. The contributions are carefully annotated and documented from Western archival and memoir sources. They include only a sampling of Soviet documentation, however, since Soviet ideological and political sensitivity still does not permit free access to Western scholars to their extensive archival holdings.

The collection of essays, capably edited by Vardys and Misiunas, can be fruitfully used to fill in the existing gaps in our knowledge about the Baltic area. It can also serve as a useful guide to further research. The essays themselves, individually and in toto, present an effective counterbalance to the standard, heavily ideological, and politically colored Soviet interpretations of recent historical developments concerning the Baltic states.

BENEDICT V. MACIUIKA
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E. A. RYBINA. *Arkheologicheskie ocherki istorii novgorodskoi trgovli X–XIV vv.* [Archeological Essays on the History of Novgorod Trade, Tenth to Fourteenth Centuries]. Moscow: Izdatel'stvo Moskovskogo universiteta. 1978. Pp. 164. 95 k.

The long and detailed excavations of medieval Novgorod have provided historians with some of the most interesting information on life in Rus'. E. A. Rybina, a student of V. L. Iarin, has concentrated on the application of the excavations' finds to questions of Novgorod's import trade, and four previously published essays are reprinted here. While the essays are only vaguely related and of uneven

quality, they provide an intriguing glimpse into a part of Novgorod's history that written sources do not treat.

The heart of the book consists of two essays treating southern and western imports. By plotting the occurrence of import wares like walnuts, slate beads, amber, and ceramics in Novgorod's dated layers, Rybina establishes chronological frequency patterns suggestive of trade patterns. Southern imports, which either originated in or passed through Kiev, predominated in the tenth, eleventh, and twelfth centuries, after which their numbers decline steeply. Western imports, by contrast, demonstrate their strongest frequency in the period after the thirteenth century. Rybina explains this correlation by reference to the Mongol destruction of Kiev and the southern trade route in the thirteenth century. Similar interruptions characterize the western and eastern trade, and Rybina attributes these stoppages to the Teutonic Knights and Polovtsy respectively. While perfectly plausible, these explanations are offered as proved, something which Rybina's evidence cannot do.

Two large essays complete the book. The first, the book's weakest, analyzes the birch bark documents for information on trade. To Rybina's credit, she observes early on that some of the most fantastic explanations proposed by L. V. Cherepnin (*Novgorodskie berestianye gramoty kak istoricheskii istochnik* [1969]) must be dismissed. Nevertheless, Rybina herself is reduced to citing documents that mention a series of fish or the purchase of two horses as trade documents.

The final essay is devoted to the historical topography of Novgorod. Rybina studies excavations of the headquarters of the most influential of Novgorod's trading partners—Gotland and the German towns. As Rybina notes, these excavations unfortunately were not completed due to modern construction, but the locations of the Europeans' trade and residence centers are now firmly established.

Most of the book is solid, but several judgments require further qualification. For example, Rybina indicates that the name by which the Novgorodians called the Gotland islanders was *Variagi*. The evidence is slim, and in any case requires consideration of other citations. Also dubious is the contention that a Kievan blockade was responsible for the temporary decline in southern imports at the turn of the twelfth century. Even if there was such a blockade (Rybina cites only thirteenth-century references), it is doubtful that the blockade could have been policed effectively.

Still, the book is stimulating. Rybina's most important conclusions are summarized at the book's end, and a brief bibliography is appended.

DANIEL H. KAISER
Grinnell College

RUSSELL ZGUTA. *Russian Minstrels: A History of the Skomorokhi*. Philadelphia: University of Pennsylvania Press. 1978. Pp. xv, 160. \$16.00.

The *skomorokhi*, or minstrels, have long been recognized as an integral part of medieval Russian culture, but, as Russell Zguta rightly notes, they have, with few exceptions, been just as long neglected by historians. Any history of these enigmatic minstrels must of necessity include much conjecture, but Zguta's conclusions on their role in Kievan pagan ritual and as conduits of Kievan *byliny* to Novgorod and Muscovy are good working hypotheses. It is to Zguta's credit that what information there is has been brought together in this compact history.

Zguta traces the history of the *skomorokhi* from Kievan Rus' to their decline in the seventeenth and eighteenth centuries. Zguta believes that the conversion of Kievan Rus' to Christianity forced the transformation of the *skomorokhi* from rural pagan priests to minstrel-entertainers. His basic assumption is that they were indigenous to Russian paganism and not the offshoot of Byzantine mime or the German *Spielmänner*, although the former contributed acrobatics and the latter the distinctive dress of Russian minstrels. As pagan priests, Zguta ties the *skomorokhi* to the occult arts of witchcraft, marriage ceremony, and the pagan festivals of the *Rusaliia* and *Koliada*. Yet Zguta draws no connection between the *skomorokhi* and the *volkhvy*, who are credited with the pagan reaction of the eleventh century and are described as pagans or as pagans in the guise of Manichaeism or Bogomilism. He repeats Vernadsky's conclusion that the *volkhvy* were an urban and upper-class phenomenon, but a sharp distinction between urban and rural paganism, particularly in the pre-Christian era, is doubtful. This raises another serious problem in that the *skomorokhi* are often described as representative of residual paganism and secular entertainment. The degree to which they are either pagan or Christian in medieval Russia is not made clear, and if they continued the pagan tradition, what does secular culture mean in this context?

According to Zguta, the political fragmentation of Kiev in the twelfth century and the Mongol invasions drove the *skomorokhi* north toward Vladimir-Suzdal and Novgorod. Condemned in Muscovy by Maksim Grek, Metropolitan Daniil, and the *Stoglav* in the sixteenth century, the *skomorokhi* flourished in Novgorod, and the reasons for this Zguta ascribes to Novgorod's traditional liberalism and democracy. But this explains little. What is needed is an analysis of the complex historical role of Russian Orthodoxy in Novgorod and Muscovy.

Zguta traces the impact of Ivan IV's devastation of Novgorod on the *skomorokhi* and their dispersal throughout Muscovy. He makes an interesting

analysis of the unregistered *skomorokhi*, who were seen by Muscovite authorities as lawless, and the registered, who ran the gamut from Ivan's personal entertainers to the *strel'tsy* and *bobyli*. The *skomorokhi* were proscribed in December 1648 as part of the spiritual reform, Aleksei's own piety, and perhaps also the urban riots. But Aleksei's motives are not fully explained, for, on the one hand, Zguta describes him as pious and, on the other, as a supporter of secularization or Westernization.

The last two chapters show the minstrels' impact upon the *byliny*, music, dance, and theater (especially puppetry). Zguta's work is a welcome addition to the growing literature of medieval Russia.

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DANIEL CLARKE WAUGH. *The Great Turkes Defiance: On the History of the Apocryphal Correspondence of the Ottoman Sultan in Its Muscovite and Russian Variants*. Foreword by DMITRII SERGEEVICH LIKHACHEV. Columbus, Ohio: Slavica Publishers. 1978. Pp. ix, 354. \$18.95.

Through his publications in both Western and Soviet journals and his participation in scholarly conferences, Daniel Clarke Waugh of the University of Washington at Seattle has already acquired an international reputation as a specialist in the study of Muscovite manuscripts and literary monuments. Further testimony to his recognized expertise as a textologist and codicologist is provided by the laudatory foreword to this, his first book, by Academician D. S. Likhachev (pp. 1-4). In this highly technical monograph Waugh analyzes one genre of Muscovite *turcica* (literary works concerned with the Ottoman Turks), apocryphal letters of the sultan. By investigating their origin and manuscript tradition Waugh locates these texts within the context of Muscovite literary culture of the seventeenth century. On the basis of the most exacting manuscript analysis, extracted from Soviet (and non-Soviet) archival repositories by prodigious labor, Waugh proves conclusively that all the Muscovite variants are translations from European prototypes, despite some assertions to the contrary. Moreover, scrutiny of the manuscript tradition suggests that as in Europe the texts reappear in patterns that conform to political events in Muscovite and Ottoman history, most notably wars. In his brief conclusion (pp. 187-98) Waugh identifies some implications of his study of the apocryphal correspondence of the sultan for our understanding of Muscovite culture.

Technically and methodologically this monograph is simply a tour de force—parallel texts in several languages, genealogical stemma, charts, graphs, and tables are ubiquitous. The body of the work (pp. 5-198) is followed by the "Apparatus

Criticus": sample texts of genuine and apocryphal letters (pp. 200–21); manuscript descriptions (pp. 222–77), at which Waugh is a master; notes (pp. 278–318); and a selected bibliography, index of manuscripts cited, and index. Waugh is quite modest in presenting the results of his research, honestly willing to revise his own earlier views on specific questions (p. 295, n. 86), frank in admitting gaps in his own manuscript notes (p. 266) and in the manuscript evidence, and insistent on the tentative nature of his conclusions pending definitive additional work. The impartial reader will find it difficult not to be overwhelmed by Waugh's evidence and thoroughly convinced by his arguments. This is a valuable contribution to our knowledge of seventeenth-century Muscovite culture.

Probably only specialists will want or need to follow Waugh's investigations step by step. The general reader can, however, fruitfully turn to the conclusions of each part and the overall conclusion for fully accessible discussions of the results of the more technical analysis.

Waugh eschews more extended interpretation of the significance of the apocryphal correspondence with the sultan until all genres of *turcica* can also be taken into account (pp. 11–12), for each of which the same painstaking and meticulous spade work will be required. One is confident that Waugh will fulfill this need with the same skill and care he has lavished on the apocryphal correspondence. In the meantime Waugh whets our appetites for his future research: by raising a host of wider questions about Muscovite culture: the degree to which it was integrated into European culture in the seventeenth century; the processes by which literature entered Muscovy from the West and was then translated and circulated; the differing roles of *turcica* in European and Muscovite culture; the relationship of this translated literature to the development of original "documentary *belles-lettres*" in Muscovy; and the connections between the importation and dissemination of *turcica* and the cultural transformation of Muscovy in the seventeenth century that culminated in the reign of Peter the Great.

As an extra bonus to the reader, the volume contains forty pages of illustrations of European and Muscovite texts (pp. 98–137, list pp. 335–36).

CHARLES J. HALPERIN
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IA. E. VODARSKII. *Naselenie Rossii v kontse XVII-nachale XVIII veka (Chislennost', soslovno-klassovyi sostav, razmeshchenie)* [The Population of Russia in the Late Seventeenth and Early Eighteenth Centuries (Quantity, Class Composition, Distribution)]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 262. 2 r. 10 k.

This important book provides data about and tentative answers to fundamental questions about the size, structure, and movement of the Russian population. It briefly analyzes the historiography and character of the problems encountered in working with census and other demographic sources in the Russian context and describes the variety of sources themselves. Ia. E. Vodarskii has filled this text with tables that summarize his findings, not always with the greatest clarity or success. There are maps that define regions and *uezdy* of Russia, a particularly difficult and important accomplishment, and extensive appendixes of data on population in 1678–1719. The administrative map showing boundaries of *uezdy*, *stany*, and *volosti* is a major contribution to our knowledge of Russia in the period. Vodarskii notes that it is the fruit of years of painstaking work. Certainly, it is on the empirical level that his work makes its most important mark, rather than in any new theoretical framework or insight.

This work presents no great surprises in its periodization or its classification of Russia's population. Instead, this is a compilation of Vodarskii's earlier work and a correlation with the analyses of other historical demographers, geographers, and historians. The discussion of the urban population of Russia illustrates the strengths and a few of the weaknesses of this book.

Vodarskii includes an important discussion of the definition of a Russian town, following the "classical Marxist criteria." He argues that Russia's towns fit this model as well as the European towns that initially provided the material for the model. His criteria for a town become rather narrow and exclusive, and, although he puts Russia into a European context, it is at some cost to the understanding of Russian urban development in this period. Towns without a trade and artisan population are found not really worthy of inclusion as part of the total urban population of Russia. The author does not completely ignore other groups of the urban population, but tends to emphasize the townsmen (*posadskie liudi*). Thus, he notes that information on other social groups in the towns is scanty and incomplete but does little to estimate important non-townsmen populations. The result of this is a systematic underestimation of Russia's urban population in the late seventeenth and early eighteenth centuries. His summary table (p. 134) presents an urban male population figure of 2 percent. In fact, the data he presents indicate the level at 4 percent even without a closer estimate of all groups in Russia's towns. The figure may be as high as 6, not 2, percent if all residents are included rather than relying primarily on the juridically urban population.

The discussion of migration is another area of important new data in this work. Vodarskii's figures

clearly show movement away from the non-Black Earth center, the northwest, and north and identify individual areas of migration within various regions. He emphasizes a more "natural" movement, without state impetus, and notes the establishment of new settlements in the south and southeast considerably before the state recognized and fortified these locations. The state's role is greatly de-emphasized in this standard Soviet interpretation.

This book suffers from the complexity of its structure and a lack of clarity in the presentation of its massive and important data. But this is a major source for all historians whose work touches any aspects of population, migration, or urban development.

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A. P. PRONSHTAIN, editor. *Don i stepnoe Predkavkaz'e, XVIII-pervaia polovina XIX v.: Zaselenie i khoziaistvo* [The Don and the Steppe of Ciscaucasia, Eighteenth and First Half of the Nineteenth Centuries: Settlement and Economy]. Rostov n/D: Izdatel'stvo Rostovskogo universiteta. 1977. Pp. 239. 2 r. 44 k.

This volume, the first of a projected two-part study, is a collection of short essays by historians from Rostov State University under the direction of A. P. Pronshtain. The geography of the Don basin and North Caucasian steppe (Ciscaucasia) receives special attention, as does the specific effect of colonization on the area's socioeconomic development. The essays are organized under four headings: colonization, agriculture, manufacturing, and trade. Since the same scholars usually deal with a specific region in each of the subdivisions, the final result has more continuity than the ordinary collective undertaking (*sbornik*) authored by Soviet historians.

Colonization was the final stage in the process of gradual annexation that the imperial regime had pursued in expanding to the Black Sea and Sea of Azov in the eighteenth century. With the growth of fortified strong points along the southeast frontier by the century's end, raids by the Turks and Nogai Tatars decreased, enabling the government to pursue a consistent policy of settling the area. One of the main contentions found in this study is that resettlement was a means of alleviating social pressure in Central Russia and the Ukraine, where a growing population pressed on available land and food. In the first decades of the nineteenth century, serfs belonging to the state were encouraged to migrate. Nobles who possessed holdings in the Don and Ciscaucasia were also permitted to transfer their proprietary serfs from other parts of Russia.

It is questionable whether these efforts produced

an adequate flow of settlers. Joining the "legal" arrivals were fugitive serfs, largely from those provinces close to the frontier. Although official policy was to prevent these refugees from registering as members of the Cossacks and thereby gaining permanent sanctuary, their numbers had grown so by the 1820s that local officials paid little attention to the letter of the law and allowed many to stay. Efforts were also made to encourage non-Russian colonists to migrate to the Caucasus through promises of good land and tax benefits.

The eventual impact of this population movement can be seen in the essays describing economic development. Statistical evidence gathered by the authors demonstrates the emergence of a settled pattern of agriculture and trade. Here again the government played a key role, pacifying the frontier so that permanent cultivation could develop and later fostering the growth of trade with the Russian heartland. Although the contributors emphasize the low technical level of agriculture in the region, the fertility of the soil and mild climate enabled production to climb steadily. So-called manufacturing (largely cottage industries) exhibited a more modest increase, apparently causing the authors to devote much of their attention to the mid-century, when the beginnings of capitalist "cooperation" can be detected.

This work does make a contribution as an introduction to the history of a region accorded little space in general accounts. Its utility as a reference work, however, would have been enhanced had more detailed maps been provided. The only maps, found in the end paper, are of little help in locating the specific regions often referred to in the text or the regions populated by various ethnic groups. The aggregate result of colonization and economic growth would also be clearer were the statistics combined in general tables, either at the end of the chapters or the volume itself.

ROBERT D. GIVENS
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V. V. POZNANSKII. *Ocherk formirovaniia russkoi natsional'noi kul'tury: Pervaia polovina XIX veka* [An Essay on the Formation of Russian National Culture: The First Half of the Nineteenth Century]. Moscow: Izdatel'stvo "Mysl'." 1975. Pp. 221. 1 r. 13 k.

This book is divided into four overlapping sections. In the first, V. V. Poznanskii summarizes "Russian culture" at the turn of the nineteenth century and places it in the context of the Russian class and economic structure. The second chapter focuses on the Napoleonic Wars in general and the War of 1812 and its consequences for Russian patriotism in particular. He sketches the liberal ideas held by the pa-

triotic intelligentsia, the self-interested nature of aristocratic nationalism, and the "freedom-loving" national spirit expressed by the "people." In chapter three, Poznanskii demonstrates the origins and implications of Official Nationality; and the final part, which concentrates on the late 1830s, is held together by constant reference to the Slavophile-Westernizer debate. The Decembrist movement is the book's most consistent focal point.

Intended as a "necessarily brief and schematic" overview of Russian culture during the first half of the century, this book reads like a long and only slightly analytical essay. Non-Marxist scholars are likely to be put off, for Poznanskii insists as a preliminary qualification that the study of the formation of a national culture in Russia "is possible only on the basis of a Marxist understanding of the structure of the social conscience" (p. 8). That warning may explain why this reader is still puzzling over the purpose of the book!

Filled with an inordinate number of quotations from Lenin and the nineteenth-century socialists Herzen, Belinskii, and Chernyshevskii—and with many other seemingly random extracts from individuals who contributed in various ways to the creation and interpretation of Russian culture—the work has no central tale. Its eclectic nature can be illustrated by the fact that Poznanskii pays little attention to the forums in which his personae expressed their ideas; that is, he cites from such journals as *Biblioteka dlia chteniia* (1834–65), *Severnaia pchela* (1825–64), *Vestnik evropy* (1802–30), and *Syn Otechestva* (1812–30) but ignores the overall role of these and other journals as vehicles of national feeling. All individual contributors to the evolution of a Russian national consciousness are treated favorably, but those who added the liberty of mankind to their Russian nationalism are the greatest heroes of all. The fact that Russian nationalism often contradicted the principle of freedom and independence—even among the Decembrists in regard to Poland—is only barely mentioned.

Poznanskii pays little heed to important movements that were international in character: pietist movements, the Russian Bible Society, Freemasonry, and so on. The fact that for the first twenty years of the century Russian culture was intricately involved in continent-wide trends—and that even under Nicholas I, the influence of German philosophy on the minds of young Russians was overwhelming—are realities that he sets aside. Granted, the author is studying the growth of an indigenous culture, but he cannot do it successfully in a vacuum.

There are bits and pieces of interesting reading here, but once on to something worthwhile, Poznanskii tends to shift to another subject. This is es-

pecially the case when he touches upon education—its organization, purpose, and content. The attempts of a variety of ministers of public instruction to shape the thinking of young Russians through state-directed programs are dealt with in far too cursory a manner. His remarks about the arts, theater, and music (with the exception of material on M. I. Glinka) seem to be thrown in almost incidentally. To add to the reader's frustration, the absence of an index makes it very difficult to trace an idea—or personal contribution—throughout the book.

The subject matter chosen by Poznanskii warrants thorough study; unfortunately, this book falls considerably short of the mark.

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M. N. PEUNOVA. *Etika N. V. Shelgunova* [The Ethics of N. V. Shelgunov]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 215. 95 k.

N. V. Shelgunov was among the several dozen most widely read Russian radical journalists of the 1860s. An admirer of Belinskii and Chernyshevskii, Shelgunov wrote his most interesting works between 1860 and 1868, suffered imprisonment and exile for his views, and continued his journalistic career sporadically, when so permitted, until his death in 1891. The term "ethics" as used in this title refers strictly to the humanitarian premises that dominated his thinking.

Unfortunately, Shelgunov was not an original thinker, and what strikes a reader familiar with this field is the amazing similarity of his views to those of other Russian radicals of his generation. Like them, Shelgunov agonized over the individual-social dilemma of utilitarianism and muddled around in the dichotomy between socioeconomic determinism and Lavrov's critically thinking individual, between the richly developed personality and the plodding masses. Shelgunov's views on pedagogy are interesting but scarcely unique, and even in his attacks on Tolstoy (as *Narodnik*, individualist, and moralist) he mirrors contemporary radical opinion. Perhaps his greatest achievement is an interesting analysis of public opinion and its effect on individual behavior and thinking.

M. N. Peunova's major efforts are devoted to pointing up similarities between Shelgunov's thinking and that of the Marxists. In this context her subject presents fertile soil, since he adopted his generation's tendency toward materialism and environmental determinism, sticking to his guns even when others abandoned theirs. Peunova's foregone conclusion is that his ideas "prepared the soil" for

Marxism (p. 4) but that he remained hidebound by his times and by his latent commitment to what she terms "idealism."

The book is filled with irritating assessments of Shelgunov from the Marxist viewpoint and continually labels attitudes as "correct" or "incorrect" in accordance with their approach to "scientific" analysis. The chronological development of Shelgunov's thought is not clear, and thus the author's important assertion that he became more, not less, "materialistic" in later years remains unsubstantiated. Contemporary Western thinkers are seldom given the credit they deserve for their influence on the Russian scene. In her efforts to read Marxism into her subject, Peunova is quick to deduce socialism from Shelgunov's statements, but from the proofs adduced here, one cannot really tell if she is right. There are not enough footnotes, there is no index, and a bibliography of Shelgunov's works is not attached.

Peunova's most interesting contribution lies in her analysis of the conflicting ethical premises of utilitarianism (here called "rational egotism") and socioeconomic determinism: that is, of an ethical system centered around man as unique biological being and one coldly conditioned by the immutable laws of historical development. We are back to individual versus society, freedom versus determinism, personality versus sociology. Greater men than Shelgunov have tried to reconcile these concepts and failed.

This is a useful book for anyone seeking the views of a typical "revolutionary democrat," but its conclusions must be accepted with caution.

DEBORAH HARDY
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EDWARD ACTON. *Alexander Herzen and the Role of the Intellectual Revolutionary*. New York: Cambridge University Press. 1979. Pp. x, 194. \$18.95.

A. I. VOLODIN *et al.* *Chernyshevskii ili Nechaev? O podlinnoi i mnimoi revoliutsionnosti v osvoboditel'nom dvizhenii Rossii 50-60-x godov XIX veka* [Chernyshevskii or Nechaev? On the Genuine and the Sham Revolutionary Character in the Liberation Movement of Russia of the 1860s and 1870s]. Moscow: Izdatel'stvo "Mysl'." 1976. Pp. 294. 1 r. 18 k.

In our age of revolution, the flow of historical studies of radical intellectuals seems assured. We join the past and the present in a search for answers to the ideological, tactical, and moral questions associated with revolutionary change. The two books under review, concentrating on related aspects of the Russian revolutionary movement, speak to these

questions but from contrasting assumptions and conceptions about the study of the past.

Edward Acton offers an intellectual biography of Alexander Herzen, a man whose life and thought were inextricably intertwined. Tracing the impact of private tragedy and disappointing public events on Herzen's thought and activity from 1847 to 1863, Acton explains how one outstanding individual worked and reworked his own ideas and aspirations and adjusted his actions accordingly. Herzen's effect on society, or on the larger historical process, is not denied, but such is really not Acton's concern. It is the biography that matters most.

Within the limits of this approach, Acton performs admirably. He is clearly at home in the materials on Herzen's career, and he writes in a concise, attractive style. The picture he draws of a life marred by personal loss and frustration is sensitive and sympathetic. In turn, he builds a strong case for his version of the development of Herzen's thought. Taking issue with Martin Malia's views, Acton denies Herzen's lifelong messianic and revolutionary nationalism and stresses the impact of family tragedy on his thought. Indeed, the compounded family misfortunes of 1852 are seen as central in undercutting the personal optimism that sustained Herzen's belief in a predestined socialist regeneration and the guaranteed triumph of the rational over the irrational in the historical process. Once Herzen had reached a more realistic and pragmatic position, by the mid 1850s, he was free to question whether a peasant rebellion, however morally justified, would in fact be creative. From this position, he found a new role for himself after the Crimean War, encouraging reform from above and acting as a brake on younger radicals who called all too quickly for a peasant rebellion. Only belatedly and reluctantly, after the collapse of his reform hopes, did he join the radicals, but by then it was too late to be an effective leader.

One might object to the narrow focus of Acton's study, which leads him to underplay some important questions. Herzen's intellectual relationship to Nicholas Ogarev, for example, or his actual participation in attempts to form an underground organization, might both have been given further treatment. Yet that same narrow focus also adds to the persuasiveness of his version of Herzen's intellectual pilgrimage.

Chernyshevskii ili Nechaev?, by A. I. Volodin, Iu. F. Kariakin, and E. G. Plimak, offers a totally different kind of concern with the past. The problem posed by the book is to distinguish between "genuine" and "sham" forms of revolutionary thought and action in the Russian movement of the 1850s and 1860s. The study is historical insofar as it examines the careers of several revolutionaries of the pe-

riod, but the book's main thrust is present minded and ideological. The authors seek to define proper revolutionary action, as opposed to its terrorist or anarchist perversions, and to establish beyond question the connection of their own Marxist-Leninist tradition with the brighter rather than darker side of the revolutionary movement. Thus, the past is approached for lessons and for validation of a present concern.

Not unexpectedly, N. G. Chernyshevskii, to whom almost three-quarters of the book is devoted, ranks highest in the utopian, pre-Marxist tradition. His writings reflect humane concern and the highest morality and, whereas he accepted the need for forceful revolutionary action, he did so with appropriate restraint, a realistic sense of timing, and a sophisticated awareness of probable outcomes. In contrast, the authors' briefer treatment of D. V. Karakozov and S. G. Nechaev serve as major examples of what should *not* be done. Counter-productive acts of terror, impatience leading to inappropriate timing, and deceitful immorality in the name of revolution, all come under the authors' severe criticism. For those nineteenth-century conservatives who lumped the genuine and sham revolutionaries together or for "bourgeois" historians who suggest some reflection of the sham tradition in Leninism, their criticism is no less severe.

Western specialists will have most interest in the chapters defining Chernyshevskii's position. In one of these a close textual analysis is given of his translation of F. K. Schlosser's histories of the eighteenth and early nineteenth centuries for clues on Chernyshevskii's own views of revolution. Another chapter suggests a second level of meaning to his novel *What Is To Be Done?* Both arguments test one's credulity but deserve serious consideration. So too, attention should be given to the authors' description of revolutionary action in the early 1860s. Building on the best of Soviet scholarship, they present a realistic picture that suggests a greater similarity between Soviet and non-Soviet interpretations than appeared a decade ago.

WILLIAM F. WOEHLIN
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SEPPO ZETTERBERG. *Die Liga der Fremdvölker Russlands, 1916-1918: Ein Beitrag zu Deutschlands antirussischem Propagandakrieg unter den Fremdvölkern Russlands im Ersten Weltkrieg.* (Studia Historica, number 8.) Helsinki: Suomen Historiallinen Seura. 1978. Pp. 279.

By now it has become well known among historians of the Russian Revolution that during World War I the German government, in addition to its military efforts on the front, also mounted a sizable uncon-

ventional war—let us call it a propaganda campaign against the Russian imperial government. The sensational findings of the late 1950s about German money flowing into the Bolshevik treasury now are considered established facts by most historians. But the Bolsheviks were not the only recipients of German largess. The representatives and parties of Russia's nationalities, disgruntled with the Romanov Empire, were equally an object of the German Foreign Office's attentions. One German approach to the nationalities receives a thorough documentation in the work under review.

That the German government would attempt to befriend Russia's nationalities, according to Seppo Zetterberg, was not surprising at all, for the nationality fissures of the empire had been studied by most important foreign offices of Europe long before the war. When the war broke out, the German strategists did not need to invent a new policy but only to develop tactics to put the existing one into effect. Although the nationality fissures potentially were more corrosive to the structure of the empire than the Bolsheviks, the German efforts to exploit them did not have the same success and significance.

At the outbreak of the war numerous exiles from the Caucasus, the Ukraine, Poland, Belorussia, and the Baltic Provinces, who were residing in Germany, neutral Sweden, Switzerland, and Denmark, desired to have their countries liberated from the stranglehold of the Russian bear. The goals of the two groups—the German strategists and the exiles—were identical, and therefore the ensuing cooperation was natural. The outcome of this cooperation was the League of Russia's Nationalities. Although much of the exiled nationalists' enthusiasm went into the efforts of the league, the author emphasizes that the main organizational, not to mention financial, support came from the Germans. The Germans' problem was how to get the league to pursue pro-German policies without acquiring the image of a German propaganda agency.

The high point of the league was the Nationality Conference of June 1916 in Lausanne. The chief weakness of the league was that its nationality representatives, though very able leaders, only represented themselves and had no parties or large following in their home countries. The February Revolution in Russia changed the status of the league: it was its death knell. In a way the league had outreached itself—its demand for national independence and the dismemberment of the Russian Empire corresponded more to Germany's war aims than to the programs promulgated by the nationalists and their parties within the empire itself. After the February Revolution, Russia's nationalities demanded autonomy in a federative state rather than

separation from Russia, which the league had demanded.

The author has done very thorough and authoritative work in unraveling the skeins of the league's existence. His book fills in an important gap in our understanding of Russia's revolution and Germany's policies during World War I.

ANDREW EZERGAILIS
Ithaca College

T. F. KUZ'MINA. *Revolutsionnoe dvizhenie soldatskikh mass Tsentra Rossii nakanune Oktiabria: Po materialam Moskovskogo voennogo okruga* [The Revolutionary Movement of the Soldier Masses of the Center of Russia on the Eve of October: Based on Materials of the Moscow Military District]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 309. 2 r. 70 k.

Although Soviet scholarship has always devoted a good bit of attention to Bolshevik penetration of the army in 1917, the quality of these studies has not, in general, been very high (exceptions are the works of V. I. Miller, M. S. Frenkin, and E. N. Burdzhakov). Although they invariably use archival materials inaccessible to foreign scholars, only tantalizing snippets serve to illustrate the well-established canons of Soviet historiography. (Even published sources, particularly of the rich decade of the 1920s, are underused.) Second, although the entire front from Finland to the Caucasus has been covered, the garrisons of the interior have remained virtually untouched, a serious omission, since Bolshevik influence originated in the turbulent, politicized garrisons and reached front units via replacements, returning wounded, and NCO and officer trainees. With the recent work of A. M. Andreev, the present study goes a long way toward redressing the imbalance; but even more rewarding is the new standard in the exploitation of archival materials. T. F. Kuz'mina's study makes thorough use of the archives of the Moscow Military District, which embraced all of the central provinces from Tver to Kharkov, as well as of the mobilization section of the general staff. The Moscow District is crucial in that it consisted exclusively of the agricultural provinces of central Russia, allowing one to observe the social process in its most undiluted form. The mobilization data for the first time afford a concrete picture of the mode of sending replacements to the front: the author offers not only a complete list of reserve regiments, locations, size of garrisons, and number of replacements by month and origin, but even the front units that they supplied, making possible a correlation of radicalized units front and rear.

Although one could write at length on the virtues

of this study, its major contributions must suffice. First, it makes clear that the July crisis was part of a nationwide process of radicalization directly connected to the accelerated sending of replacement units to the front in connection with the June offensive (Alexander Rabinowitch has confirmed this for Petrograd, Kuz'mina for the entire central region). The turbulent elements most susceptible to instant bolshevization were the recovering wounded (roughly 30 percent of many units) and soldiers over forty, who had been released for field work and recalled. Second, unlike Petrograd, the major shift of garrison troops to the Bolsheviks took place *after* the July crisis, because the Bolsheviks were able to capitalize on the attempts of authorities, with the cooperation of local soviets, to restore military discipline, officer authority, and regular training duties. Rabinowitch has already modified the conventional picture that Bolshevik fortunes were at a low ebb until the Kornilov days, but for the garrisons of the central region, the scheme does not apply at all. Until July, most soldier soviets were under the secure control of SRs, but, as a direct result of unrest and punitive expeditions against recalcitrant reserve regiments who did not wish to be shipped to the front, the soldiers went over en masse to the Bolsheviks.

The study also offers a comprehensive overview of the impact of the Kornilov affair, demonstrating that the galvanizing of soviet-oriented elements into various revolutionary defense committees, in which local Bolsheviks invariably played a key role, was a universal process, not simply of the major urban centers (Kuz'mina identifies eighty-one such committees, twice the number previously established by Soviet investigations). More than any previous study, this one confirms the secure hold the Bolsheviks attained by October on the key soviet constituencies, which were easily translated into local transfers of leadership and a near majority at the Second Congress of Soviets, giving firm underpinning to the proclamation of soviet power. The study fails on two important questions, however: it ignores entirely the process of "self-demobilization" of the garrisons (which was surely reflected in the reports Kuz'mina otherwise extensively cites); and second, though the slogan "soviet power" is given its due, the Second Congress of Soviets is not mentioned once, though surely the question of representation at the congress was a major issue exploited by local Bolsheviks to force by-elections and overturn Menshevik-SR majorities in local soviet executive committees. In fact, the study stops short of dealing with the ramifications of the October Revolution, particularly of local transfers of power in which the garrisons certainly played a role. Other themes are neglected or overschematized to suit official his-

toriography, but on the whole this is one of the better Soviet monographs on 1917.

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RICHARD K. DEBO. *Revolution and Survival: The Foreign Policy of Soviet Russia, 1917-18*. Toronto: University of Toronto Press. 1979. Pp. xiii, 462. \$25.00.

This well-appointed volume, including the luxury of footnotes at the bottom of the page, ventures upon historical ground plowed over by numerous predecessors. It is a conscientious study of Soviet external affairs in the year following the Bolshevik Revolution (the subtitle is somewhat presumptuous since the emphasis, if judged solely by a word count, is upon diplomacy). E. H. Carr, George F. Kennan, John W. Wheeler-Bennett, and a host of less prominent historians pioneered the way, and Richard K. Debo has graciously conceded as much in the introduction. With such a profusion of talent having pre-empted the field, it is legitimate to inquire whether there is room for yet another volume, especially one that recapitulates the familiar story of the Brest-Litovsk negotiations and the genesis of Allied intervention. Anticipating the question, the author provides a forthright answer by maintaining that his precursors wrote at a time when relatively few primary sources were available and that, among other material, he has utilized the archival depositories of Washington, London, Paris, and Bonn. This new documentation has enabled him "to focus on the development of Soviet foreign policy itself rather than viewing it simply as a foil against which other governments pursued their interests in Russia" (p. xii). His objective is laudable, if not always attainable. At any rate, his diligence has succeeded in illuminating formerly obscure nuances of Soviet policy; and when his own research becomes redundant he does not hesitate to defer to the appropriate secondary authorities. But his implication that the time is ripe for a fresh appraisal of Soviet foreign policy is flawed: the Kremlin archives remain closed to independent scholarship, and the publication of Soviet documentary collections has furnished additional details without gratifying our desire for "inside" information emanating from the highest level, that is, Lenin and the top echelon of his party colleagues.

Lenin emerges, unsurprisingly, as the *deus ex machina* of Bolshevik foreign policy. There is a tendency to inflate his skill—not that the interpretation is uncritical—and he emerges as a consummate diplomatist, fully matching his stature as a political tactician. Even under inept leadership, however, Russia could hardly have failed to benefit

from the military confrontation of the Allies and the Central Powers. One could have wished for a more human portrayal of the leading protagonist—indeed the dramatis personae seldom appear as more than fleshless symbols of government policy—but perhaps this is the province of biographers and popular writers. The work makes few concessions to the general reader despite clear and vigorous prose occasionally spiced with a pungent phrase or an apt metaphor. The narrative, while demanding no arcane knowledge, is closely packed and presupposes some familiarity with the revolution and civil war. A map or two would have helped. But professional historians, certainly those who write monographs published by university presses, need not lust after the broad reading public. *Survival and Revolution* is a provocative and in most respects highly satisfactory achievement. Specialists will consult it with profit and perhaps with moments of pleasure.

ROBERT D. WARTH
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V. P. DANILOV. *Sovetskaia dokolkhoznaia derevnia: Naselenie, zemlepol'zovanie, khoziaistvo* [The Pre-Collective Soviet Village: Population, Land Tenure, Household]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 317. 1 r. 80 k.

N. K. FIGUROVSKAIA. *Agrarnye problemy v sovetiskoi ekonomicheskoi literature 20-kh godov* [Agrarian Problems in the Soviet Economic Literature of the 1920s]. (Problemy Sovetskoi Ekonomiki.) Moscow: Izdatel'stvo "Nauka." 1978. Pp. 257. 1 r. 40 k.

V. A. SIDOROV. *Klassovaia bor'ba v dokolkhoznoi derevne, 1921-1929 gg.* [Class Struggle in the Pre-Collective Village, 1921-29]. Moscow: Izdatel'stvo "Mysl'." 1978. Pp. 245. 1 r. 10 k.

V. P. Danilov is the dean of Soviet students of peasant life during the New Economic Policy of the 1920s. His latest book, which summarizes a number of his articles over the last few years, is a clear and elegant summary of the main lines of peasant social and economic development in the USSR on the eve of collectivization. He employs a minimum of obscuring catchwords, such as "kulak," and concentrates on how the village functioned as an economic and social unit. The book is divided into the three sections of the subtitle—population, land tenure, and households. Aware of the enormous regional differences, he attempts a reasonable, and persuasive, description of the whole. Necessarily, the small peasant household family unit, organized into village communes, dominates the picture. One of his important points, which he has made previously but which still comes as a shock to preconceptions,

is that peasant agriculture in the 1920s was on the whole remarkably successful in its own terms. This follows from his analysis of the effects of the agrarian revolution and the economic revival of the middle 1920s. His careful discussion of the number of households indicates that the negative effects of splintering were less than is usually supposed; household units increased from sixteen million to twenty-four million, rather than twenty-five. Productivity improved considerably compared to the prewar period and reached its high point in 1925 and 1926. In making the comparison Danilov quite properly includes prewar sharecropping among peasant farming rather than that of landlords. Separate peasant households in the 1920s increased their acreage, while the existing collective farms were primarily planted on familiar and long-used ground.

Danilov demonstrates the familiar weaknesses of small household farming in communes—the multitude of strips, their distances from the homestead, the obstacles to the introduction of new methods of cultivation—but his point is that the agrarian revolution did indeed lead to a rational liberation of peasant activity. The backwardness and misery of peasant life is brought out by the fact that everyone over the age of ten had a place in the rural work force. Danilov discusses social structure by looking at the distribution of occupations within the “family cooperative.” In the nature of things, over half of the *batraki*, full-time wage earners, the authentic agrarian proletariat, were under twenty—that is, they were people who had not yet started a family and who were therefore something less than full members of the commune. Danilov reminds us of the rigorous connection between family size and social position: poor peasants had small families, rich peasants had large.

The nationalization of land resulted in its effective disposition by the village commune, or *mir*, which was now called the “land society,” governed by a rural assembly, and granted a renewed lease on life by the revolution. As an authentic peasant organization it was supreme, and one of the more important and generally unsuccessful campaigns of the government was to bring the *mir* under the control of the rural soviets, precisely to break up the impenetrable unity of the *mir*. The authorities felt obliged to intervene if the *mir* repartitioned the land among its members too often, thus interfering with production, or not often enough, thus avoiding the egalitarian impulse of the revolution. They also tried to regulate repartitions along familiar class lines, so that poor peasants were to receive the best and nearest land and kulaks the worst and farthest—criteria that the *mir* found inane or impossible. Even when the criteria were formally ob-

served, they were frustrated by the molecular processes of rural life; poor peasants, without animals and tools, had to cede their better land to richer peasants under a wide variety of disguises. Initial NEP legislation was inclined to codify what the peasants themselves wanted. Along with the recovery of agriculture came attempts to introduce normative rules that made better economic sense. The supervisory rights of the rural soviets over the peasants’ *mir* became especially contentious in 1927–29 as the governing bodies themselves searched for new principles.

Danilov examines at some length the two visible alternatives to small-scale household farming within the *mir* that were present in the middle and later 1920s. One was the formation of separate farms, *khutora* and *otruby*, the model offered by the prewar Stolypin program. The other was the appearance of various forms of collective farming, state farms, co-operatives, and so on. On the face of it, neither was especially favored by the peasants, who were busy absorbing the results of the agrarian revolution. At the beginning of 1927 only about 1 percent of the land was worked cooperatively, and that under unfavorable circumstances. The movement toward entrepreneurial forms of agriculture was little more successful, given the combination of peasant community resistance and implacable government hostility. Danilov’s conclusion is that while peasant agriculture made considerable progress over prewar norms, it was unable to increase production sufficiently to meet the demands of the country. The crisis that emerged at the end of the 1920s was not generated by internal contradictions within the agrarian sphere by itself. Consequently, one may presume, collectivization was the result of pressure by the state. Danilov’s book is a first-rate description of how the peasants managed to cope before being pushed aside by exogenous requirements.

N. K. Figurovskaia’s essay is concerned with urban perceptions about the peasants during the 1920s. Its interest lies in the tangential recovery for Soviet readers of some sense of the alternatives that seemed to exist during NEP. She is poignant about the silence that fell for over two decades after 1930. Her discussion, however, of the issues during NEP is locked into a distorted pattern that makes it impossible even to describe the views of the most important contemporary students of village life. She is not really concerned with the economic problems but rather with their political dimensions. Susan Gross Solomon’s recent study, *The Soviet Agrarian Debate*, is much more forthcoming on some aspects of the material, though it does not deal with as wide a range of topics as Figurovskaia.

V. A. Sidorov’s book is an old-fashioned, unreconstructed treatment of Soviet agrarian affairs, re-

plete with castigations of kulaks, unspecified apart from their moral failings and their recalcitrance to the policies of the government. Yet, for all of his heavy breathing about class struggle within the village, he too feels that collectivization was less a process emanating from agriculture itself than a requirement coming from external demands upon the village that could not be met by the peasants using their old forms of organization—that is, a requirement coming from the needs of urban industrialization. The rationale for it, therefore, lies in the area of intersectional relations of the economy more than in intravillage social struggle. If so, it seems particularly unpleasant to label those who failed to rise to the challenge as vile enemies of the regime and their fellows, when the real difficulty was that they could not understand its purposes, let alone apply themselves to them. Sidorov's archival references remind us of the huge amount of factual material that remains to be brought into circulation to form the basis of judgment about what happened at the end of the 1920s.

Two comments seem in order. One is terminological, with important methodological implications. The neologism *dokolkhoznyi*, "pre-collective," to describe Russian individual peasant household farming appears to have become accepted usage by Soviet writers. By employing it, the only problem is to explain how peasants made the transition from a lower, incomplete stage to a higher more perfect form: the result is inherent in the adjective. Half a century after collectivization that might be thought legitimate, but it distorts the historical problems of the 1920s to the point of absurdity. The other comment is that the conceptual scheme of a "manufacturing stage" in collective farming, first proposed by Stalin, has been anonymously revived. Since the reference is an analogy to the discussion by Marx of "manufactures," with his litany of horrors about the extension of "absolute surplus value," one wonders whether Soviet writers are being ingenuous or Aesopian. My guess is that it depends upon the intelligence of the writer.

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AFRICA

L. H. GANN and PETER DUIGNAN, editors. *African Proconsuls: European Governors in Africa*. New York: Free Press. 1978. Pp. xi, 548. \$29.95.

This book is the latest volume in an ongoing effort by Peter Duignan and L. H. Gann to produce a total history of European colonialism in Africa (there

have already been one synthetic work, a five-volume set of topical essays, and now this and at least three other studies of colonial elites). It is also a collection of twenty-two biographical articles by seventeen authors varying greatly in their approach.

The general contribution that the editors claim they are making with this volume is to "raise some of the most significant questions which can be posed regarding modern Africa," since they think the governors discussed here "have as good a claim to be regarded as the state builders of modern Africa as the African nationalists who later took over from the colonial regimes" (p. 16). This is hardly a very original or penetrating comment and appears relevant only because the "African interpretation" with which the volume concludes (a device used in several of Duignan and Gann's previous collections) makes the equally unimpressive argument that the governors were nothing more than a group of authoritarian misfits working out their personal psychological problems in an Africa that had little need of them.

The substantive center of the book is far better than either the editors' introduction or the critic's conclusion suggest, but the book as a whole does suffer from failure to place the colonial experience within the kind of political and economic framework that might suggest its major historical relevance. Gann and Duignan, along with the authors chosen to write the introductory essay on each national group of governors, move somewhat in this direction by analyzing the social background of the men sent out to administer Africa (essentially a provincial bourgeois "service class" rather than either aristocratic or mercantile-industrial bourgeois, as is often claimed). This observation allows the editors to repeat their arguments against "Marxists" and tell us again how convinced Marx was of the modernizing effects of colonialism. They do not, however, come to grips with notions of social imperialism and dependent underdevelopment that are the mainstay of contemporary radical critiques of colonialism. Moreover, even the individual articles pay relatively little attention to economic issues beyond the building of infrastructures and the protection of African peasants against white settlers. It is particularly unfortunate that the manifestly close ties that existed between administrators and private-sector colonial interests in the Belgian and French empires are not explored (though touched on by Bruce Fetter) in the sections devoted to governors from these two African spheres. The only article with a clear economic analysis is one by Woodruff Smith on Julius Graf Zech, governor of the minor German colony of Togo.

In general, the individual articles in the book are carefully researched and well written; twenty to

thirty pages seems the ideal length for the treatment of colonial biographies. One drawback to the authors' expertise is that they have chosen to write about figures who warrant intensive individual study. We are thus presented with an array of heroic empire-builders, exemplary developers, and even one or two decolonizers, many of whom were distinguished for actions other than those carried out while functioning as governors and few of whom are really representative of their genre. Perhaps no governor could be fully representative, but it might have helped to throw in a few mediocrities or even downright failures and crooks.

Despite the atypically lofty stature of the figures chosen, most of the authors appear to maintain some critical distance from their subjects: John E. Flint does something of a hatchet job on the revered Lord Lugard (he allows that Lugard became a better person after retiring from Nigeria, which is when his more admiring biographer, Margery Perham, came to know him); Duignan gets a bit carried away with Sir Robert Cornynndon (a companion of Cecil Rhodes already!) but supplies enough information so that the defense of some controversial moments in the man's Uganda career seems reasonable, if awkwardly overstated.

The section on French governors by Leland Barrows, Henri Brunschwig, William Cohen, G. Wesley Johnson, and Virgil Matthew is perhaps the most informative since relatively little serious work has previously been done on the running of this colonial system (except for Brian Weinstein's biography of Eboué, which is condensed into an article here). The British section has a very lengthy introduction by A. H. M. Kirk-Green that effectively draws upon the rich material available in print to assemble a valuable and entertaining portrait of the gubernatorial phenomenon. Harry Gailey shows why Sir Hugh Clifford was really better than Lugard, and Ronald Robinson's brief essay on Sir Andrew Cohen breaks new ground on the final period of the British African empire, while whetting the appetite for a promised full-length biography. Both the introduction and the two biographies in the Portuguese section are by Douglas Wheeler, and again the information is quite new and valuable, even if it focuses more on how fascists and liberal nationalists thought about African territories than the manner in which they were actually governed. The Belgian and German sections are also brief; in the latter, Gann contributes an account of Heinrich Schnee which is nicely balanced between sympathy and criticism and comes perhaps closest of all the pieces in the book to presenting a typical figure.

Despite its shortcomings, therefore, this volume makes a valuable addition to the Gann and Duignan colonial history opus. It is not my idea of a

model for further work in this field, but, with Gann and Duignan themselves still going strong, that is not really an issue.

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TERENCE WALZ. *Trade between Egypt and Bilād as-Sūdān, 1700-1820*. (Textes Arabes et Études Islamiques, number 8.) Cairo: Institut Français d'Archéologie Orientale. 1978. Pp. xvii, 297.

This monograph is the first archival study of the historic trade route between Egypt and the Sudan. Based on the records of the Maḥākīm al-Sharʿiyya (Cairo), the Archives of the Ministry of Awqāf (Cairo), the French Archives Nationales, the British Public Records Office, and the National Archives at Khartoum, it resembles the pioneering work of André Raymond, *Artisans et commerçants au Caire au XVIII^{ème} siècle*, in its use of sources, focus on commodity prices, and the layout and structure of the Wakāla among the markets of Cairo. It differs from Raymond's work in its more monographic approach to the Gallāba, merchants who traded in the goods of the Sudan. Terence Walz's account also serves as a corrective to the popular view of the slave market found in Gerard De Nerval's account. Walz suggests the essentially utilitarian character of the slave as a commodity along with the complex moral and legal situation that the institution of slavery represented in Egyptian life. Walz's material on the escape of some slaves, the personality traits of male and female slaves who asserted themselves, and some information about their origins is quite new. The discussion of the training of slaves, both men and women, in arts and crafts, industry, and skilled cooking shows how their value rose as their potential for economic activity increased. The trade of the Gallāba merchants was not composed solely of slaves; diversification was an economic necessity. Ivory, ostrich feathers, and many other commodities were also traded.

In 1800 French officials collected taxes directly from the Wakāla for the central treasury, abolishing the middlemen of the Ottoman period. Later Muhammad 'Alī made his government the official purchaser and salesman of commodities. The number of products that the Wakāla traded shrank under this system of the Nāẓir Maṣlaḥat al-Wakālat al-Gallāba. By the middle of the nineteenth century the Sudan trade favored rich Greek merchants who went into partnership with the Assūyi's, bringing to an end the Wakāla in Cairo as an important institution.

The use of the trade route as a unit of analysis reveals how the Cairo merchants owned property

along their route in cities like Asyūṭ. The archives make clear that a wide range of "nationalities" were involved in this trade. The lives of three shaykhs of the guild further demonstrates the multisided activities of the merchants—as administrators, fiscal authorities, military figures, and labor negotiators with craftsmen. It is difficult to subsume all this under the concept of merchant or merchant capital, the concept in general use. What is now becoming apparent is the need for a new formulation. This comes out also in a consideration of the diversity of merchant nationalities; many came from nomadic areas such as oases. Perhaps these were neither merchants nor different nationalities but capitalists engaged in commercial and industrial production, and caught up in a labor history. The capitalists whose assets are listed in the archives needed nomads to carry their goods and artisans to fashion them for various markets. In all industrial contexts there is struggle. In this instance, presumably, sometimes the nomads had power and sometimes the artisans.

PETER GRAN
Temple University

PETER GRAN. *Islamic Roots of Capitalism: Egypt, 1760–1840*. (Modern Middle East Series, number 4.) Foreword by AFAF LUTFI ALSAYYID MARSOT. Austin: University of Texas Press. 1979. Pp. xvii, 278.

This is an important and stimulating book that challenges the ethnocentric notion that modernist thought and a capitalist economy could only be transferred to the peripheral states through direct contact with the European center. Peter Gran argues that the capitalist transformation of the Egyptian economy was begun by Muslim merchants and Mamluk rulers in the eighteenth century. Those chapters dealing with the cultural, mainly intellectual, underpinnings of this economic transformation are the most outstanding. Gran follows the career and writings of Shaykh Ḥasan al-ʿAttār (ca. 1766–1835) to demonstrate that the evolution of modernist concepts among the religious class in Egypt resulted from indigenous developments and was derived from classical Islamic sources. He reviews an impressive range of religious and literary works to demolish the previously held view that the eighteenth century was a period of economic decline and cultural stagnation.

The study is divided into two general periods that he sees as dominated by merchant capitalism (1760–90) and state capitalism (1790–1840). According to Gran, the economic structure had a direct impact upon the religious thought of these periods. "For the period in which merchant capital predominated, the modality of logic was induction, which was rooted in ḥadīth; for the period of state

commercial dominance systematic theology rooted in deductive logic enjoyed a rebirth" (p. 179). Herein lies the major weakness of this study. Gran has produced an outstanding intellectual survey from sources almost totally unread by Western scholars, but he stretches his literary sources too far and ties his theories to an ill-defined framework of a world market economy. Without consulting Egyptian, Turkish, or European archival materials he constructs broad theories of social and economic history, weaving into his theories an unusual number of dichotomous propositions and sweeping generalizations that are sure to be challenged, especially since they too often seem to place events in strict causal relationships. He argues, for instance, that "Aristotelian logic was at a low ebb in Egypt during the period of commercial predominance. It recovered in the early nineteenth century as the state took over the capitalist sector and subsumed it under its administration. Its recovery occurred as the actual social relationships shifted to the polarity of bureaucrat and peasant" (p. 50).

Few of Gran's conclusions will go unchallenged; some are unfounded. He implies that the Wahhabi emergence in central Arabia resulted solely from the decline of the Gujarat–Red Sea trade (pp. 101–02); asserts that the influx of European goods destroyed the guild structure by the end of the eighteenth century (p. 178), despite published work that demonstrates its survival throughout the nineteenth century (for example, Gabriel Baer, *Egyptian Guilds in Modern Times* [1964], which is not included in the bibliography); and argues that the religious leaders consciously promoted a Sufi revival among the masses as a means of stabilizing a turbulent social situation among the artisan class (p. 179).

Gran's book, required reading for students of modern Middle Eastern history, is a pioneering study into the intellectual and economic history of Egypt. Its basic contentions and sweeping conclusions are sure to provoke further debate and stimulate research.

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J. DEAN O'DONNELL, JR. *Lavignerie in Tunisia: The Interplay of Imperialist and Missionary*. Athens: University of Georgia Press. 1979. Pp. xvii, 300. \$17.00.

Although this book concentrates on Lavignerie's work in Algeria and Tunisia it also treats his entire ecclesiastical career from his leadership of the *Oeuvre des Écoles d'Orient* in 1856 in the eastern Mediterranean until his death in Algiers in 1892. The question of whether the love of the Church or French patriotism drove this energetic and ambitious man is never fully answered. The two forces were so

strong in his life that their influence is inseparable. In 1868 Charles-Martial Allemand-Lavigerie founded his Society of Missionaries of Algiers, the White Fathers, which became the strongest French missionary force in Africa. His ignorance of and prejudice against the Muslim faith was matched only by the zeal he put into the early mission of the White Fathers to convert North Africans. Lavigerie eventually understood that numerous conversions would not occur and he then had the mission concentrate on serving European Catholics and establishing schools and hospitals that also ministered to Muslims. As archbishop of Algiers Lavigerie worked diligently, starting in 1875, for the extension of French control over Tunisia. Since Louis IX of France (St. Louis) died in Carthage in 1270 while on a crusade, Lavigerie sought to have the see of Carthage restored with himself as bishop.

This book, using an abundant supply of primary sources, shows how Lavigerie was successful in the dual function of extending French control over Tunisia and becoming the archbishop of the archdiocese of Carthage, at Tunis. He also became the political confidant of the French consul general at Tunis, Théodore Roustan, and of Paul Cambon, who arrived as resident-minister in 1882. The book gives only slight consideration to the major changes necessary to give France real control in Tunisia—the abolition of capitulations and consular jurisdiction. This was almost solely the work of Paul Cambon and not of Lavigerie. The book gives the impression that almost everything depended on Lavigerie and it ignores the work of the highly competent Cambon. This is, however, the only weakness in a very valuable study of Cardinal Lavigerie (he received the red hat in 1882). It is also a scholarly contribution to the history of Tunisia. The mark that Lavigerie made on Tunisia and a fitting summary of his life is provided in Resident-General Rouvier's statement on Lavigerie's successor: "We need here a prelate with a French heart, who is a politician more than an apostle" (p. 196).

Lavigerie and Cambon each established a style in their respective offices in Tunisia that their successors could not match. Today, the Basilica Saint-Louis rises out of the Tunisian landscape as mute testimony to the work of Cardinal Lavigerie.

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JOHN VOGT. *Portuguese Rule on the Gold Coast, 1469-1682*. Athens: University of Georgia Press. 1979. Pp. xii, 266. \$18.00.

In 1482 the Portuguese built a fortress on the Gold Coast (modern Ghana), situated about eighty miles southwest of Accra. They called it "São Jorge da Mina" (Saint George of the Mine)—he "mine" being a wistful reference to the gold fields they were

never able to approach—but in recent centuries Europeans have referred to the place as "Elmina." The Portuguese traded at that location until the fortress was captured by the Dutch in 1637.

John Vogt's book tells the full story of Portuguese Mina for the first time, exhausting most aspects of the topic. This straightforward narrative monograph can be divided into four phases: (1) preliminary exploration and the building and organization of the fortress-factory (1434-82); (2) successful commercial operations (1482-1580); (3) declining commerce and challenges from the Dutch, French, and English (1580-1637); and (4) fall of Mina and eviction of the Portuguese (1637-82).

The European side of the story—based on archival research in Portugal, Spain, the Netherlands, Britain, and Italy—is told fully, but the African side, unfortunately, needs more imaginative treatment and further research. Interesting details include the Portuguese use of oared galleys to defend the coast, and the curious "backwards" slave trade, from the island of São Tomé to Mina, which involved slaves that probably originated in Congo-Angola.

"Appendix C" contains a table of Mina gold receipts at Lisbon for forty-eight scattered years during the period 1487-1572. They seem to indicate an annual average of from 450 to 500 kilograms during the years 1490-1520, falling to 150 kilograms per year after 1550. All the figures given add up to about 16,000 kilograms of gold—an amount that at the summer 1979 gold price of \$300 per ounce would be worth over \$150 million.

Vogt does not do much with his figures, but one can extrapolate from them to arrive at an estimate of total Portuguese gold extraction of 33 tons from Mina during the hundred-year period 1487-1586. This was enough gold to keep Europeans interested in Mina, yet represented only about 5 percent of the total world production of about 640 tons per century. And the figure of 33 tons pales beside the estimated 225 tons of gold extracted by the Spaniards from the Americas during the sixteenth century.

The book has a reproduction of a sixteenth-century sketch of Mina and also a map of the coast and a ground plan of the fortress. "Appendix B" lists the names of forty-eight governors of Mina, many of whom also served in India and Brazil.

It should be noted that the expulsion of the Portuguese from the Gold Coast in 1682 was not permanent. Hundreds of Portuguese and Brazilians, as well as Afro-Portuguese, traded there in the eighteenth and nineteenth centuries. Only in 1961, when the Republic of Dahomey was established, were the Portuguese forcibly evicted from their last fortress, the quaint compound of São João Baptista de Ajudá at Ouidah.

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RHODA HOWARD. *Colonialism and Underdevelopment in Ghana*. New York: Africana Publishing, 1978. Pp. 244. \$22.50.

The relation of colonialism to underdevelopment has become a prominent theme in recent works on African nations. Walter Rodney in *How Europe Underdeveloped Africa* (1972) and Chinweizu in his historical overview, *The West and the Rest of Us* (1975), have presented the theme most fully for Africa south of the Sahara. In the volume under review Rhoda Howard treats this theme to some extent in her postscript and in more specific detail for the Gold Coast (referred to as Ghana) from 1885 to 1939. The effects of colonial rule are analyzed along with the development of what is termed the peripheral capitalist economy and economic oligopolization. The formation of a new class system in the African sector is also traced in this new economy (p. 181). The author uses neo-Marxist "dependency theory" for making this analysis. Her primary sources are drawn from colonial documents and business records such as those of the Manchester Chamber of Commerce.

It would have been helpful if several aspects of the presentation had been clarified further so that the historical facts could be better interpreted by the reader. For example, when mentioning British involvement in economic activities on the West African coast in the seventeenth century (p. 29), a brief summary of the various activities undertaken by the British through each century mentioned would have been useful. Only brief attention is paid to the civil servants who came to Africa from Britain (p. 154), or, for that matter, from many other parts of the Commonwealth. Again the author should have indicated the period to which she refers: is it the 1920s or the 1930s? Also, some attention should have been given to the educational background of the so-called new poor.

Further, more detail could have been provided about the implementation of the 1937 agreement among various firms, together with more discussion about the types of recommendations that came from the Colonial Office as indicated in the Nowell Commission Report (pp. 108, 200). At the very least, reference should have been made to works that give these details (for example, my article on the 1930 Gold Coast crisis and 1977 volume on British business and Ghanaian independence). Mention of other sources on the effects of transferring banking institutions to the Gold Coast would also have been helpful (for example, C. Brown's 1965 seminar at Birmingham).

Howard elaborates on the colonial underdevelopment theme. One wonders though, if as a result of these economic trends, there were not some by-products that were or could have been used in the development of the economy for this new independent state. Certainly such aspects as a distribu-

tion system, a Western-educated elite, and business-oriented managers drawn from the indigenous population should be considered.

After the war years (1939-45), with the British Labour government's emphasis on development toward independence, the people of the Gold Coast moved quickly toward statehood. In her postscript Howard emphasizes continuance of the colonial underdevelopment theme for these post-World War II years, as did Bjorn Beckman in *Organising the Farmers* (1976) and R. Green and A. Seidman in *Unity or Poverty?* (1968). It is of course true that in this period economic and political systems throughout the world became more complex and interdependent. But are Marxist- or capitalist-derived characterizations sufficient to explain these complex new systems? Such characterizations may be useful in beginning an analysis, but they should not be regarded as providing full interpretations. In both the developed and developing nations today, internal governmental controls and international aids and controls make theoretical interpretations difficult. Perhaps our measure in evaluating governmental and economic development could be the degree to which government and business leaders are concerned with the welfare of the people as a whole rather than their own selfish interests. Particularly in the late 1970s Ghana has been ruled by military governments that have been concerned with their own interests. This situation has led to breakdowns in both the economic and political systems. These aspects are not covered by the Howard postscript. Chinweizu's advice "to demythify the conflict, pierce through all that ideological propaganda the West uses to screen off the real issue, and focus on the politics of world resources use" (p. 481) should be heeded.

JOSEPHINE F. MILBURN
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ARTHUR ABRAHAM. *Mende Government and Politics under Colonial Rule: A Historical Study of Political Change in Sierra Leone, 1890-1937*. Freetown: Sierra Leone University Press; distributed by Oxford University Press, New York. 1978. Pp. xiv, 330. \$22.00.

Recent historical work on Sierra Leone, as elsewhere in West Africa, has emphasized how the fluid and frequently innovative political institutions of precolonial states were often stultified in their development by the imposition of colonial rule. This argumentative book by a young Sierra Leonean historian, Arthur Abraham, examines the combination of force and guile by which the British imposed a protectorate over the Mende chiefs of southern Sierra Leone and then "fossilized" the Mende pattern of government into a fragmented mass of petty chiefdoms readily manipulated by the colonial administration. In doing so, he offers new details to substantiate the general "revisionist" approach, as

well as some challenging speculations that should stimulate further work.

Some of Abraham's major claims would spark little controversy. He demonstrates, for example, that domestic "slavery," which furnished one pretext for British intervention in Mendeland, was a very benign form of clientage, hardly worthy of abolitionists' fervor. His evidence makes clear that the main pressure for annexation of the Sierra Leone hinterland came from the Sierra Leonean traders, with more fitful support from colonial officials on the spot and fairly consistent, if irresolute, opposition from the Colonial Office. He acknowledges, too, that among British officials and politicians there was some concern over the duplicity and the brutality shown by some British representatives. He also neatly turns upside down earlier "modernization" arguments by demonstrating that, in making Mende chieftaincy ascriptive and insulating it from pressures from below, the British transformed it from a relatively "modern" to an "antiquated" institution.

His most important claim, however, is also his shakiest. Abraham contends that Britain deliberately fragmented Mende political institutions by reducing the position of "kings" of Mendeland's major states to the same status as their erstwhile subordinate "chiefs," thus allowing local decentralizing tendencies full play. Unfortunately, he never demonstrates that these "kings" enjoyed any meaningful power over their "states," except in those transitory situations where an effective warrior-administrator like Kailondo could subdue other chiefs during his lifetime. The heads of the five "territorial states" he perceives along the coast enjoyed symbolic superiority over other chiefs in their area, but there is no evidence to suggest such a "king" could compel these other chiefs or their subjects to do anything on his behalf. The "personal-amorphous states" of the interior, on Abraham's own admission, owed their being to the power of a particular leader, and it seems improbable that any would have survived that leader's death even if no colonial power had ever intervened.

Still, despite the weakness of its main contention and the fact that it is not quite as original as its author claims, this book is useful in providing us with further details on the workings of British colonialism.

JOHN R. CARTWRIGHT
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M. G. SMITH. *The Affairs of Daura*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xix, 532. \$30.00.

In 1804 in the area which later became northern Nigeria, the declaration of a *jihad* marked the cul-

mination of a religious reform movement and the beginning of successful military campaigns that toppled most of the ruling Hausa dynasties and created the Sokoto Caliphate, a loose federation of emirates headed by Fulani conquerors who owed allegiance and tribute to the successors of the original reformer. Historians have used Arabic correspondence and other writings of the Fulani aristocracy (as well as a smaller volume of material in Hausa and Fulani), the accounts of travelers, and oral data to describe the government and military affairs of the caliphate and individual emirates. Most recently studies of economic history, the sociology of warfare, and slavery have been in the forefront.

M. G. Smith's book returns to issues first raised in his own pioneering work on government in Zaria and shows how much can be learned from the study of these precolonial states. The book is the first in a series that will use Smith's field work of 1958-59 to trace the political history of the Fulani emirates as well as of Hausa states that successfully resisted Fulani conquest. The emirate of Daura was a wise choice for the launching of the new series. A Hausa government in exile survived there, and Smith can therefore compare four variations of precolonial government: Daura before the Fulani *jihad*, the Hausa successor state, one of two Hausa splinter groups, and the state formed by the Fulani conquerors. The book begins with introductory material and a chapter describing pre-*jihad* Daura government, the form of which persisted in the three other states under study. That government was characterized by a differentiated structure of status; offices as the principal units of government distinguished from each other by rank, title, material and social resources, traditions, rights, privileges, obligations, and relations to other units; chiefship as the most senior office; electoral councils; territorial administration through fiefs; and an administrative apparatus in which senior officials had staffs of titled assistants. The next chapter presents a minutely detailed political history of the main Hausa successor state, Zongo. Smith describes its successive schisms, its waning military fortunes and subjugation to a suzerain state, and the subversion of the power of the chief by the *kaura*—one of the two senior titled officials—with the collaboration of the suzerain ruler. The following chapters treat the Fulani government, one of the two Hausa splinter states, and restoration Daura under British colonial rule, which began in 1903. Not suspecting that the *kaura* was predominant in Zongo and choosing to ignore that he ruled half the kingdom, the British restored in one stroke the power that the chief had lost over several decades. Smith devotes the last three chapters to an analysis of the political narrative, first examining each important event by using an abstract scheme that consists of the latent cate-

gories in local government. He then uses corporation theory for a more concrete appraisal of change in the state, the government, and the chiefship.

Minor criticisms scarcely detract from this major achievement. Smith might have given readers more information as a basis to judge his use of oral data, his most important source material; but he inspires trust because of what he says about his method and because of his clear reasoning in areas where he is forced to speculate. Even the long delay in publication can be forgiven in view of the complexity of the task—Smith's interest in generalizing about political change—and the time he lost having the work translated and read to his principal Daura informants before publication.

STEPHEN BAIER
Boston University

JAN S. HOGENDORN. *Nigerian Groundnut Exports: Origins and Early Development*. (Ahmadu Bello University History Series.) Zaria: Ahmadu Bello University Press and Oxford University Press, Nigeria. 1978. Pp. xvi, 173. \$29.95.

Here is another important chapter in the history of *homo economicus* in rural West Africa. Complementing earlier studies of the rapid and rational responses of West African farmers to international marketing opportunities in palm oils and cocoa, Jan S. Hogendorn's splendid little book examines the peanut export boom from northern Nigeria in the early colonial period. The focus is on Kano province, from which most of the commodity came, during the two years after the Lagos-Kano railroad was opened to general traffic in April 1912. While the emphasis is on the Hausa farmers and traders of the region, the role of European merchants and consumers is also carefully presented. An epilogue usefully sketches the expansion of the trade down to the present.

The railroad's opening coincided with a boom in European use of margarine and the development of the process of hydrogenation of soft oils, creating a demand that drove the price of shelled peanuts in Kano from £5 a ton to £10 in the first season. The reaction of the Hausa, whom Europeans ironically had considered unresponsive to market forces to the point of "combative inertia," was dramatic. Peanut exports from Kano alone went from near zero to some ten thousand tons in 1912–13, clogging the railroad for months after the actual harvest, and Hausa farmers planted even more heavily for the next season. While the Hausa accurately foresaw the railroad's potential, the peanut boom, Hogendorn demonstrates, surprised almost all European officials and merchants. The British Cotton Growing Association and British colonial officials had been actively promoting the railroad in anticipation

of a boom in cotton that, in reality, never materialized. Hogendorn further argues that the significance and, indeed, the existence of this massive economic response by Hausa farmers and middlemen has not been appreciated because the exports in the subsequent two years were curtailed by extraordinary acts of man and nature. In 1913 the crop was decimated by one of the worst droughts on record, though peanut exports still climbed 16 percent above the previous season. Export of the 1914 crop became impossible when the opening of World War I cut off German shipping and markets. Once these impediments were removed, Nigerian exports soared to some fifty thousand tons a year in 1916–21 and eventually to a peak of eight hundred and seventy-two thousand tons in 1962–63.

Because Europeans had not foreseen the early peanut boom, the factors promoting African production and marketing went almost entirely unrecorded at the time. For this reason the most significant and original part of Hogendorn's work has been the reconstructing, through extensive oral interviews conducted in 1965 and 1975, of the ways in which Hausa farmers and traders had perceived this economic opportunity and responded to it. By combining this information about farming methods, land tenure, credit systems, and middlemen very skillfully with European colonial and commercial records, he has produced a detailed and definitive account of the beginnings of a major commodity trade. The material is presented with the rigorous precision of a trained economist and the sensitivity of one well familiar with the region. In short, Hogendorn has produced a superior monograph, meticulously researched, skillfully analyzed, and narrated with a lucidity, brevity, and grace that make reading it a delight.

DAVID NORTHRUP
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JOHN TOSH. *Clan Leaders and Colonial Chiefs in Lango: The Political History of an East African Stateless Society, c. 1800–1939*. (Oxford Studies in African Affairs.) Oxford: Clarendon Press. 1978. Pp. xi, 293. \$29.95.

This book presents a discussion and analysis of the structure of Lango political authority and its transformation over a century and a half since 1800. It is a welcome addition to a growing number of monographs on stateless and small-scaled societies in Africa (for example, J. Lamphear, *The Traditional History of the Jie* [1976] and J. B. Webster, *The Central Luo during the Aconya* [1976]), and helps to fill a void in the literature, which to date has too frequently discussed hierarchical state systems. John Tosh's volume is also important in another respect. He focuses on both the precolonial and colonial eras, thereby providing a useful comparative perspective

about the changing nature of political authority in an African society. Until now, too many studies have limited their investigation to one era or the other.

Tosh initiates his study with a valuable introduction on the "Stateless Society and the Historian in Africa." He reviews the place of stateless societies in African historical literature and capsulizes the conflicting viewpoints of anthropologists and historians. Until recently, the central cause for historians' neglect of stateless societies has been the debate concerning whether their oral traditions could be collected and evaluated. Tosh's work is an affirmation that they can be. He used oral interviews of Langi clan members and corroborated this material with colonial and other written records. Several examples of the transcribed interviews and a useful methodological statement on his fieldwork are included in two appendices.

Lango society, like others in northern Uganda and East Africa, was shaped by the patterns of migration and settlement, in this case primarily during the nineteenth century. For most of the period Lango political communities remained amorphous, controlled by clan and lineage leaders. The first four chapters of the book explore the evolution of this type of leadership pattern, its values, its weaknesses, its major personalities, and its tendencies for change prior to the appearance of British interests in the area in the 1890s. British rule imposed for the first time a centralized political system on the Langi. The transition of Lango political patterns to conform with British perspectives on administrative efficiency resulted in the evolution of the infamous "colonial chief" in a society that had no traditions of such focused authority.

The last four chapters provide an excellent discussion of the manipulations, frustrations, resistance, and restructuring of local politics during colonial rule. Particularly well elaborated are the details on how local families and individuals maneuvered and counterpaneuvored within the colonial system to achieve dividends, either by calculated resistance or collaboration.

As a sharply focused study of political change, the work contains few remarks about the concurrent economic and social change occurring in Lango society. Tosh does mention the impact of trade on early leadership patterns and discusses briefly the changing agricultural routines, the imposition of labor demands, and the impact of missionary education during the colonial period. Since these themes are not central to his thesis, he presumably leaves the reader to consult his other writings on some of these subjects (for example, "Lango Agriculture during the Early Colonial Period," *Journal of African History*, 19 [1978]).

In all, the book is a well-written and illuminating case study that helps to corroborate the important

perspective that African interests were central and in many ways shaped the process of colonial rule. By detailing the historical dimensions of Langi political values and actions as they faced a changing world, the book helps to remove the bias of colonial history from such evaluations.

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NORMAN R. BENNETT. *A History of the Arab State of Zanzibar*. (Studies in African History, number 16.) London: Methuen. 1978. Pp. 304. \$19.50.

With the active encouragement of its rulers, Zanzibar in the nineteenth century became the major entrepôt of the western Indian Ocean, servicing intensified world commerce and linking it with an ever-widening mainland network of trade in East Africa. Beginning in 1890, a British colonial administration reduced the domain and fortunes of the Busaidi dynasty and stabilized the island society by protecting the "Arab" owners of clove and coconut plantations. This class formed a local oligarchy supported by access to education, manipulation of labor to reduce its costs, and restraint on Indian creditors. A revolution of workers and lower classes against the sultan and the constitution took place shortly after the country achieved independence in 1963. The history of Zanzibar during the period covered by Norman Bennett's book is indeed epic, easily focused, and yet multifaceted.

What a disappointment it is, therefore, to find the rendering so narrow. The strength of the work, its close adherence to documentary sources, dissolves into weakness when the limitations of these materials become apparent. Much of the same evidence has been used in previous studies of the nineteenth century, most notably by Reginald Coupland, the Oxford scholar who wrote during the interwar period. Bennett, like Coupland, emphasizes rulers and rival foreign interests, but he goes further than Coupland's Anglocentric classic to consult American and French sources. The Coupland tradition, modernized by John Flint in his chapter on Zanzibar from 1890 to 1950 in the second volume of the *Oxford History of East Africa*, still has qualities of insight, sustained argument, and literary style that are singularly lacking here. How can the Methuen editors have been so indifferent? The main point, however, is neither the viability of the older scholarly tradition nor the inadequacy of style. What is most wanted is some reflection of recent historiography, with its concerns for the processes and ramifications of Western economic penetration as the context for politics. While the contributions of Abdul Sheriff and Fred Cooper are mentioned, the discussion does not grapple with the issues they have raised.

The leading components of the colonial situation are indicated without being brought into conjunction. Stress is laid on the varying fortunes of the clove industry and the mistake of excluding the Swahili elements of the population from educational opportunities. Yet, with respect to the latter, we learn that when these groups were offered schooling, they did not respond positively. Why? Insufficient attention is given to the mainland-derived workforce, the fluctuating commercial and port conditions in urban Zanzibar, and the rigidification of social and racial stereotypes, all of which were preconditions for the revolution of 1964.

A comparable one-volume history of Zanzibar before 1964 is not available. A gap has therefore been occupied, if not filled, and students have at least a point of departure. The unfinished task of interpretation remains. Those who are curious and investigative can go in many fruitful directions, but they must above all be propelled by the question that eludes the present author: "why?"

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BISMARCK U. MWANSASU and CRANFORD PRATT, editors.
Towards Socialism in Tanzania. Buffalo: University of Toronto Press. 1979. Pp. x, 243. \$20.00.

The development of public policy in Tanzania, presided over by the eloquent Julius Nyerere, has been of considerable interest to those concerned with modern African political evolution. In 1967 Tanzania's only party, the Tanzania African National Union (TANU), committed itself to achieving socialism. With most citizens engaged in subsistence agriculture, a severe shortage of skilled technicians and management personnel, and a clear certainty that rapid economic development was impossible, Tanzania appeared an unlikely locale for the success of any policy designed to ameliorate the living conditions of most of its population. Nonetheless, Tanzania's approach calls for careful observation since TANU, in contrast to most other African parties striving for progress toward some form of socialism, planned to manage the transition to full socialism without coercing the country's peasants and workers to follow foreign doctrines imposed by a narrow ruling elite. In 1976 a conference held at the University of Toronto considered the Tanzanian experience; this volume, with additions from subsequent discussions, presents a selection of these deliberations.

The editors open with a useful introduction, supplying the necessary background and the current state of the debate on the success or failure of Tanzania as a society in transition to socialism. R. H. Green continues with an investigation of goals,

strategies, and results from 1967 to 1974, his analysis presenting a clear description, with all mistakes and victories, of Tanzania's step-by-step implementation of policies. Green, along with several other contributors, identifies the most positive fact in the experience: the need for the government to convince peasants and workers that it is moving to meet their requirements. So far, it appears, this criterion has been achieved. I. Parker contributes by recounting the problems involved in realizing TANU's goals in one important organization, the National Development Corporation; his article is an informative effort except when he abandons a historical framework to plunge into policy recommendations. J. Loxley adds valuable analysis in considering monetary institutions and the class struggle; he among other matters elaborates upon the state of the Tanzanian bureaucracy, finally concluding that this group—so vital in every African country—is reasonably open to policies outside its own narrow interests. J. Barker and J. Boesen examine the most striking of Tanzania's initiatives, the effort to create a rural socialism, first through *ujamaa* villages and then through villagization, the latter a process requiring the moving of all Tanzanians into villages by 1976. Both authors show the many difficulties encountered in this major social transformation, with Boesen—to me the most perceptive contributor—offering ample evidence of how the government violated the ideals expressed through its leaders by coercing individual citizens into following decisions imposed from above. C. Pratt closes the volume with an intelligent piece in which he presents the views of critics of the Tanzanian experience, especially those he designates as "Marxist socialists," individuals who conclude that Tanzania is not in transition to socialism. In contrast, as an avowed "democratic socialist," Pratt contends that Tanzania, with a political party open to debate and with some form of elections to sample the opinions of the masses, is on the path to socialism. He recognizes that both democracy and socialism may not triumph in Tanzania but maintains that the path for both is still open.

Whatever the reader's predilections on the path of contemporary political development in Tanzania, and the remainder of Africa, this volume should provide considerable matter for reflection. Those interested in the emergence of democratic societies (in the Western sense) will find much of value to contrast with the political realities of life elsewhere in Africa.

NORMAN R. BENNETT
Boston University

ROBERT V. KUBICEK. *Economic Imperialism in Theory and Practice: The Case of South African Gold Mining Finance*,

1886-1914. (Duke University Center for Commonwealth and Comparative Studies, number 45.) Durham, N. C.: Duke University Press. 1979. Pp. xii, 239. \$12.75.

For three generations debate about modern imperialism has revolved around theories of its economic or capitalist roots and determinants. South Africa has been taken as the classic case. To the critique of this case Robert V. Kubiczek offers an interesting and original contribution. He agrees with those who assert that all interpretations are based on inadequate evidence of the structure and behavior of the business interests involved. The fundamental questions still need answering. Who, and what, were the controllers of the mines? What were the bases of their operations? What were their business strategies and how was capital raised and used? Previous studies are especially weak on these points. The special merit of this book is its use of new evidence in the mine controllers' records, especially those of the great Corner House group, but also of Consolidated Goldfields and numerous smaller operators. Much has been known, of course, but—and this is new—the result is a probing analysis that supplies unprecedented and extensive detail on financial sources and mechanisms, priorities and strategies, similarities and differences among groups, companies, and individuals. Its weakness lies in a relative lack of balancing treatment regarding political connections, but part of the argument apparently rests on the absence or fecklessness of such activities. Kubiczek is "struck not by the forcefulness but rather the restraint with which the magnates used their economic power" (p. 199).

His findings are destructive of economic determinist and neo-Marxist interpretations. Finance capital was fragmented, self-destructively competitive, and politically feeble. Absentee, international, non-British interests and perspectives tended to dominate. Business strategies were incompatible with government priorities. Rhodes, the outstanding local entrepreneur, was not prototypical: no other major magnate shared his intensely political priorities or boldness. Most were interested primarily in speculation, and much less capital was actually invested than was raised in promotion. Huge amounts went to safe government securities, expatriated personal fortunes, and ventures in other countries. It is well known that in the mid-1890s big German and French capital was interested in direct control, but the competition and irresponsible speculation of the mine controllers, political turmoil, and the onset of the war turned them away. Some mines were immensely profitable, but even the greatest groups, Corner House and Goldfields, were attempting to withdraw and were investing outside the empire. The last thing wanted was war and

British takeover. In the decade before 1914, big German, French, and British finance was retreating from South African gold mining.

This book offers neither a simple refutation of economic determinism nor unqualified support for its major alternative inspired by the well-known work of Robinson and Gallagher. Rather it adds qualifying complexity to the debate. "International capitalism, British imperialism, and Afrikaner nationalism did to some extent coexist. But these forces . . . were fundamentally at cross-purposes. South African developments, consequently, should be seen basically as a function of clashing priorities and the inability of any one or a combination of these forces to achieve supremacy" (p. 204).

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ASIA AND THE EAST

J. ARTHUR LOWER. *Ocean of Destiny: A Concise History of the North Pacific, 1500-1978*. Vancouver: University of British Columbia Press. 1978. Pp. xiv, 242. \$16.50.

The Pacific Ocean, "the ocean of destiny," has long "presented a physical and psychological barrier separating North Americans from the alien civilizations of Asia . . . [but] the Pacific also is a bridge" (p. xiii). J. Arthur Lower attempts to cross the bridge as a Canadian, outlining Canada's connections with the history of the North Pacific.

Beginning with European exploration of the Pacific, Russian expansion into Siberia, and Spanish missionary imperialism in California, Lower examines the role of the fur traders and sealers, the missionaries and traders in establishing contacts across the Pacific and in penetrating China and Japan during the nineteenth century. During the "decisive decade," 1840-50, the stage was set for an economic, social, and cultural revolution in East Asia. New power conflicts developed as both North America and Europe sought to build formal or informal empires in this region. The "open door" and the Russo-Japanese War were important landmarks in this process. The emergence of Japan as a great power at the beginning of the twentieth century pointed the way to the undermining of European influence and then the gradual withdrawal of Europe between the two world wars. Despite the defeat of Japan in 1945, the era of European (and North American) domination of the western Pacific ended with the Korean armistice in 1953. A new multipolar balance was then established among

China and Japan, the Soviet Union and Korea, the United States and Canada.

The new multipolar balance is in fact quadrilateral rather than hexagonal: Canada and Korea are at present junior members. Whether a North-West Pacific community is developing is another matter. There are numerous bilateral economic and cultural agreements between members, but these fall far short of community ties; the European Economic Community and NATO are vastly different organizations.

Canada's role in the North Pacific has so far been marginal, since its foreign policy has been oriented toward Europe rather than Asia. Canadian participation in the Korean War was, in a sense, out of character, despite active participation in peace-keeping operations elsewhere; Canada, as a member of the ICC, "excused itself from direct involvement in the Vietnam War" (p. 172). Despite Lower's argument for an increasing Canadian role—scientific, cultural, and economic rather than military—in the North Pacific, he has to admit that in recent years it has become almost a forgotten region as "Canadians give a low priority to North Pacific affairs" (p. 205).

The view from Vancouver or Ottawa is not very different from that from San Francisco or Washington. It is lower in key, less concerned with power; policy is more passive than aggressive. As a middle power with a long common boundary with a superpower, Canada's freedom of maneuver is limited. "From the Asiatic point of view Canada is a friend, if not an ally or satellite, of the United States" (p. 175).

Lower has dealt very thoroughly and competently with the process of opening up the Pacific and East Asia. His book is very well illustrated and contains useful tables. The fine edge of his writing is blunted only in the last chapter, which tends to become a tract on modern Canadian interests in the Pacific. His range of sources is impressive, though it is a pity that he did not include the work of an important Canadian diplomat in his bibliography, E. H. Norman's *Japan's Emergence as a Modern Power*.

NORMAN HARPER
University of Melbourne

RAYMOND DAWSON. *The Chinese Experience*. New York: Charles Scribner's Sons. 1978. Pp. xxv, 318. \$25.00.

As a general introduction to traditional China, *The Chinese Experience* by Raymond Dawson is unique in three aspects. First, instead of giving a chronological account, it describes "the essence of the Chinese experience in all the major branches of human activity" (p. xv). As a result, the main body of the book is divided into four parts: Part one deals with the

political experience, part two with the philosophical experience, part three with the social and economic experience, and part four with the esthetic experience. Second, these facets of the Chinese experience are chosen and explained with a view to contrasting them with those of the Western experience. For example, as the author rightly observes, although religion colored all aspects of Chinese life, it never attained "that separate and independent status which a powerful church has sometimes achieved in European society" (p. 164). In the realm of art, it is important to know that the sense of movement in Chinese landscape painting required the Chinese artists to adopt a shifting perspective that is markedly different from the one-point perspective in European art (p. 212). There is also much truth in the observation that the Chinese language, characterized mainly by a "magnificent economy," is not the kind of vehicle of philosophical thought that "could have nurtured some of the problems which have preoccupied Western thinkers through the ages" (p. 236). Third, the Chinese experience is treated mainly as a living tradition. Certain traditional modes of thinking and patterns of behavior, for instance, are seen to be continually active in today's revolutionary China.

This last point is well taken in the author's discussion of the twentieth-century experience in his epilogue, where he shows great insight when he says, "The old-style mandarins who knew the Classics by heart have their modern equivalent in the cadres who are expert in the current orthodoxy. Very traditional, too, is the emphasis on political correctness to the detriment of technical expertise" (p. 288). The simple fact that the "redness versus expertness" controversy has been much more heated in post-1949 China than in any other socialist society clearly suggests that the problem must be deeply rooted in Chinese tradition. The author interprets this phenomenon in terms of the Confucian scheme of things, which always places "right thinking" above "technical concerns" (also see p. 19). Generally speaking, this is correct. It is also necessary, however, to point out that contemporary Chinese views on this matter have been sharply divided between those who stress the primacy of "redness" over "expertness" (*hsien-hung hou-chuan*) and those who embrace equally "redness" and "expertness" (*yu-hung yu chuan*). This division immediately reminds us of the Neo-Confucian rivalry between the Lu-Wang and the Ch'eng-Chu Schools. As we know, according to Lu Hsiang-shan (1139-93) and Wang Yang-ming (1472-1529), a man must set his mind morally right before he can meaningfully begin any kind of intellectual inquiry, whereas, according to Ch'eng I (1033-1108) and Chu Hsi (1130-1200), morality and knowledge are actually as inseparable as Siamese twins. Unfortunately, the

author says nothing about Neo-Confucianism and thus leaves half of the Chinese philosophical experience unaccounted for.

Equally unfortunate is the author's complete silence about the role of the prime minister(s) vis-à-vis that of the emperor in the Chinese political tradition. Almost from the beginning of the imperial age in 221 B.C., the relationship between the emperor as head of the state and the prime minister as head of the officialdom was characterized, more often than not, by tension and conflict that eventually culminated in the abolition of the prime minister-ship by the founding emperor of the Ming dynasty in 1380. The relevance of this historical background to the most recent Chinese political experience of the so-called Cultural Revolution is obvious. Is it too far-fetched to suggest that the role played by Chou En-lai is more reminiscent of that of a traditional prime minister under a founding emperor than that of a comrade-in-arms under a revolutionary leader?

YING-SHIH YÜ
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EVELYN SAKAKIDA RAWSKI. *Education and Popular Literacy in Ch'ing China*. (Michigan Studies in China.) Ann Arbor: University of Michigan Press. 1979. Pp. xii, 294. \$16.50.

Modern researchers of traditional Chinese society continue to assault the notion of elite and popular cultures as sharply differentiated and mutually impenetrable traditions. The book under review carries this discussion into an area that obviously is crucial to the issue: the distinctive Chinese ideographic script and educational efforts to master that script. Using evidence from Chinese local histories, clan genealogies, and school texts as well as a wide array of Chinese, Japanese, and Western secondary works, Evelyn Sakakida Rawski shows that in eighteenth- and nineteenth-century China there was a continuum from the barely literate to the highly educated scholar-elite class. Although the book focuses on Ch'ing China, the first and last chapters place education and literacy in the context of both traditional and contemporary China. The intervening chapters treat the types of education available, its cost and financing, the distribution of schools, popular literature and educational texts, and the role of literacy in Ch'ing society.

The author contends that the term literate should not be reserved just for those in traditional China who after long years of study mastered the Confucian Classics and moved into elite ruling circles, or at least attempted to. This fully literate elite was only the most visible end of a spectrum of literacy. There were also the educated merchants, shop-

keepers, and artisans in the urban areas, and even some landlords and rich peasants who could use their elementary literary skills to advance their careers. Below this level came functional literates who knew only a few hundred characters, but whose command of limited and often specialized vocabularies enabled them to keep records, read proclamations, and cope with the practical demands of daily life.

The bulk of the population was illiterate, with little or no recognition of the Chinese written script, but Fawski estimates that 30-45 percent of males and 2-10 percent of females possessed some level of literacy—functional, elementary, or advanced. The male rate is comparable to Japan's during the late nineteenth century and probably higher than in preindustrial Europe. Other variables, therefore, the author argues, must be used to explain Meiji Japan's rapid modernization and China's slower response.

The author has surveyed the educational materials used in elite and popular education, and again she finds that a continuum existed in the content and emphasis of these texts. At the popular level, there were arithmetic guides and character books (*tsa-tze*). The latter ranged from simple illustrated glossaries (in which abstract ideas and Confucian concepts were ignored in favor of concrete objects and matters relevant to daily life) to collections of rhymed couplets forming meaningful phrases that began to exhibit elite concerns, to elaborate narratives for specialized audiences—such as merchants, prosperous suburban households, and children of middle-peasant families. As further aids, a person of limited education could consult a variety of popular encyclopedias.

In the elementary education provided by community and clan schools, whose numbers were increasing in the eighteenth and nineteenth centuries, three basic primers, the *Thousand-Character Classic*, the *Trimetrical Classic*, and the *Hundred Names*, introduced Chinese youth to China's past, its heroes, and Confucian precepts for proper conduct. The bulk of the school boys (girls rarely enrolled in these schools) received further Confucian values through popularized versions of the Classics. Boys from elite families preparing for the civil service examinations, however, were enrolled in formal studies with a tutor to tackle the central works of the Confucian orthodoxy, the Four Books and the Five Classics.

There are some organizational weaknesses to this study. The author has amassed a great array of details on the subject but occasionally presents it in a fragmentary fashion. This sense of disconnectedness is heightened in those chapters that have ten or more short sections. The concluding chapter, "Continuities in Modern Chinese Elementary Education," is more appropriately an epilogue; it does not

provide the reader with an adequate summary of the book's arguments. But these are somewhat picky observations. More importantly, the author has provided a new, richly detailed look at a key social institution of traditional China, the educational system in its elite and popular forms.

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THOMAS L. KENNEDY. *The Arms of Kiangnan: Modernization in the Chinese Ordnance Industry, 1860-1895*. (Studies of the East Asian Institute of Columbia University.) Boulder, Colo.: Westview Press. 1978. Pp. xvi, 246. \$17.00.

Was "self-strengthening" in nineteenth-century China a progressive movement revealing the resilience of the traditional order and its ability to innovate, or was it rather a conservative phenomenon that inhibited more fundamental responses? Although Thomas L. Kennedy's study of the fledgling Chinese ordnance industry does not explicitly ask this question, it is one that has inevitably informed most studies of modernization in late imperial China. Kennedy demonstrates that the self-strengthening movement went farther toward industrial modernization and had wider implications than has usually been assumed.

The narrative follows the development of modern arms production from its origins in the crisis of 1860, which brought home to leading Chinese statesmen the urgency of military modernization, to China's disastrous defeat by Japan in 1895. The initial efforts to supply arms for the armies engaged in suppressing the Taiping and Nien rebels were succeeded by the emergence of more permanent establishments when, after 1868, the orientation shifted to maritime defense against the Western powers. From 1868 to 1875 production of the three principal arsenals was dictated by the different perspectives and needs of their leading sponsors. The Kiangnan Arsenal, under Tseng Kuo-fan's influence, was oriented almost exclusively to steamship construction, while those under Li Hung-chang's tutelage were involved in ordnance production. From 1875 to 1885 a major reevaluation of policy culminated in the termination of shipbuilding and the development of ordnance, especially heavy artillery for coastal defense, at all three arsenals. Finally, after the experience of the Sino-French war in 1885, efforts were made to modernize the production and administration of the arsenals. (In two preliminary chapters, Kennedy discusses the development of ordnance production in traditional China before 1860 and the intellectual background of institutional reform and self-strengthening since the seventeenth century.)

Attempts to understand the promise or failure of self-strengthening have usually amounted to an examination of the political and social influences that retarded its development. Kennedy also takes this approach. The problems afflicting the ordnance industry included excessive costs of production and overhead, especially costs of material and personnel; difficulties in mobilizing and maintaining financial support; the nature of leadership and management; and foreign technological dependence. Perhaps the most important single influence was the decision, concluding the policy debate of 1872-75, to give priority to inner Asian frontier defense. Kennedy does not fully pursue the implications of this fateful decision, which pre-empted the emergence of a coordinated industrial modernization program. Although the arsenals made quite a respectable showing in arms production, it is questionable whether the foundations of a "military-industrial complex" (p. 18) ever existed outside the minds of a few Chinese proponents. The shortcomings of self-strengthening lay particularly in this failure to achieve a coordinated enterprise. Although the weakness of this monograph is that it stops short of probing the broader implications of self-strengthening and its failures, it nevertheless adds much to our knowledge of the movement.

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JING SU and LUO LUN. *Landlord and Labor in Late Imperial China: Case Studies from Shandong*. Translated with an introduction by ENDYMION WILKINSON. (Harvard East Asian Monographs, number 80.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1978. Pp. xiii, 310. \$15.00.

In the late 1950s Jing Su and Luo Lun of Shandong University wrote a monograph arguing that in the Qing (1644-1911) capitalist elements were emerging in rural north China. The key to this emergence was commercial landlordism. From conventional sources they showed that Shandong towns were growing, specialization increasing, and commercial agriculture expanding. In the countryside emerged an incipient trend toward a two-class society, a rural bourgeoisie exploiting a rural proletariat. Then they adduced some new sources: landlord account books and a survey of old-timers in one hundred ninety-seven villages. These sources showed that some "managerial landlords" flourished around 1900. Jing and Luo argued that this stratum must have been emerging in the Qing, an argument that squared with Mao's dictum that China had its own protocapitalist phase.

The study drew Japanese and Western attention in the 1960s. Its conclusions were not novel or fully acceptable. But the authors had focused on a little-

studied region, and they had generated some new data. Consequently, Endymion Wilkinson has translated it and supplied a critical introduction.

The following picture of Shandong emerges from the book. Rural and urban population grew steadily. The economy adapted through existing technology. Indeed, the society seemingly could not make full use of established techniques. Managerial landlords' yields far exceeded those of small land holders, but the landlords cultivated only a small fraction of Shandong's total arable land. And these efficient farms, pieced together over decades, sooner or later refragmented because of inheritance rules.

Shandong's rural inhabitants arrayed themselves on a finely graduated scale of wealth, status, and power. Some of the landless worked as long-term laborers for landowners, and some sharecropped. Some families owned a little land and rented a little. At a higher rung were families who owned enough to live on. All of the above might hire themselves out in peak seasons, and many pursued handicraft or peddling sidelines. Richer peasants might employ seasonal workers or even long-term labor, because their holdings were more than a family could farm. Then there were families who built upon rich peasant wealth, upon urban business money, or upon the spoils of officialdom and accumulated large holdings of one or two hundred acres, rarely as much as a thousand acres. Although not at China's social pinnacle, these families enjoyed local influence and sometimes purchased rank. The cream lived in the district capital and hobnobbed with officialdom.

Big landlords rented out their land to numerous small tenants. But some farmed directly fifty to a hundred acres. They supplied animals, tools, and hired labor. Part of the harvest fed and clothed their large households and their employees; part went to market. These families also operated handicraft, commercial, and financial enterprises.

Jing and Luo have presented new data on estate accumulation and dispersion, on the limits to managerial farm size, on the proportions of rented to directly managed land, on inputs and yields, on landlord backgrounds, and on employment conditions. Wilkinson's introduction supplies the context for assessing the study and points out some mistakes, excesses, and omissions. The whole is a worthy contribution to the study of traditional rural society.

CRAIG DIETRICH
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LUC KWANTEN. *Imperial Nomads: A History of Central Asia, 500-1500*. Philadelphia: University of Pennsylvania Press. 1979. Pp. xv, 352. \$25.00.

A new history of Central Asia, attractively packaged by a major university press, which aims to get

rid of old clichés and give us a new perspective on this key aspect of world history, incorporating all of the new scholarship that has been focused on this area in the more than three decades since the publication of René Grousset's *L'empire des steppes*, is surely an exciting prospect for a reviewer. Alas, my anticipations have been grievously disappointed.

The majority of Luc Kwanten's pages have to do with the Mongols, but he seeks to link this most spectacular and extensive of the steppe empires with the long tradition of similar eruptions that lay behind it. This is a sound idea, but hardly as original as he seems to imagine. He criticizes the "evenemental approach" that has allegedly prevented his predecessors from realizing that there was "an autochthonous historical tradition" in the steppe and that has led them to concentrate on charismatic personalities such as Chinggis Khan. Certainly there have been popular works of this kind, but the recurrent patterns of nomadic political formations and imperialistic expansion have not gone unnoticed or lacked attempts at explanation. The author's own attempts at providing continuity and a conceptual framework are not impressive. It takes more than a ritualistic invocation of the names of Fernand Braudel and the *Annales* school to give a structure to the chain of events and reveal their inner connections.

It is astonishing that the name of Owen Lattimore is never mentioned by the author, even in his bibliography. Although Lattimore's work is certainly in need of revision, his conception of the role of the frontier provides a far more sophisticated analysis of the cycles of political rise and decline on the steppe and their relation to economic and political cycles in the agricultural civilizations to the south than anything offered here.

Kwanten's narrative is not only weak in general conceptions but also highly unreliable in its factual information. Space will not permit a detailed listing of errors of this kind. A serious shortcoming, though, is the gross underestimation of the role of the oasis dwellers along the trade routes of Central Asia. One would scarcely guess, if one did not already know it, that when the Chinese first penetrated into this region toward the end of the second century B.C. they found a series of city states occupied by Indo-European speakers and that the struggle for control of these states and the trade routes that passed through them was an important aspect of the conflicts between the Han empire and the Hsiung-nu. The occupation of these oases by the Uighurs in the ninth century and the sedentary, literate civilization that developed therein are described, incredibly, without reference to the Tocharian Buddhist civilization that it supplanted and absorbed. Kwanten refers to the role of Sogdians in converting the Uighurs to Manichaeism while they were still on the steppe but seems to be totally un-

aware of the earlier role of the Sogdians in the empire of the Turks.

One could go on but space will not permit. There is certainly room for a new interpretive outline of the role of Central Asia in world history, from the first harnessing of the horse to the war chariot until the eastward expansion of the Russians, but this book, regrettably, does not fill that need.

EDWIN G. PULLEYBLANK

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ROBERT JAY LIFTON *et al.* *Six Lives, Six Deaths: Portraits from Modern Japan*. New Haven: Yale University Press. 1979. Pp. xiii, 305. \$16.95.

In this collaborative effort a prominent American psychohistorian, Robert Jay Lifton, and a Japanese man-of-letters, Shūichi Katō, assisted by a graduate student, Michael R. Reich, study six "deaths and lives as a way of exploring issues of history and social changes" (p. 289). In the first chapter Lifton explains that this psychohistorical study incorporates Erik Erikson's perspective on the "great man or woman in history" and his own shared-themes approach—namely, "a psychological investigation of a group of individuals who commonly experience an important historical event" (p. 5). In this instance the authors focus on six separate lives and deaths in terms of their special significance and examine the intermeshing of three levels of collective historical, cultural, and universal experience in their subjects' lives. Death and continuity is the paradigm used, and the theme of symbolic immortality, Lifton's hobbyhorse, remains constantly in the background.

In linking the lives and deaths of individuals to the larger sociohistorical context, the authors assert that revolutionary changes are like historical deaths. In modern Japan two such historical deaths occurred: the Meiji Restoration and the defeat in World War II. These events and the lives of the six men, the authors argue, were closely interlinked. This thesis is borne out especially convincingly in two instances: that of the Meiji Restoration and the "hero" of the Russo-Japanese War, General Nogi, and that of World War II and the novelist, Mishima Yukio. These two men were torn between the traditional and the newly emerging values as their country underwent dramatic changes; both committed suicide by the sword. The others (the novelist-army medical officer, Mori Ōgai; the progressive political theorist-social critic, Nakae Chōmin; the Marxist academic, Kawakami Hajime; and the novelist-literary critic, Masamune Hakuchō) were also riven by the changing times, thinking about and facing death in their own unique and courageous manner; but they did not make as striking an imprint on the Japanese mind as did Nogi and

Mishima. All of these men were alienated outsiders whose (dare I use the word?) neuroses were kindled initially by a demanding or possessive father, mother, or grandmother and fanned by the changing sociocultural environment. Their psychological tensions and obsession with death made them acutely sensitive observers of their society and time and generated in them a creative vitality. Despite the authors' emphasis on death, what is remarkable about these men is the way they lived—with courage, integrity, and moral sensitivity.

This study overflows with incisive, thought-provoking observations, which, I regret, space does not permit me to illustrate. Let one example suffice: "Nogi's particular human combination of achievement, confusion, folly, glory, anachronism, rigidity, despair and determined self-completion . . . was that of the Meiji era" (p. 62).

One may not be fully satisfied that the themes outlined in the first chapter are convincingly delineated in all cases; one may question some of the assertions (for example, the claim that the Japanese made the leap to Christianity because of its powerful image of death and revitalization is contradicted by the authors' own discussion of Kawakami's dramatic leap of faith triggered by his encounter with the Sermon on the Mount), the overloading of some of these men with a medley of psychological hang-ups, and the appropriateness of some of the comparisons with Western personalities (for example, Mori Ōgai with Montaigne), but unquestionably this work is a major contribution to Japanese studies, to the art of biography, and to the discipline of psychohistory.

MIKISO HANE
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CHONG-SIK LEE. *The Korean Workers' Party: A Short History*. (Histories of Ruling Communist Parties.) Stanford, Calif.: Hoover Institution Press. 1978. Pp. xiv, 167. \$5.95.

The first in a series of monographs on the histories of the sixteen ruling Communist parties, this work presents a succinct and readable summary of the origins, structure, and functioning of the Korean Workers' Party (KWP), the ruling party of the Democratic People's Republic of Korea (North Korea). Chong-Sik Lee brings to his task an impressive array of credentials: more than a decade of laborious and path-breaking scholarship on the various aspects of Korean communism and an enviable linguistic competence that enables him to use original sources in the Korean, Japanese, Chinese, and Russian languages.

Indeed, it is precisely because the author is a pioneer in his field that a preliminary question arises:

What, if anything, does he have to add to his already voluminous publications, not to mention to the literature of Korean communism? The question becomes germane, because the author, in collaboration with Robert A. Scalapino, published the award-winning *Communism in Korea* (2 vols., 1972), which, together with Dae-Sook Suh's *The Korean Communist Movement, 1918-1948* (1967), virtually exhausts the topic. The author's own answer is that there is room in the literature for "a shorter work of interpretative nature" (p. xiii). Additionally, he provides a new interpretive analysis of the developments in North Korea since the Fifth Congress of the KWP in November 1970. In short, the present monograph is not merely a summary of the massive Scalapino-Lee study but also contains new interpretations.

To the uninitiated, the following features of Korean communism that emerge from this study may be of interest: First, in its formative stage, the early 1920s, the Korean Communist movement was an offshoot of the Korean independence movement. Second, the Korean Communist movement was plagued by factionalism; the situation was so bad that there were even bloody clashes between contending factions, and the Comintern eventually ordered the dissolution of the Korean party. Third, although riven by factionalism, the movement was remarkably free from theoretical disputes, which Lee attributes primarily to the domination of the movement by the relatively uneducated—a situation that contrasts sharply with the experiences of China and Japan. Fourth, Korean Communists were scattered widely both inside and outside of Korea, notably in China and Russia. Fifth, the movement was singularly unsuccessful. Lee cites as the principal reasons for the Communist failure (1) the brutal efficiency of the Japanese police, (2) Korea's then precapitalist stage, and (3) the adverse impact of the Comintern, which prescribed radical and counterproductive guidelines in disregard of the problems on the scene. Sixth, the *coup de grâce* was administered by Kim Il-sŏng, a man totally alien to the Korean Communist movement, although he did play a part in the anti-Japanese guerrilla operations under the direction of the Chinese Communist Party. Kim all but decimated the remnants of the Korean Communist movement after taking over the reins of power in North Korea under the auspices of the Soviet occupation authorities. Seventh, since 1949 when the KWP was formed, Kim has not only consolidated his power beyond challenge but also transformed the party into an intensely personal one—a veritable tool in the propagation of a cult centering about himself and his legendary family. Finally, the KWP has been grappling with the problem of striking a balance between redness (loyalty) and expertise. As the grow-

ing requirements of industrialization dictate the primacy of expertise, the KWP is becoming increasingly dominated by technocrats.

Many problems remain in North Korea, not the least of which is one of the institutionalization of its political system, for the supreme leader is bound to pass from the scene sooner or later. In sum, this is a useful short book, which is likely to inform and stimulate the layman and the expert alike.

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SUKHDEV SINGH CHARAK. *History and Culture of Himalayan States*. Volume 1, *Himachal Pradesh*, Part One. Foreword by S. C. DUBE. New Delhi: Light and Life; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. x, 413. \$32.50.

The Himalayan region of the Indian subcontinent contains a diverse range of peoples and cultures whose history, like the terrain they inhabit, consists of isolated fragments. Historical studies of these peoples—as of most others in South Asia—that take indigenous perspectives seriously are only now beginning to emerge. In a massive series, projected to extend to ten volumes, Sukhdev Singh Charak attempts to provide a comprehensive account of the social, cultural, and political history of each of the states of the entire Himalayan region. The first volume, considered here, treats Kangra and related states in present-day Himachal Pradesh.

Much of the earlier work on this northerly fringe of South Asia consists of studies by the British imperial government, which attempted to categorize the people it ruled. From the end of the eighteenth century until independence in 1947, imperial administrators produced gazetteers, settlement reports, and other accounts that classified not only the land but also the social and religious practices of every "caste" and tribe under their authority. Rich in detail and description as such surveys are, they necessarily represent an exogenous vision, often saying as much about the categories of the author as those of the subjects. Much current work seeks to remedy these deficiencies by examining South Asian society as it sees itself.

Although Charak displays an awareness of some of the indigenous sources available to historians of the region, including family histories, epigraphic and numismatic materials, and folk traditions, his decision to deal with such a vast topic apparently forces him to rely primarily on the aforementioned official and exogenous sources. Charak promises to deal with not only the approximately eighty states of Himachal Pradesh but also Assam, the North-eastern Hill States, Jammu and Kashmir, Sikkim,

Bhutan, and "possibly Nepal" as well. To present a "comprehensive study," Charak thus draws almost exclusively on English printed secondary sources. Whole sections of his history are taken directly from other texts, including one fifteen-page section (pp. 30-44) from John Hutchison and Jean Vogel's two-volume *History of the Panjab Hill States* (1933). Later, an exact quotation from another source extends unbroken for ten pages (pp. 283-93). Charak further detracts from the value of his contribution with incomplete footnotes and the absence of any bibliography. The layman may be confused by a mass of occasionally inaccurate dates and inconsistently spelled names: the river Sutlej, for example, also appears randomly as Satluj and Satlej.

Despite these flaws, however, Charak's work draws together a series of separate and less accessible accounts and histories. He takes into account the Himalayan environment that helped fashion these numerous states. He presents us with a picture of each as it struggled, apparently perpetually, for its political autonomy and cultural identity against outsiders: other hill states and the larger Hindu, Muslim, and British empires of the plains. We can further hope that Charak's ambitious effort will serve to stimulate interest in the region, providing landmarks for future research.

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J. VAN GOOR. *Jan Kompenie as Schoolmaster: Dutch Education in Ceylon, 1690-1795*. (Historische Studies, number 34.) Groningen: Wolters-Noordhoff. 1978. Pp. 205. f 35.

Nineteenth-century European imperialists in Asia controlled wide territories, systematized (or transformed) traditional taxation systems in search of revenue and stability, created a dependent local elite and attempted to restrain its exploitation of the peasantry, and saw in the spread of missions and of European-style schooling sources of political strength for the empire as well as self-sufficient religious and cultural values. J. Van Goor's groundbreaking book allows us to see Dutch policy in Ceylon (now Sri Lanka) in the eighteenth century as one of the most important and revealing precursors of all of these nineteenth-century phenomena. The author's choice of the schools as the focus for his study was an excellent one. Indigenous schoolmasters, apparently usually members of prominent local families, kept the records for Dutch exaction of local taxes and labor services. The Dutch authorities saw the conversion to Protestantism of their Roman Catholic, Buddhist, and Hindu subjects, largely through these local schools, as one of the most effective means of assuring their loyalty. The

seminaries in Colombo and Jaffna were to produce the necessary schoolmasters, indigenous ministers, and so on for this missionary effort and also were to provide European-style education for sons of Dutch residents and for sons of the indigenous elite who sought to qualify for full-time employment by the Dutch East India Company or for rural posts that reinforced customary dominance.

Van Goor's study is based on wide research in the great archives of the Dutch East India Company, Dutch church archives, and a small amount of material in Sri Lanka. It has required an extremely conscientious piecing together of bits of information (1,097 footnotes for 144 pages of text!) and is full of fascinating detail on the differing responses of castes and ethnic groups to the schools. Appendixes list the names and careers of individuals known to have attended the seminaries. We see the school system staggering under its multiple functions, always starved for funds and personnel by the cost-conscious company, facing cultural obstacles that few Dutchmen seem ever to have understood. Permanent and genuine conversions to Protestantism were scarce except in Colombo and its environs, and the seminaries were more effective in producing a bicultural and multilingual indigenous elite than in training mission personnel.

In view of the arduous research and very substantial thought embodied in this book, it may seem unfair to say that it would have been much better if the author had taken a year or two to expand and revise it into a more polished piece of work with more substantial treatment of background and of some of the issues of interpretation implicit in it. The Dutch system of quick publication of the doctoral dissertation, admirable in many ways, simply is not adequate for a piece of research that opens up as much new territory as this one does. As it stands, the book is sprinkled with *obiter dicta* and with inadequate summaries of policies and events. It needs coherent background expositions of the author's understanding of the nature of Sinhalese society and culture and of the main stages of Dutch policy in politics and commerce as well as in education. Many aspects of the Dutch effort can be very fruitfully compared with Roman Catholic missionary and educational policies in Asia, but Van Goor has done very little in that line. This is a premature publication of exciting research that will be of interest to every student of European empire in any part of maritime Asia.

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WESTON BATE. *Lucky City: The First Generation at Ballarat, 1851-1901*. Carlton, Victoria: Melbourne Uni-

versity Press; distributed by ISBS, Forest Grove, Oreg. 1978. Pp. xv, 302. \$20.00.

Few permanent cities grew out of the gold fields that so captured the minds and imaginations of the nineteenth century. But one that did endure was Australia's Ballarat; and, although it is no Johannesburg or Denver, it is a sturdy provincial city with an individuality and tradition all its own. That Ballarat did not become a ghost town, as so many other gold rush sites did, was due to a variety of factors: the continuing richness of the gold yield, the town's proximity to a rich and developing agricultural region, and a willingness on the part of those made wealthy by the mines to invest in local industrial production and later to enrich the community life by their philanthropy. Both Anthony Trollope and Mark Twain found much that was agreeable during their brief visits to the town.

Weston Bate has told the story of the first generation in Ballarat, a rather long-lived generation by some standards, since he carries the account down to the turn of the century. His work is essentially a municipal biography; the overwhelming bulk of the author's sources are literary, with a heavy reliance on the Ballarat press of the day. Adherents of the newer approaches to urban history will find the work old-fashioned. And, for all the fullness of the text, there are some odd omissions. Despite the dependence of the town on the local mines for many decades, there is little discussion of changes in mining technology; metallurgy is not even listed as a topic in the index. Nor does the author seem concerned about the health problems related to mining, although public sanitation is the central matter in his discussion of the general health of the community.

Despite such deficiencies and the rather graceless literary style, *Lucky City* is a commendable addition to the growing number of works on Australian urban history. Melbourne University Press has made a handsome book of it, with a lavish number of illustrations.

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UNITED STATES

JOHN DEMOS and SARANE SPENCE BOOCOCK, editors. *Turning Points: Historical and Sociological Essays on the Family*. (*American Journal of Sociology*, volume 84, supplement 1978.) Chicago: University of Chicago Press. 1978. Pp. xi, 413. Cloth \$20.00, paper \$10.00.

Several years ago, Daniel Scott Smith proposed that "a systematic history of the American family can be

reconstructed if sociological theory, long-run series of quantitative data, and historical imagination in devising subtle measures of change are combined." *Turning Points* advances research in family history in the direction Smith suggested. Conceived in 1975 by the Russell Sage Foundation Program on Age, the book benefited from extensive exchanges among its authors and editors (a historian and a sociologist) as well as outside review before its publication as a supplement to the *American Journal of Sociology*. The collection of essays is a valuable contribution, not simply for its theoretical insights and substantive findings, but for its demonstration of the fruitful ways that interdisciplinary collaboration can improve historical scholarship as it enhances non-specialists' familiarity with historians' materials and methods.

Turning Points has four parts. In the first section, Glen H. Elder, Jr., offers a critical appraisal of pivotal sociological inquiries into the family and identifies conceptual and methodological issues confronting scholars who investigate family and social change over time. The historians' essays in the second section emphasize transitions to and from the family. Carl Kaestle and Maris Vinovskis, employing nineteenth-century Massachusetts census data and school records, analyze shifts in the manner families and society dealt with children from ages three to six. Michael Katz and Ian Davey's comments on the cultural-historical origins of adolescence, parent-child relations, and the social consequences of public education rest on evidence from a larger case study of Hamilton, Ontario, between 1851 and 1891. Adopting a life-course approach, John Modell, Frank Furstenberg, and Douglas Strong persuasively argue that the timing of first marriages, and its relation to other passages to adulthood, did not change significantly from the late 1800s until 1940. Tamara Hareven provides the book's most provocative and impressive essay, an examination of the dynamic and multi-faceted role of kinship among French-Canadian immigrant textile workers in Manchester, New Hampshire, from 1880 to 1936.

Attention in the third section turns from studies of overt behavior to explorations of certain norms defining or prescribing appropriate age and sex roles in the past. Joseph F. Kett traces the connotations of "precocity," focusing on the ramifications of commentators' animosity to adult behavior in adolescents between 1830 and 1930. Carroll Smith-Rosenberg offers an intriguing ethno-historical analysis of Jacksonian America as she relates Victorian sexual repressions, phobias, and fantasies to broader conditions in society and the family. In the most compelling analysis of old age in early America currently available, John Demos contends that the position of the elderly in colonial New Eng-

land was sociologically favorable but psychologically disadvantageous.

The sociologists' commentaries in the last section should be read by all social historians. The essays by Rosabeth Moss Kanter and by Neil Smelser and Sydney Halpern place the interrelationships among individual life histories, family processes, and structural changes in education and the economy into broader theoretical perspective. Anne Foner applies an age stratification model to make stunning inferences about age relations in nineteenth- and twentieth-century families, thereby elucidating key changes in family patterns. Sarane Spence Boocock presents a judicious critique of the strengths and weaknesses in the seven historical essays and useful suggestions for alternative research designs in future studies.

W. ANDREW ACHENBAUM
Canisius College

BARBARA J. HARRIS. *Beyond Her Sphere: Women and the Professions in American History*. (Contributions in Women's Studies, number 4.) Westport, Conn.: Greenwood Press. 1978. Pp. x, 212. \$15.95.

The core of the argument in Barbara J. Harris's new book is that traditional assumptions about woman's intellectual inferiority and domestic obligations have restricted her career opportunities. There is nothing especially novel about this theme and the book is frankly intended to serve as an introduction to the field. It does provide an intelligent review of the secondary literature on this ideology, but it does not go beyond what the informed reader will already know.

Harris first traces the development of the "cult of inferiority" from its origins in pre-modern Europe through its transplantation to the American colonies. Moving through four hundred years of history in twelve pages she must be highly general and so we read that "traditional misogynous ideas were thus part of the ideological inheritance of the new nation and constituted one of the major obstacles to females who openly expressed professional aspirations for themselves or their sex early in the nineteenth century" (p. 22). The "cult of domesticity" in the nineteenth century built upon these earlier notions and again restricted female career advancement in England and America.

Believing that these ideologies effectively deterred most women from leaving home, Harris focuses her next chapters on the few American women who rebelled, those who established female colleges (to challenge the notions of the cult of inferiority) or who entered professions such as medicine or law (to challenge the assumptions of the cult of domesticity). But despite these efforts the ideology persisted,

at least through the 1960s. Harris's two concluding chapters treat post-World War II America, particularly the increase of women workers and the growth of a new feminist ideology. Curiously, she makes few links back to her own material. Although her book provides some analysis of the current situation of professional women, she does not explore why so persistent an ideology finally lapsed.

Harris wants her historical analysis to prod women today to action, to encourage them to continue the fight begun by their "noble predecessors" (p. 191). But this approach is really of little help, either to history or to social policy. By focusing on the ability of a few women to overcome barriers, Harris becomes a woman's counterpart to the efforts of immigrant historians to glorify the achievements of a few newcomers. These historians also celebrated those who made it—and by implication suggested that those who failed had themselves to blame. In this same spirit, Harris's glorification of the rebellious women limits the possibilities for a deeper analysis, a more structural approach to the barriers that professional women confronted. She excluded lower-class women from her book as "peripheral to my subject" (p. x); ostensibly, they would be more affected by external circumstances. Perhaps in that way Harris thought she could avoid analyzing external, social considerations—as though middle-class women were in control of their fate. But such an argument will not do, and the history of the rebellious few makes this altogether apparent.

First, the earlier leaders were not nearly as rebellious as Harris would have it. She relates the establishment of a profession such as nursing to the work of a handful of courageous women to expand opportunities for their sex. But the development of this profession (like the expansion of teaching and the rise of office work, both of which occurred at the same time) demonstrates the power of stereotypic notions to keep women in their proper place even when they left the home. Harris cannot take note of the dynamics that led to the sex-stereotyping of woman's work. Her analysis neglects to consider that positions which became women's positions were the ones than men with the same training would not assume. In effect, she gives her rebellious few too much credit and underestimates the dead-end character of the posts that women occupied, even when they were outside the factory. Not surprisingly, the rebellious few who did manage to enter a male profession had little impact on the norms of the profession. Harris's volume contains statistics demonstrating how women now are crowded into a few jobs and how they suffer from unequal pay. But none of this prompts Harris to consider underlying causes, to ponder whether heroic types are sufficient to the task of promoting social change.

In the end, it is not enough to encourage women

to "take heart from the experiences of the heroic women who opened higher education, law . . . and countless other opportunities to members of their sex. . . . The debt should be repaid by continuing the crusade to build a society founded on sexual equality." We need, in addition, an understanding of the dynamics that operate within social institutions so that the expansion of opportunity for some will become synonymous with the equality of opportunity for all.

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H. ARNOLD BARTON. *The Search for Ancestors: A Swedish-American Family Saga*. Carbondale: Southern Illinois University Press. 1979. Pp. xi, 178. \$11.95.

Historians who view genealogy with condescension would do well to examine this model combination of history and genealogy written by H. Arnold Barton, the editor of a volume of immigrant letters, *Letters from the Promised Land: Swedes in America, 1840-1914* (1975) and editor of the *Swedish Pioneer Historical Quarterly*. With exhaustive research in Sweden and America, including extensive interviewing, and with a background in Scandinavian history, the author gracefully traces his family back into the sixteenth century, placing each generation in the context of the historical developments of its time through to the time of emigration in mid-nineteenth century and thence to the many settling places on the American continent.

The place of the author's family origins may be familiar to those who have read the novels of Vilhelm Moberg, who came from those same rocky uplands of southern Småland in Sweden. Although not all of the author's ancestors came from this area, the main lines seem to have stemmed from the parishes of Södra Vi, Djursdala, and Odensvi. One line is traced back to 1538, but the more useful records are from the eighteenth century forward. One gets an impression of a good deal of movement of persons from parish to parish and from one class or occupation to another, together with much cross-fertilization of stock. As shown later in the volume, not until the third generation and after did Swedes intermarry with persons of other groups to any extent. The circumstances of the growing emigration after 1840 are clearly indicated.

The familiar story of the Atlantic crossing is sketched for the author's families, but the major part of the latter half of the volume deals with the process of dispersal and acculturation in the United States. The varying degrees of acculturation are brought out with some indication of survival of interest in ethnicity. In an appendix on "Sources and Problems" the author explains his methodology and

the special problems involved in this type of research. Anyone entering this field, regardless of ethnic group, would benefit from this essay. There is detailed documentation for each chapter, a good index, and some illustrations. One or two maps would have been helpful.

CARLTON C. QUALEY
Minnesota Historical Society

DAVID MALDWIN ELLIS. *New York: State and City*. Ithaca: Cornell University Press. 1979. Pp. xi, 256. \$9.95.

For those who desire a brief treatment of New York State that encapsulates the entire timespan of the region's development, this book will fulfill many expectations. David Maldwyn Ellis's objectives are mainly to elucidate significant themes. Written in rapidly moving, almost newsmagazine-style prose, this survey devotes perhaps one-third of its space to the pre-1825 period and the balance to the subsequent century and a half. The author strives to interconnect politics, economics, and culture and is most successful in the three chronological chapters to 1825. There are separate chapters on the state's ethnic groups, character, economy, culture, political parties from 1825 to 1977, and the upstate-downstate rift.

A unique combination of abundant natural resources, geographic location, and the character of the multiethnic population have affected the state's most notable achievements. An abundance of land in the preindustrial period lured large numbers of migrants whose productivity sustained the drive for commercial leadership by New York City's merchants. Later, industrialization and urbanization afforded jobs for those who forsook their roots for New York. Integral to this process, triggered and bolstered by revolutionary changes in transportation technology, was the state's access to our west and the world. Although old Yorkers and transplanted Yankees predominated numerically, morally, and socially prior to the Civil War, subsequent population migrations restructured the demographic base, imparting new force and direction to socioeconomic and political alterations. These alien residents won ultimate acceptance and nourished the improvement of the general welfare.

Ethnic mix and an ability to discover means "to accommodate change" (p. 24) rank among the important characteristics of a citizenry whose headlong pursuit of materialism had its negative aspects. Still, Ellis awards the state high marks for a receptivity to innovation in several spheres that offsets "crassness" and occasional "tribalism."

Another theme, the upstate-downstate discord, is an ineluctable topic. The author duly touches major historical issues (for example, home rule, control

of the legislature, and distribution of state fiscal revenues), but his explanation of the hostility is insufficient because the problem's roots have permeated almost all facets of life. Ellis attributes the causes of the phenomenon to racial, religious, cultural, and political antagonisms, all of which indeed have played a role, but, for example, these elements have changed over the last century, a broad range of economic and social interest groups have manipulated them, fearful perceptions of a reordering of family and sexual relationships have taken their toll, and the abysmal failure of our institutions to inculcate civility and a decent respect for the humanity of others have exacerbated them. Amid the stresses of industrial and urban life, New York City deviated sharply from upstate-conceived conformity and so became the sacrificial lamb, despite Ellis's contention that the urbanites had their own contemptuous attitudes toward the upstate "benighted yokels" (p. 198). This poisonous situation, however, is not a matter for the impartial distribution of responsibility between both regions; rather, it signifies a depressing, long-term bankruptcy of the state's leadership.

One of the problems with "panoramic overviews" is that some readers have a different conception of the panorama. Undergirding familiar notions of industrialization, urbanization, and ethnicity, among others, the author has assumed the desirability of growth and society's ability to cope equitably with its strains. Nevertheless, the book is murky on the nature of preindustrial society, on the process, costs, and profound social impact of industrialization or "modernization." Moreover, the scant references to family relationships, feminism, the Great Depression, the politics of the thirties and forties, the infection of McCarthyism, Vietnam, and environmental and nuclear-energy perils are serious deficiencies.

An assessment of Ellis's accomplishment must be two-fold. This volume will please those who want a descriptive sketch of the more traditional thoroughfares of this state's history. For those seeking an explanatory narrative that attempts to account for the state's growth along the lines indicated above, *New York: State and City* will be disappointing.

BERNARD MASON
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THEDA PERDUE. *Slavery and the Evolution of Cherokee Society, 1540-1866*. Knoxville: University of Tennessee Press. 1979. Pp. xiv, 207. \$12.50.

Cherokee cultural adaptation to the dominant white society and the resulting factionalism between traditionalists and progressives has been explored and explained numerous times with varying

interpretations. Economic self-interest, internal political power struggles, and ideology are popular themes for explaining the phenomenon. Theda Perdue contends that slavery played a larger role in this change and struggle than has been supposed and that it "helped shape the economic class structure and conflicting value systems which produced the persistent factionalism" (p. 144). Perdue's thesis is an intriguing one, but at times her approach is frustrating. Frequently, too much space is allotted to surveying the familiar story of Cherokee society without investigating the impact of slavery on the tribe or the internal workings of the slave system.

The Cherokees practiced aboriginal bondage but with great dissimilarities to the system developed by whites. Slavery among the ancient Cherokees was not based on the economic rewards of owning slaves, nor was status attained in owning bondsmen. Indeed, slaves who were not adopted into the kinship system were practically valueless. The adoption of and desire for European manufactured goods changed the nature of Indian slavery. This period of adaptation saw the large and powerful Cherokees make war on lesser tribes in order to gain captives for the slave trade. Here then began a singular change in traditional concepts. Warfare in this new situation was not based on revenge or restitution but on obtaining captives for barter. Slaves now had a definite economic value.

The Cherokees soon discovered that trade in black slaves was much more profitable and less costly than tribal warfare. Tribal members became adept slave-stealers and traders during the eighteenth century. Plantation slavery "developed only after the alteration of . . . the kinship system, the division of labor, and the political system" (p. 50). By the 1820s and after the development of an elaborate legal system, slave codes and restrictions on Negroes became an integral part of Cherokee society. Perdue found no hint of desertion on the part of slaves, much less any insurrection of the blacks, until after removal. She couples the outbreak of such incidents to the disruption of Cherokee society under removal. Perdue explains the increasing severity of the Cherokee slave codes in the West as a surrender to "white" supremacy and a growing racism.

Readers will probably be most interested in the chapter on "Masters and Slaves," hoping to discover there how the slavery system worked among the highly acculturated Cherokees. One especially wants to know about the basic relationships between the owners and their bondsmen. While expressing a belief that the institution was generally a lenient one, the author gets diverted on peripheral issues, and the chapter is weakened and becomes somewhat disappointing. Why, for instance, was valuable space assigned to scanning Cherokee wealth in nonagricultural pursuits, such as steam-

boating, without linking such activities to slavery? Perhaps the sources are not adequate to answer the more difficult questions on the red man's relations with his black bondsmen. Outside of the WPA interviews of the 1930s in the "Indian-Pioneer History," no extensive body of material exists. Perdue, however, did make full use of the available resources and has compiled an impressive bibliography.

Although the first chapters are stronger than the latter ones, the work as a whole exceeds any previous study in this area. The author may certainly be complimented on her scholarship and writing style. Given the nature of the topic and the book's merits, the work should find a place on any library shelf and in private collections as well.

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PATRICIA K. OURADA. *The Menominee Indians: A History*. (Civilization of the American Indian Series, number 146.) Norman: University of Oklahoma Press. 1979. Pp. xx, 274. \$14.95.

Peacefully inclined Indian groups complained in the nineteenth century that the United States government neglected them in favor of more warlike tribes. Somewhat the same charge can be leveled against historians: warrior societies, especially those from the Great Plains, have received more attention than the relatively inconspicuous tribes. Happily, in recent years historians have been redressing the balance and giving attention to previously neglected groups.

The Menominees, though willing and able to defend themselves, never fought a major war against the United States. Miraculously, they were allowed to retain a portion of their aboriginal territory as their reservation. So successfully did they appear to be coping with the problems of acculturation that they were among the first tribes chosen for the termination of government services in the 1950s. The disastrous aftermath of termination—and the subsequent restoration of their former status—brought them to public attention and presumably led to the writing of this full-scale tribal history.

Patricia K. Ourada's history of the Menominees departs from the usual practice of ending somewhere about 1900 and carries their story through 1975, when the tribe's relationship to the government was restored. In fact, more than a fifth of the book deals with the twentieth century. The tribe's earlier history is not ignored, however. After a rather thin survey of Menominee culture, Ourada traces their successive relations with the French, the

British, and the Americans and details the gradual reduction of Menominee territory and the pressures for acculturation in the nineteenth century. In a chapter on the early twentieth century she attempts to show how the tribe progressed economically to a point where some of its own members and many outsiders believed that it could get along without further protection and supervision by the Indian Bureau.

There is a limit to what a book can accomplish in less than 225 pages of text, however, and the reader may have a sense of being hurried from one historical period to the next, without acquiring any real familiarity with any of them. Plenty of facts are presented, but rarely does the author explain their significance or their relationship to one another. The result is a certain chopppiness, less evident in the chapters dealing with recent events than elsewhere.

The book is also marred by carelessness of diction—as when "extract" is used for "exact" (p. 37) and "forestalling" for "postponing" (p. 209). The index is adequate but by no means complete; for example, no entries are provided for Stockbridge, Munsee, or Oneida, three tribal groups whose reservations were carved out of Menominee land. The two maps are too small to be of much value, and many geographic names mentioned in the text do not appear on them.

Despite these faults, *The Menominee Indians* is a worthy addition to the University of Oklahoma's Civilization of the American Indian Series. It makes a unique contribution as the case history of a tribe that underwent termination—and was able to reverse what nearly everyone assumed was the inevitable course of history.

ROY W. MEYER
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GEORGE PIERRE CASTILE. *North American Indians: An Introduction to the Chichimeca*. New York: McGraw-Hill. 1979. Pp. xiii, 314. \$12.95.

Do not be misled by the title of this book. Although the word "Chichimeca" is usually applied only to the tribes on Mexico's northern border, George Pierre Castile uses the term to refer to virtually all of the Indians of North America. The book is designed as a college text for courses on the Indian in America. Although such courses were formerly based on anthropology alone and fixed in the "ethnographic present," they are increasingly rooted in a historical framework. Yet they have remained in anthropology departments and have been taught by anthropologists. The present book, which is organized in a chronological manner and divided into pre-European contact and postcontact halves, is more historically based than previous essays in In-

dian history by anthropologists. At the same time each chapter is keyed to a specific cultural topic.

Castile writes in a breezy style, peppering his book with catchy section headings. He thus sugarcoats some of the heavy anthropological jargon that is *de rigueur* among anthropologists. But the sugar coating is not bastardization or oversimplification. It is good and effective popularization. Other virtues of the book are Castile's healthy skepticism concerning some of the "metaphysical wrangles of anthropological theory" (p. 61), his use of pungent analogies from non-Indian cultures (for example, European feudal relationships to help explain Indian kin, rank, and class relationships), his refusal to take the fashionable ideological bias of glorifying contemporary Indian radicals and condemning elected tribal leaders, and his refreshing modesty in admitting his inability to determine the answer to "the Indian problem."

In sum, this is a refreshing, erudite, and witty account of the American Indian from twenty thousand years ago to the present. While anthropologists have a lot to learn from historians, historians probably have more to learn from anthropologists in this as in other fields of history into which anthropologists are moving. If readers are not misled by the title, this book should find a solid place in college curricula dealing with the American Indian.

WILCOMB E. WASHBURN
Smithsonian Institution

R. DAVID EDMUNDS. *The Potawatomi: Keepers of the Fire*. (Civilization of the American Indian Series.) Norman: University of Oklahoma Press. 1978. Pp. xii, 367. \$19.95.

The University of Oklahoma Press Civilization of the American Indian Series has, as one of its most recent volumes, this excellent tribal history of the Potawatomi. R. David Edmunds has given us a comprehensive, heavily annotated account of these people and their white contacts from the seventeenth century to the era of removal in the 1830s. The story of the Potawatomi is complex, involving a number of separate bands, some of them difficult to identify as Potawatomi. Indeed, as Edmunds points out, the Potawatomi and the Chippewas were believed to have been one tribe by early white observers in the seventeenth century. And throughout the whole colonial period the Potawatomi were often close allies and neighbors of other Algonquian woodland peoples, including the Ottawas, the Winnebagos, the Miamis, the Sacs, the Foxes, the Kickapoos, and others. In the flood of detailed narrative, however, Edmunds identifies bands that were clearly Potawatomi, those of Detroit and St. Jo-

seph, those living along the Wabash and Illinois Rivers, others who lived among the Hurons, and those called the Prairie and Woods Potawatomi.

These people, as well as their Algonquian neighbors, were excellent farmers whose fields were planted with corn, beans, squash, melons, pumpkins, and tobacco. Potawatomi women harvested a bonanza of wilderness crops, particularly wild rice, roots, berries, and maple sugar. Surplus food was stored in clay pots, skin bags, and baskets. And this rich diet was supplemented by fish and meat. Potawatomi woodland craftsmen made superb dugout and birchbark canoes, arched-roof bark houses for summer, and wigwams for winter.

This Potawatomi woodland lifestyle was, of course, gradually altered with the coming of the whites. Edmunds continually stresses the trade motive in his history of Potawatomi-white relations, which began with exchange of surplus corn with the French for weapons, tools, and liquor. Eventually the Potawatomi, whose history is closely identified with those major tribes of the Great Lakes, were dispossessed, their chiefs bribed with expensive gifts to sign away a landed heritage. There was a familiar pattern of Indian resistance, defeat, and an impact of disease usually coming along with the white man's missionaries and Indian agents.

In the early years of white contact the Potawatomi established friendly relations with the French. Some of them were converted by Jesuit missionaries. They traded with LaSalle and fought with Frontenac against the Senecas. According to the author they were a key factor in the French victory over Braddock. Later they aided Pontiac in his rebellion, and they were among the Indians who warred against William Henry Harrison. At the Treaty of Fort Wayne, the Potawatomi and their Indian allies signed away some three million acres. Here, then, is a story of war, treaties, defeat, and removal. Edmunds's complex narrative is essentially the history of a tribe that was gradually expelled from a vast territory in the Northeast and upper Middle West.

The full force of the American frontier advance was not to be denied. Not without justification the Potawatomi complained that "the plowshare is driven through our tents." As they came under the control of the American government in the nineteenth century, they were forced to submit to the will of their conquerors. Though they valued blacksmiths rather than missionaries and preferred Catholic rather than Baptist missionaries, their wishes were usually overlooked. Their forced removal westward to Kansas was, as Edmunds states, "plagued by hardship."

Edmunds's summary commentaries state that the removal with its hardship was also marked by "fraud and chaotic planning" and that removal

treaties "were fraught with criminality." Certainly, the University of Oklahoma Press is to be congratulated in giving us an authoritative account of this chilling part of our national history. Edmunds's thorough research and his evocative book are truly significant contributions to American Indian history and Indian-white relations.

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BRUCE G. TRIGGER, editor. *Handbook of North American Indians*. Volume 15, *Northeast*. Washington: Smithsonian Institution. 1978. Pp. xvi, 924. \$14.50.

This volume, the second to be published of the twenty-volume *Handbook of North American Indians* (General Editor, William C. Sturtevant), covers the history, cultural background, and present circumstances of the Indian peoples of the northeastern United States and southeastern Canada. Supported by federal appropriations to the Smithsonian Institution, partly through its Bicentennial Programs, the series is intended to supersede F. W. Hodge, ed., *Handbook of American Indians North of Mexico* (2 vols., 1907-10), which has been a standard reference work for more than half a century.

The new *Handbook* differs significantly from the original one. It not only has greatly expanded and updated Hodge's *Handbook* by fully incorporating the results of the research done during the past four or five decades but also has adopted a new approach. The seventy-three refereed articles, written by forty-seven anthropologists, five historians, one sociologist, and one geographer, are arranged not alphabetically but topically in four major categories: general prehistory, the Coastal region, the Saint Lawrence Lowlands, and the Great Lakes-Riverine area.

The volume has succeeded in accomplishing two apparently contradictory objectives. While the work is basically a reference encyclopedia, providing a well-balanced summary of each topic suitable for general readers, it nevertheless maintains high professional standards of theory, method, and accuracy. The essays read as a series of scholarly monographs. There is a remarkable evenness in the quality of so diverse articles, and Bruce G. Trigger's introduction and concluding chapter, which categorizes the Northeastern Indian cultures into six major patterns, serve to unify the volume.

The *Handbook* consists of both topical and tribal essays. Among the noteworthy topical essays are Brasser's vivid discussion of early Indian-European contacts, Washburn's concise and crisp chapter on the seventeenth-century Indian wars, Fenton's illu-

minating "Northern Iroquoian Culture Patterns," Trigger's comprehensive article on early Iroquoian contacts with Europeans, Tooker's perceptive essay on the League of the Iroquois, and Wallace's "Origins of the Longhouse Religion," an excellent synthesis of his earlier works.

The tribal chapters provide detailed information on each of the Indian groups. Repeatedly, the complexity of Indian history is recounted: major geographical dislocations and the formation of larger tribal units in response to early European contacts, their disintegration as a result of growing white pressures, and the recombination of refugees to form new, heterogeneous, and often resilient groups. These chapters are frequently repetitive, due to many common characteristics among all the Northeastern tribes, such as division of labor, dual chieftainship, customs regarding birth, child-rearing, funerals, mourning, and menstruation seclusion and other sexual practices. The tribes within one region also shared the same cultural pattern. The contributors, however, have made conscientious efforts to single out the unique character of each tribe. The tribal essays include Jennings's brief but penetrating "Susquehannock," Heidenreich's rich, authoritative article on the Huron, Sturtevant's short but revealing essay on the Oklahoma Seneca-Cayuga, Lurie's balanced history of the Winnebago, and Clifton's solid "Potawatomi."

The format follows the style of the *American Anthropologist*. References appear in the body of the text with a unified bibliography at the end of the volume. "Synonymy" in each tribal chapter and a brief bibliographical essay at the end of each chapter are highly useful. Illustrations include numerous photographs, maps, drawings, statistical tables, and charts hitherto not printed. The encyclopedic value of the volume could have been enhanced if the index, though extensive, were more thorough; items like crimes, dreams, intermarriage, and interpreters should have had separate entries, while some existing entries like adoption and gift exchange could have been expanded.

In sum, this is a superb work that will become a standard reference book for many decades. The volume, having a strong anthropological approach, should be particularly valuable to historians in broadening their interdisciplinary perspective. The articles, written in full sympathy with and understanding of the Indian cultures, will also have significant effects on living Indian people and their relations with the larger society. If the rest of the series, scheduled to be out soon, lives up to the same standard, the *Handbook* will indeed be a major and most impressive achievement of our generation.

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ROBERT KELLEY. *The Cultural Pattern in American Politics: The First Century*. New York: Alfred A. Knopf. 1979. Pp. xiv, 368. \$15.00.

This first installment of a two-volume overview of American political history demands the attention of all political historians. Robert Kelley has audaciously undertaken to synthesize recent scholarship emphasizing ethnocultural determinants of voting behavior with the older scholarship emphasizing economic issues. It is an impressive effort, enriched by the transatlantic comparative perspective developed in Kelley's previous work. He has digested practically everything useful in the enormous literature. Though acknowledging a "tilt toward cultural explanations" (p. 14), he has consistently sought to do justice to alternative interpretations.

The result gives us a more comprehensive appreciation of ethnocultural forces: the central role of Scots-Irish Presbyterians in the coming of the American Revolution in the Middle Colonies; the appeal of the Jacksonian Democratic party for cultural "outsiders" like Irish Catholics in the North, and of the Whig party for cultural "outsiders" like French Cajuns and Appalachian mountaineers in the South.

There are, however, many difficulties. In some crucial cases Kelley's "tilt toward cultural explanations" causes him to push cultural explanations further than I find convincing, as when he sees the American Revolution primarily as a conflict between Anglicizers and the "outgroups" of British-American society—Dissenters, Scots, Irish, and Welsh. In other cases, the argument is bedeviled by internal contradictions. For much of the nineteenth century, Kelley's broadest cultural categories are New England Yankees and Southerners, the former putting their stamp on the Whig and Republican parties and the latter on the Democrats. Yet he has to acknowledge that New Hampshire and Maine were banner Democratic states in the Jacksonian years, while Whiggish "outsiders" divided the South almost equally with the Democrats.

The root problem is theoretical and conceptual. Kelley divides political impulses into economic and cultural, with sometimes one and sometimes the other playing the dominant role. His concept of culture "refers to the realm of *consciousness*, as distinct from that of the material world and its demands" (p. 10), and thus includes intellectual and ideological factors. So far so good. Yet he waffles on the crucial question of the relationship between the cultural and the material, proclaiming culture to be "independent of (though in interaction with) the material setting" (p. 11), whatever that may mean.

Certainly the interaction between the two remains unexplored. But if consciousness is shaped by

material circumstances (as, among others, Lee Benson has argued in the "Postscript" to his *Turner and Beard* [1960]), the case for an independent cultural factor becomes tenuous indeed. Thus Ronald P. Formisano has pointed out, with reference to Kelley's cultural Southerners, that "from a territorial and economic base Southernness became a cultural difference" (*AHR*, 82 [1977]: 572).

By taking cultural differences as "givens" and failing to explore their linkages with material circumstances, Kelley has finessed the central problem of political historiography. That is why his account, rich and valuable though it is, misses so much that lies at the heart of our history: the material distinctions that made egalitarian and democratic impulses so dynamic a part of Jeffersonian and Jacksonian politics; class situation as a shaper of the cultural consciousness of Irish workers in Jacksonian Philadelphia or Brahmin entrepreneurs in Boston; and in general the different responses of various groups to industrialization and urbanization as mediated through the lens of culture.

CHARLES SELLERS
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PHILIP J. SCHWARZ. *The Jarring Interests: New York's Boundary Makers, 1664-1776*. Albany: State University of New York Press. 1979. Pp. xvi, 343. \$40.00.

The history of intercolonial boundary disputes in America has been told and retold as part of the larger subject of intercolonial relations within the British Empire. Focusing on New York, several scholars in recent years—Patricia Bonomi, Michael Kammen, Sung Bok Kim—have considered its territorial controversies in their studies on related subjects; now, for the first time, New York's "jarring interests" of this nature appear in full detail in Philip J. Schwarz's book. If "so much tedious detail seems to be involved," to quote from the preface, his admixture of cultural, economic, and social history gives due emphasis to the *dramatis personae* involved in "the politics of interest," and he includes portraits of some who played major roles—Cadwalader Colden, Robert Livingston, Jr., and William Smith, Jr.

New York, bordering on five other colonies, provides the best vantage point from which to narrate and interpret the complex issues of the perennial boundary controversies. New York's status as a continuous royal colony gave it some advantage over chartered and proprietary colonies in its close ties with the Stuarts in the seventeenth century and its traditional loyalty when territorial disputes were taken in hand by the crown. As Schwarz points out, the issues derived basically from local occupation

and ownership of land, contested by physical violence as well as polemics, but extended into the larger arena of territorial jurisdiction and administration. The pressure of increasing population exacerbated the issues, not confined to feuding settlers but rising to the provincial level where territorial claims were of primary concern and thence through colonial agents in London to ultimate royal authority.

The inhabitants in areas of conflict "revealed the power of the powerless," as the author aptly puts it, "by voting with their feet" (p. 98), in the manner of successive generations of frontiersmen. It was, for example, the land riots of 1766 that led eventually to the Hartford Agreement of 1773 on the boundary between New York and Massachusetts. Factionalism, characteristic of colonial New York's politics, with its powerbase in the landed aristocracy, inevitably thrived on intercolonial territorial disputes.

Periodic conflict in New York over its boundaries was a complex development. It is not the stuff that makes for easy reading, but Schwarz has established himself as the authority on the subject. Unfortunately his writing is marred by the current corruption of the language in converting nouns into processions of adjectives—"successful conflict resolution," "frontier defense funding problem," "interest harmonization"—for lack of a well-placed preposition. It is also disturbing to read that the Hudson River "flowed northnortheast rather than due north or northnorthwest" (p. 11), with, one can only conclude, grave historical consequences. Six maps, contemporaneous and later (some of which show the course of the Hudson), complement the text, although necessary photographic reductions have rendered some of them quite illegible.

LESTER J. CAPPON
Newberry Library

PETER N. CARROLL. *The Other Samuel Johnson: A Psychohistory of Early New England*. Cranbury, N.J.: Fairleigh Dickinson University Press. 1978. Pp. 247. \$16.50.

The other Samuel Johnson, a leading eighteenth-century Anglican cleric in Connecticut, is a worthy subject for a book-length biography. Although the colonial Johnson was a distant relative of the English lexicographer, his career was unconnected with that of his more illustrious kinsman. The other Samuel's claims to distinction result entirely from his efforts to expand the Church of England in a colony that was bitterly hostile to Anglicanism. His few successes and many failures as a missionary, the leadership he exercised as first president of King's College, and the reaction of his presbyterially inclined neighbors to his efforts all form a fascinating chapter in early American ecclesiastical history.

The book is noteworthy not only as a biography but as a psychohistory of early New England. The author's definition of psychohistory and his method of attack are best explained in his own words. From the wealth of surviving Johnson material, Peter N. Carroll hopes to "reconstruct an interaction between the man and his culture, . . . observe the formation of a mature personality and explore the subtle relationships between emotional, sometimes unconscious, feelings and more visible behavior" (p. 9). There is no need to question his procedures to that point. It is in compensating for the limited documentary material suitable for psychohistorical analysis that the author's techniques require examination. Again it is best to allow him to provide his own explanation. "In transcending these difficulties, the psychohistorian draws upon the fullness of his own life experience. . . . Many of the insights of this book . . . reflect not so much psychoanalytic theory as the personal experiences of the author" (p. 11).

Any historical work is influenced to some degree by the subjective judgments of the researcher. Yet substituting personal experience for adequate theoretical underpinning—particularly in psychohistory, where unconnected snippets of data are given coherence only by substantial and comprehensive theory—is dangerous. It creates a strong possibility the study will have no meaning beyond that generated in the relationship between biographer and subject. Still, Carroll's life of Samuel Johnson is not a meaningless study. It is a clear, concise, perceptive, and scholarly biography. The other Johnson's Boswell has achieved all this by avoiding many of the problems of his own methodology. He has produced an account in a generally traditional pattern and allowed psychology only an intrusive rather than an essential function. The resultant combination works well.

Carroll's stated wish is to have his study speak not so much about Johnson as about ourselves. He has not achieved this aim, but he has written an admirable monograph. If we have not learned much about ourselves, we have considerably enriched our knowledge of early Connecticut, eighteenth-century American Anglicanism, and one man's struggle, first with his own conscience and then with the consciences of his family, friends, and fellow colonists.

B. R. BURG

Arizona State University

CORNELIUS P. FORSTER. *The Uncontrolled Chancellor: Charles Townshend His American Policy*. Providence: Rhode Island Bicentennial Foundation. 1978. Pp. xv, 155. \$9.95.

The most overworked word in this volume is "probably," and the recurrent use of that modifier reflects

the difficulties a historian encounters in attempting to explain Charles Townshend. "Capricious" is an inadequate description of the man. He changed parties as often as he changed posts, and his correspondence alternately praises and condemns virtually everyone with whom he came in contact.

Cornelius P. Forster focuses on the last six years of Townshend's career, from the time he achieved ministerial rank as Secretary of War in 1761 until his death in 1767. In seven brief and generally well-written chapters, Forster explores Townshend's struggle for political power and his program for the colonies.

Forster argues that despite Townshend's "propensity to fluctuate" (p. xiii) he was consistent on two points. He unwaveringly pursued his own aggrandizement and he advocated a comprehensive imperial program to ensure the supremacy of the mother country. Townshend's decision to accept or reject a governmental position reflected his evaluation of a particular government's staying power and his desire for leadership in the House of Commons. With reference to the colonies, Townshend is pictured as working to free colonial officials from their dependence on local assemblies by paying them from funds raised through parliamentary taxation.

Although Forster has done extensive research in primary materials, he fails to present a clear picture of Charles Townshend. The major portion of the book, despite its title, is a tedious description of Townshend's quest for political power. To argue that there is consistency in pursuing one's own advancement is confusing at best; here it seems to mask confusion about what Townshend is doing or why. He appears now and then in a murky narrative of British politics, but we end up knowing little about him except that he is a brilliant speaker and a political trimmer. We rarely see Townshend attempting to justify his behavior or even to explain it. Moreover, his relationships with such major figures as Pitt, Newcastle, Rockingham—even his brother George—often appear inexplicable. He seems not simply unprincipled but erratic. Why?

If we assume that Townshend was consistent only in pursuing his own career, what explains his purported devotion to a single colonial policy? Were his proposals for America the result of political expediency or did he, in this one instance, have some deeper loyalty? Little attention is given to Townshend's colonial policy and much of what is discussed (the taxation acts of 1767) has been previously explored by Robert J. Chaffin. Forster asserts that Townshend had a "comprehensive imperial program" (p. xiii), but he does not give a clear overview of that program or attempt to explain Townshend's motivations. It might also be argued that Townshend's colonial policy was neither consistent nor comprehensive.

In short, Forster does not tell us much about Townshend, about British politics, or about American policy that we did not already know. The fault may well be in the intractability of the materials rather than in the author, but the book is a disappointment.

DAVID L. AMMERMAN
Florida State University

MARÍA PILAR RUIGÓMEZ DE HERNÁNDEZ. *El gobierno español del despotismo ilustrado ante la independencia de los Estados Unidos de América: Una nueva estructura de la política internacional (1773-1783)*. (Trabajos monográficos sobre la independencia de norteamérica, number 3.) Madrid: Ministerio de Asuntos Exteriores. 1978. Pp. 338.

María Pilar Ruigómez de Hernández has prepared a monograph on Spain and the American Revolution. Unlike previous studies that argued that Spain clearly attempted to help the British colonies out of some noble motive, this book looks at the broad nature of international affairs in the 1770s and early 1780s and the role Madrid and the colonies in North America played in that scheme of events. Thus the book is more of a general history of Spanish foreign policy of the period than a specific analysis of Spain's early relations with the United States.

The book is divided into two sections. The first deals with international political and economic rivalries, especially those among France, Britain, and Spain during the eighteenth century, both from an intellectual and political perspective. The second section has a number of chapters devoted to the American Revolution, involving Franco-Spanish negotiations, a section on Spanish-American contacts, the decision to help the colonies by Spain, and finally on the whole question of the western boundaries in North America. There is no concluding chapter.

The book explores more clearly than earlier studies the role of domestic events on the development of Spain's foreign policy and offers considerable detail regarding the role of individuals and the effect of their political philosophies on diplomacy. The author, however, says little that is new. Archival research is used to discuss Spanish-American relations and contacts with Paris, although most of this material has already been mined by other historians. Bibliographic references suggest that none of the literature on the general subject of late eighteenth-century diplomacy published after 1970 was consulted. The author argues, on the basis of research conducted, that the question of territorial boundaries in North America was extremely important to Madrid, more so than even Gibraltar, and that ri-

valry between Madrid and Paris was reflected in their mutual distrust and animosity toward London when dealing with the New World. Considerable attention is paid to the roles of Arandz and Florida-blanca in developing Spanish foreign policy, an issue that previous historians have glossed over.

The study is too simplistic in its arguments and is poorly researched. No French, British, or even Spanish colonial archives were consulted. The author has, however, provided a balanced view of Spanish foreign policy in the 1770s that will appeal to the serious general reader but not necessarily to the specialist of eighteenth-century diplomacy.

JAMES W. CORTADA
IBM Corporation

A. A. FURSENKO. *Amerikanskaia revoliutsiia i obrazovanie SShA* [The American Revolution and the Formation of the U.S.A.]. Edited by V. I. RUTENBURG. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otdelenie. 1978. Pp. 414. 1 r. 90 k.

A. A. Fursenko, a senior member of the growing band of Soviet Americanists, has already produced a series of works in the field devoted mainly to later periods but including *The American Bourgeois Revolution of the Eighteenth Century* (1960). This present work is more than a rehash of its predecessor, even though it falls into that category designated for Soviet readers as "popular-scientific,"—based exclusively on published sources and assuming no great prior knowledge of the subject. Fursenko has been able to make good use of a stay at the Woodrow Wilson International Center for Scholars to develop a close acquaintance with much recent writing on the subject. Thus, if there are inevitably references to the Old Left of Herbert Aptheker, Eric Foner, Philip Foner, and W. Z. Foster, there are also citations of the New Left of Staughton Lynd and especially of Jesse Lemisch. And Fursenko demonstrates wide reading among other schools of domestic scholarship on the American Revolution, not surprisingly finding more to agree with in the pages of Merrill Jensen and Jackson Turner Main than in those of Bernard Bailyn and Edmund S. Morgan. None of these scholars would be advised to seek a translation of the book unless they wish to ascertain that their views have not been misrepresented, for they would learn little if anything from it about the area of their expertise. Indeed, a warning might more generally be issued to all potential American readers, none of whom would be likely to need the sometimes very simple accounts of such milestones on the road to revolution as the Stamp Act crisis and the Townshend Acts.

Nevertheless, the book has some interest as a well-written presentation of the American Revolution

and the debates surrounding it simply because it has been achieved by a Soviet scholar. It clearly indicates that Soviet writing on its subject has come of age and that Soviet readers are no longer obliged to accept the crudities that were offered them just a few decades ago. True, they are warned off the "consensus" school of interpretation and are urged to see the inconsistency of those who simultaneously preach the exceptional and the universal nature of 1776 and its sequel. Moreover, they are discouraged from giving their acceptance to the Atlantic interpretation of the American Revolution, that is, from placing it in anything like the same category as the French. Yet these positions have all been taken up by at least some reputable American historians in recent years, and Fursenko is aligning himself with them rather than dismissing all non-Marxist American scholarship as "bourgeois falsification." In addition, there is in his work from time to time a spirit of enthusiasm for at least some moments in the great drama and for at least some of the actors. He appears genuinely stirred by the Boston Tea Party and the "shot heard around the world" as well as by Benjamin Franklin and Patrick Henry, in a manner that those brought up on such events and people might find difficult to recapture. At the end, however, and not surprisingly, Fursenko takes issue with Richard Morris about the relative significance of the American and Russian Revolutions.

PAUL DUKES
University of Aberdeen

GERARD W. GAWALT. *The Promise of Power: The Emergence of the Legal Profession in Massachusetts, 1760-1840*. (Contributions in Legal Studies, number 6.) Westport, Conn.: Greenwood Press. 1979. Pp. x, 254. \$19.95.

Whereas ministers were the dominant professionals in colonial America and doctors attained that eminence in our century, the nineteenth century belonged to lawyers. Gerard W. Gawalt traces the structural transformation of the Massachusetts legal profession as it rose to power in the early nineteenth century. He provides informative data on lawyers' social backgrounds, career patterns, wealth, income, and legal training and on the political struggles of leading lawyers to maintain professional autonomy.

Gawalt's interpretation of this data is very much in keeping with the recent work of legal historians Richard Ellis, Morton Horwitz, and William Nelson. According to Gawalt, the growing commercial economy after the American Revolution required lawyers' skills, thus stimulating a dramatic increase in the number of lawyers. Lawyers became competitive and individualistic, shedding the corporate-minded orientation they had before the Revolution.

Ironically, as the profession became less cohesive and more stratified, it became more powerful.

The Promise of Power also builds on the revisionism of Maxwell Bloomfield by providing a detailed refutation of the traditional view that the 1830s marked the nadir of professional development, when an uninformed egalitarian spirit undermined professional standards. Gawalt shows that the power of lawyers grew despite attacks by antilawyer and antielite reformers and despite the occasional passage of legislation designed to democratize the profession and undermine the power of legal institutions. As a refutation of earlier interpretations, Gawalt's study is convincing. He shows that, although formal bar admission standards were lowered in the 1830s, entry to the bar remained restricted by informal barriers. Also, leading lawyers were able to blunt or coopt most reforms. They were able to do so in part because the legal profession had established a virtual monopoly over the judiciary and had become a significant voice in the state legislature and politics generally.

As an exploration of the ramifications of lawyers' power, however, the study is less satisfying. A full analysis of power should explore more than lawyers' fees, access to public office, and control over training and over entry into the profession. How did lawyers perceive the role of law and their profession in the emerging commercially oriented society? Did they have a guild mentality, or did they think of themselves as atomistic individuals? Were they in any sense class conscious? How, for example, did they define their relationship with other professionals and with the new commercial elite? Gawalt provides suggestive bits and pieces of evidence about all these matters but explores none of them in depth. *The Promise of Power* should stimulate studies of other states. Hopefully, such studies will not only build on Gawalt's excellent base, but will also examine the culture of professionalism and the full implications and ramifications of lawyers' power.

WAYNE K. HOBSON
California State University,
Fullerton

WILLIAM L. VAN DEBURG. *The Slave Drivers: Black Agricultural Labor Supervisors in the Antebellum South*. (Contributions in Afro-American and African Studies, number 43.) Westport, Conn.: Greenwood Press. 1979. Pp. xvii, 202. \$16.95.

Rather than an analysis of drivers *per se*, this revisionist study concentrates on what various observers (historians, planters, travelers, exslaves) have said about them. An appendix provides an interesting and useful essay on the evidence. Thoroughly re-

searched in both primary and secondary sources, *The Slave Drivers* is the most thorough statement on the topic to date.

The historical reputation of black slave drivers (bondsmen who managed slave laborers) has suffered from neoabolitionist interpretations that stereotype drivers as barbarous half-savages so debased by slavery that they found sadistic pleasure in inflicting pain on their hapless brothers in bondage. William L. Van Deburg disputes this depravity thesis and argues that while the drivers may have served their owners' economic interests, it does not follow that they were insensitive to the needs of the slave community. Their responsibilities included directing labor in the fields, maintaining discipline, time- and account-keeping, guarding the crops, distributing weekly rations, and more. The author draws an analogy between drivers and modern shop foremen who must satisfy the employers' demands while articulating the concerns of the workers for whom they are responsible. He believes, however, that the driver "most often sides with 'labor' as opposed to 'management,'" and concludes that "the pressures of possessing both instrumental and expressive modes of leadership did not destroy the driver psychologically or turn him into a sadistic oppressor of his fellow bondsmen" (p. xiv).

Actually, there were no universally recognized traits that identified the slave driver. They were young and old, large and small, servile and fiercely independent, brutish and humane. While some flogged slaves without mercy, others assisted bondsmen by faking whippings, covertly distributing extra rations, fighting with violent overseers, and behaving in other ways that were decidedly not in the master's interest. In doing so, they "sought to preserve their integrity" by refusing to become "psychologically devastated brutes" (p. 115). The key to understanding these so-called "privileged" bondsmen lies in the realization that drivers had their roots in the slave quarters. They had stronger emotional bonds with fellow slaves than with masters and, as members of the slave community, were part of its family, religious, and social life. The core of their identity lay in the quarters, which provided them with the psychological protection to withstand the forces of dehumanization that might have rendered them into brutes.

If a reservation can be lodged against this book, it stems from the paucity of primary sources on slave drivers, which in turn forces the author to strain the evidence to support his thesis. The chapter dealing with the perceptions of travelers in the South, for example, is based on only six accounts. This is beyond Van Deburg's control, of course, for there probably are no caches of material on the drivers to be unearthed. Nevertheless, the author has used the available materials with dexterity and imagination

and has shed considerable light on a little-understood phase of slave life.

RONALE L. LEWIS
University of Delaware

ROBERT J. BRUGGER. *Beverley Tucker: Heart over Head in the Old South*. Baltimore: Johns Hopkins University Press. 1978. Pp. xvii, 294. \$15.00.

In this interesting biography of Nathaniel Beverley Tucker (1784–1851), Robert J. Brugger has recounted a life of unusual variety and chronicled opinions the more absorbing for being lost to us in this democratic age. Tucker was a man of Virginian habits and inclinations, opinions and prejudices. Allied to the Randolphs and himself the son of that distinguished gentleman and jurist St. George Tucker, Beverley was an excitable and ambitious young man who lacked discipline and good luck and never succeeded, after having begun in a dozen ways to make his mark, in making his mark stick. Brugger shows him pursuing his studies in a clamorous and diminishing Williamsburg, braving and failing at the practice of law in Southside, Virginia, and finding the conditions of life there increasingly irritating to his pride and mortifying to his hopes of independence and prosperity. Always shadowing this young man was the stern standard of his father—indeed, of that Founding generation that his father represented.

Like many another unlucky gentleman, Tucker “lit out” for Missouri, where he lived for many years prospering and at times gaining public esteem and judicial office. Among Tucker’s most intriguing projects in those years was his hopeful and nostalgic attempt to establish at Dardenne Creek a neighborhood of likeminded settlers, a “slaveholders’ Camelot.” Tucker and his slaveholding friends were to live a life of leisure and measured elevation and occupation, patriotic and distinguished. This vision of a nostalgic alternative to the contemporary democratic confusions appealed to and informed Tucker’s vision of the world around him. He looked for that gentility and order that would be his proper setting and that would accord him the deference and inner satisfaction he seems not to have gained from his family or from the world and that he continued to consider his due. Tucker was an unhappy man, dissatisfied and, as Brugger emphasizes, at ever-increasing distances from the commonly perceived reality of his countrymen.

Returning to Virginia in the wake of Nullification and the threat of Jackson’s nationalist fervor, Tucker succeeded to his father’s place as professor of law at William and Mary and became preceptor to a generation of Virginia law students and quarterly readers. In many respects he followed his ec-

centric half-brother John Randolph in his opinions and in his ambitions for an independent southern future modeled on the Virginian past. He wrote two novels, *George Balcombe* and *The Partisan Leader*, which sought, one pastorally and the other politically and apocalyptically, to make clear in fiction those standards and possibilities Yankee democratic life in America was obscuring. Tucker spent his last years dizzied and depressed by the hope of making his conservative-romantic and secessionist views count for practical purposes.

Brugger describes especially well Tucker’s frustrations as a young lawyer and his experiences as a Virginian in frontier Missouri. He presents Tucker’s ideas clearly and tries to give back to them some of the force the intervening and, from their point of view, calamitous history has taken from them. In one sense this is the story of a child of a Revolutionary father stymied and infuriated by the burdens of his inheritance. Brugger is careful not to strain our sense of Tucker’s importance. He presents a man whose opinions, and emotions, and experiences reflected those of his southern brethren. This is argued in an afterword, which, if it does not altogether establish Tucker’s representativeness, does show how certain of the opinions he advanced were common southern notions, although this sharing is noted rather than explored. For a book that presents itself as psychohistory, the discussion of Tucker’s character, personality, and family relations is less substantial than the rather elaborate methodological observations on the subject lead one to expect. The book’s tone is distant, as is its portrait of Tucker the man. In short, this book gives a good account of Beverley Tucker and rescues him from neglect.

ROBERT DAWIDOFF
Claremont Graduate School

VIRGINIA BERGMAN PETERS. *The Florida Wars*. Hamden, Conn.: Archon Books. 1979. Pp. 331. \$22.50.

Between 1810 and 1858 the United States fought a series of nasty little wars in Florida, which have been largely forgotten by Americans, and it is not difficult to understand why. These were wars that were fought between a large force of Americans, with great wealth and technological superiority, and a small force of fugitive Indians and blacks whose wealth and technology consisted of no more than what they could cultivate, scavenge, and steal. They were wars that were fought in a tropical area, then largely unknown to American scientists—an area that inflicted terrible punishment on the American military. They were wars in which a massive and expensive American military presence was made to look foolish by a handful of ethnic adver-

saries who fought so valiantly that many American soldiers came to admire them and said so. They were wars that, as they wore on and on, led to moral revulsion on the part of an important segment of American society.

The similarity between the Florida wars and the Vietnam war is obvious, and Virginia Bergman Peters touches on this similarity more than once. Apart from this, she has little to say about the Florida wars that has not already been said. Her main contribution in this book is that she pulls together, within the covers of a single volume, the entire, sad sequence of events. As such, her book is a readable synthesis of a fairly substantial literature.

It is a literature that is rich in the stuff of which novels are made. A heavily armed fort at the mouth of the Apalachicola River manned by escaped black slaves; a society composed of southern Indians and blacks whose loyalty to each other was so fierce that the American military had to batter them to a bloody remnant before any of them could be forced or persuaded to be informers or turncoats; a black interpreter, Abraham, who negotiated for the Indians with polished manners and "with a countenance which none can read" (p. 141); an Indian leader, Halleck-Tustenuggee, captured by the Americans under a white flag, accused by one of his men of *wanting* to be captured, who leaped up and knocked the man to the ground by striking both feet onto his chest, and then bit his ear off, ground it in his teeth like a mastiff, and spat it to the ground, shouting "Tustenuggee . . . Halleck-Tustenuggee!" (p. 247).

The shortcomings of Peters' book are the shortcomings of the previous scholarship on which it is based. Two fundamental questions beg for answers. How did the Indians and blacks maintain discipline and morale in the face of such terrible odds? That is, from what ideological wellsprings, from what social experience, and from what vision of the future did their actions proceed? And an even more fundamental question is how did this series of wars fit into the history of the American social and economic system? Why, in retrospect, do these wars, as does the Vietnam war, seem so grotesque, so seemingly aberrant? Such questions make it clear that what Frances Fitzgerald's *Fire in the Lake* did for the Vietnam war needs to be done for the Florida wars.

CHARLES HUDSON
University of Georgia

JOHN MACK FARAGHER. *Women and Men on the Overland Trail*. (Yale Historical Publications, Miscellany, number 121.) New Haven: Yale University Press. 1979. Pp. xiii, 281. \$17.50.

The title of this volume is misleading. This is *not* a study of the overland trail. Rather, it is a study of

"the relationship between men and women in marriage in the mid-nineteenth century Midwest" (p. 3). The stage on which John Mack Faragher examines this relationship is the overland trail, 1840-70. Although Faragher contends that "the trail experience was no more remarkable than ordinary family life and struggle," he also acknowledges that, "for most of the emigrants the trip was a spectacular event in their lives, unlike anything they had done before" (pp. 4, 11). And, as Faragher describes the difficulties of the journey, it is clear that the trail experience was unique and that it put unusual strains on family relationships. The key to this seeming contradiction is that Faragher is not so much interested in family relationships on the trail as he is in examining family life in a small segment of society at a particular time—the midwestern farm family just prior to the coming of modernization and commercialization. The overland trail is a convenience necessitated by the lack of adequate source material of the type Faragher needs, specifically, diaries by both men and women. Since midwesterners wrote little about daily farm life, Faragher is forced into the difficult position of trying to use the available descriptions of an extraordinary experience, the overland trail, to reflect what the relationships between these people must have been like before their journey.

In the course of this historical balancing act, Faragher falls into several contradictions and errors. Almost every sentence is loaded with value-laden words and phrases that depict women as "exploited," "subordinate," and "powerless." Writing from a Marxist orientation, Faragher defines all relationships in terms of the class, race, and sexual struggle, and within this struggle Faragher sees women as victims of a patriarchal society dominated by masculine attitudes of superiority (pp. 15, 62). To prove his points, Faragher makes statements that are not supported by his sources or are misleading as he uses them. For example, Faragher writes that trail women rose before the men to stoke the fires and milk the cows (p. 76). His support for this statement is a diary by Helen Carpenter that taken in context, says "some women" had these responsibilities and that most of those who did were "Missourians." Faragher dismisses this by saying that most women "were from Missouri and its midwestern environs" (p. 80). But "Missourian," as Faragher well knows, was a trail term used to designate a particular class of people, usually down-and-outers. If the evidence does not fit his preconceptions, Faragher simply discards it. On page 87 he uses a quote from Phoebe Judson to illustrate a "divergence of the sexes" and, in a neat piece of juggling, accepts her statement as to her emotional state but rejects her comments about her physical well-being.

Despite these flaws, there is much that is right about the book. Faragher's analysis of midwestern family life and his provocative use of new social science techniques raise exciting possibilities for further research. Faragher has borrowed liberally from social psychology, sociology, folklore, and demography. He is not very sophisticated in applying these new methodologies, but he has shown how they might be utilized in developing new approaches to the study of regional history. This book will excite a good deal of comment, and it should generate a number of useful new studies.

SANDRA L. MYRES
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THOMAS A. BRYSON. *An American Consular Officer in the Middle East in the Jacksonian Era: A Biography of William Brown Hodgson, 1801-1871*. Atlanta: Resurgens Publications. 1979. Pp. xiii, 214.

Thomas A. Bryson's biography of William Brown Hodgson is part of a growing body of scholarship about the early American experience in the Middle East. The precocious Hodgson was sent to Algiers as a language student by President John Quincy Adams in 1826 because the United States government was so devoid of expertise about the area that letters received in Arabic could not be translated. After a three-year apprenticeship under Consul William Shaler, Hodgson became one of that handful of individuals in nineteenth-century America who tried to pursue a professional career in an unprofessional foreign service, where place and advancement depended less upon merit than upon political connections. After working as a clerk-interpreter at the Department of State, in 1831 Hodgson became dragoman to the American legation at Constantinople. In 1833, however, he was unceremoniously fired by Commodore David Porter, the ill-tempered retired naval officer whom Andrew Jackson had appointed to represent the U.S. to the Ottoman Empire. Bryson uses the unseemly feud between the nepotistic minister and the pretentious dragoman to assess the glaring weaknesses of the foreign service in the age of Jackson. Because the politically astute Hodgson had been able to enlist an unlikely collection of patrons that included John McLean, Martin Van Buren, and Daniel Webster, he continued to receive occasional appointments from 1833 to 1840 as special agent to Egypt, Morocco, Peru, and Prussia. Hodgson finally achieved the position he had worked for most of his life when President Tyler made him consul to Tunis in 1841, only to resign a few months later in order to marry a wealthy heiress, Margaret Telfair of Savannah, Georgia. Thereafter, Hodgson lived a life

of ease devoted mostly to studying the languages and customs of the peoples of Africa and the Middle East. Without the benefit of a formal college education, Hodgson earned an international reputation as a philologist and mastered nearly a dozen languages including Arabic, Berber, Persian, and Turkish.

Drawing primarily on the records of the Department of State and the Hodgson papers at the Georgia Historical Society, Bryson focuses on Hodgson's career as a consular officer from 1826 to 1842. Bryson's research is thorough, his judgments balanced, and he ably places Hodgson in the context of the complicated international politics of the Mediterranean world. Bryson, however, is not much of a stylist, and one wonders whether it was necessary to record every stop Hodgson made on a trip from Washington to Callao, Peru, and back in 1837-38 as a bearer of dispatches. It also may be doubted whether Hodgson merited anything beyond a biographical essay. The most important contribution of the book lies not in the biography of Hodgson himself but in the insights into the amateurish foreign service provided through Bryson's thoughtful analysis of Hodgson's consular career. The author has written a useful study of one of those little-known men who "deserve their place in history, for their enterprise provided this nation with its entrée into a remote region of the world, often under the most difficult of circumstances" (p. 5).

KENNETH E. SHEWMAKER
Dartmouth College

ROBERT E. LEVINSON. *The Jews in the California Gold Rush*. (Landmarks of Western Jewish History, number 1.) New York: KTAV Publishing House. 1978. Pp. xvii, 232. \$15.00.

The ships that brought gold seekers of many nations to California beginning in 1850 carried a conspicuous number of recent German Jewish immigrants. Most of them set up in business in the mine towns after peddling among the camps, while some were actually miners. Their life as businessmen and local worthies and as Jews in the gold fields from about 1850 to 1880 forms the subject of Robert E. Levinson's interesting work. As he demonstrates, they did well, thanks not only to their skill and probity as merchants but also to the advantage they enjoyed of having good connections with the Jewish wholesale houses in San Francisco that were their suppliers. Besides, the miners were eager customers. The German Jewish immigrant merchants constituted the great majority of the commercial class in the gold fields during their heyday.

A few of the merchants were prosperous enough to invest in the expensive pit mining enterprises

that replaced the worn-out surface mines. Few merchants were that wealthy, but they were men of substance, active in the rudimentary social and civic life of their towns. Their religious observance was casual, although most were nominally Orthodox. Yet they erected fine synagogues (which they seldom attended) and equipped them handsomely. As the years passed, however, the mining towns settled down from turbulent frontiers of expansive consumption to placid, rather self-sufficient agricultural backwaters. Then the Jewish settlers tended to drift away, mainly to San Francisco. Synagogues were closed and cemeteries abandoned.

All this Levinson tells us fully, with exhaustive documentation from obscure local sources supplemented by interviews with numerous descendants of the settlers. He has provided fine illustrations and an elaborate index. However, not all his abundant material is effectively integrated into the text. It was regrettably thought essential to list financial sponsors in front, a page each for "Honor Historians," "Patrons," and "Sponsors." *The Jews in the California Gold Rush*, however, is no filiopietistic tract, notwithstanding a light apologetic tone here and there (for example, pp. 38, 68).

Levinson is not always successful at the interpretive level. Was there anti-Semitism—if that term applies then—in the gold fields? He stresses that the Jews were freely and fully accepted in local society (pp. 62, 88, 101). Yet he devotes fifteen pages to anti-Jewish expressions in the local press. Most of it emerges as earthy jokes or rough and ready comments on distant matters. Two actual incidents appear insignificant. Above all, the Jews were a very visible and distinct group, and comments on them seem to lack deep hostility or venom.

The author stresses that his merchants' business trips to San Francisco were also occasions for matchmaking in that large Jewish community. A good match also meant improved commercial connections, as had been true for centuries of mercantile history. However, we hear nothing of family life in the gold field towns.

Levinson's carefully detailed book is pleasant reading and is a worthwhile contribution to American Jewish and regional history. One wishes Levinson had deployed some of his rich material for comparative and somewhat deeper analysis.

LLOYD P. GARTNER
Tel-Aviv University

ROBERTA BALSTAD MILLER. *City and Hinterland: A Case Study of Urban Growth and Regional Development*. (Contributions in American History, number 77.) Westport, Conn.: Greenwood Press. 1979. Pp. xiv, 179. \$17.50.

As Roberta Balstad Miller correctly points out at the beginning of her book, "the study of American urban growth as an integral part of the development process of the surrounding region and, in turn, as a critical influence upon that region has rarely been attempted" (p. 3). The implication that this lacuna in American urban history is important is also correct, for it is in the context of developing city-hinterland relationships that we can best understand the role of the city in the social and cultural life of a nation that remained largely rural through most of the critical stages of its growth. Thus, the appearance of a book that the author describes as "a case study of urban growth and regional development" could be expected to arouse the deepest interest of urban historians, especially since the locus of the study is the region of a growing inland city (Syracuse, New York) during the critical decades of urbanization and national expansion that preceded the Civil War.

Unfortunately, *City and Hinterland* does not rise to its own challenge. Though useful for what it achieves (basically, a delineation of the effects of sequential transportation developments on certain economic and demographic shifts within a narrowly defined hinterland), it is too narrowly conceived to suggest but a fraction of what can be gained from systematic study of developing urban regions. Missing is the articulation of functioning institutions and communities, set within the physical space and specific terrain of the developing region, and missing as well are many of the issues that a broader and more complex study might have raised—for example, one gets no sense of the cultural significance of the city in its hinterland or of the tenor or even the frequency of rural-urban contact. Still more disturbing is the failure to make the most of those aspects of regional development on which the work does focus. No mention is made of central place theory; yet the entire study could have been conceived of and presented as an attempt to provide the dynamic, developmental component that this fascinating geographical construct still lacks—the data are assembled, but they serve only more modest ends. Finally, one of the author's central conclusions, that "urban growth had unfortunate consequences for the city's hinterland" (p. 157), is contradicted by her own data, which show that competition from the more fertile farmlands of the Midwest, not the growth of Syracuse, drained rural Onondaga County of its profits, its capital, and many of its people. The growth of Syracuse, indeed, was the one thing that continued to sustain the life of the hinterland. Miller should have challenged the idea of "urban dominance," rather than reinforce it.

The contribution of this book, then, is that it alerts us to a field of study that has been badly ne-

glected by urban historians. It is my hope that it will inspire others to go beyond it to more fully developed analyses of cities in their regions.

STUART M. BLUMIN
Cornell University

CARL SIRACUSA. *A Mechanical People: Perceptions of the Industrial Order in Massachusetts, 1815-1880*. Middletown, Conn.: Wesleyan University Press. 1979. Pp. 313. \$20.00.

It is possible, Carl Siracusa assumes, to get at Massachusetts opinion regarding industrialization and the working class through the public statements of the state's leading politicians, men who presumably told the public what it wanted to hear. In fact, they said very little on this subject, which encourages Siracusa to move in two interesting directions. First, he pieces together what fragmentary references he can find to reconstruct the image of the "respectable worker." This rather vague and superficial image, he argues, dominated political discussion and apparently blunted serious criticism, though he acknowledges that after mid-century the labor movement forced the "labor question"—if only in a limited way—into political discourse.

The simple fact of this ideological impoverishment constitutes his second concern. He correctly asks why there is so little evidence of genuine critical discourse in Massachusetts politics. What made the case of industrialization in Massachusetts so different from that in England, where questions concerning the condition of the working classes moved rather quickly into the political arena? It is a fundamentally important question, but I do not think he satisfactorily answers it. He looks to the history of the economy (for example, milder American conditions, the absence of competing economic interests) and general cultural "values" (for example, Yankee materialism, nationalistic optimism) for his answer. Surely part of the answer is there, but it is striking that he does not look also for a political explanation—one that explores the social history of power, political institutions, and political discourse. Although suggestive material abounds in his narrative, he seems unable to phrase the question in a way that leads into the political realm, into the comparative character of the two political cultures. How did issues in England and Massachusetts become a part of political discourse? Moreover, Siracusa defines political rather narrowly: forty-two very elite and mostly Whig officeholders, two party newspapers, and not much more. Perhaps the realm of political discourse is in fact wider.

The book's organization is not entirely satisfactory. In part two, where Siracusa reconstructs the consensual image of the "respectable worker," the

period from 1815 to 1865 is treated as a whole with little sense of change over time; statements from a variety of speakers from any year in this dramatically transformed half-century are brought together to form a single composite. When we get to part three, where some critics are introduced, we find an overlap of dates with the earlier positive consensus of twenty-five years. This causes some conceptual problems he might have worked through. (It should be said, however, that the third section itself is better in relating opinion to historical change.)

For this sort of study, precise meanings must be given to (and taken from) words like manufacturing, factory, factory system; worker, laborer, wage earner, operative, working class; commercial and industrial. The changing patterns of usage—along with the historically evolving sociology attached to them—make these terms vexing for the historian trying to extract meaning from texts and in choosing his own language. Siracusa seems to realize this, but the relation between these terminological matters and his thesis is so close that I wish he had faced it more fully and directly.

THOMAS BENDER
New York University

GEORGE B. FORGIE. *Patricide in the House Divided: A Psychological Interpretation of Lincoln and His Age*. New York: W. W. Norton. 1979. Pp. x, 308. \$14.95.

Freud was nine when the American Civil War ended. It was not until 1979 that the two had a truly substantial meeting. It has been a long time but the wait was worthwhile.

In searching for the origins of the Civil War George B. Forgie combines the analytical techniques of psychohistory, American studies, and traditional history. His thesis is that "a [the?] central dilemma" of the antebellum generation was the fame of the revolutionary fathers, which made their "sons" (Clay through Douglas) members of a "post-heroic cohort": men whose ambition was thwarted by the immortality of their sires. The sons' consequent parricidal urges were balanced by their filio piety; they rebelled but in an unconscious manner. In the 1840s Young America's commitment to an illusion of boundlessness—illustrated most dramatically in manifest destiny—was a failed rebellion. So was the Kansas-Nebraska Act. Douglas, the chief rebel, "inflicted a great wound" on the fathers' tradition of compromise on slavery but was "killed" by Lincoln. Between the twain, and in the face of the parricidal tendencies, some advocated sentimental regression from politics to domesticity in the fathers' house of the early Republic. But there is no escape into childhood, and the sentimentalist flight helped bring on the Civil War by liberating "the

fratricidal impulses of the post-heroic generation" (p. 198).

In the 1830s the "killer" of Douglas, among others, prophesied a fratricidal conflict. The prophecy was self-fulfilling, and in the 1850s, in a melodramatic ritual, Lincoln made Douglas play the scapegoat: the bad son threatening the fathers' house to be "killed" by the good son. Lincoln could thus both save the fathers' house and match their immortality. The projection of unrecognized parricidal urges onto Douglas, however, created tensions that Lincoln could not control. The war came (and thus Forge provides the greatest leap of faith in his schema) and with it the birth of a modern nation destroying the fathers' Republic forever. Such conceptualization gives Lincoln a gigantic role but he was able to play the role because his generation "shared the conflictual emotions he contained within himself and acted out for them" (p. 284). In the end, having long ago transferred his filial affections from his father Thomas to the fathers of the nation, Lincoln abandoned them to become "the obedient and ultimately sacrificial son" of "our Father in Heaven" (pp. 33, 288).

To readers who can accept the legitimacy of Forge's approach—though they may disagree with much, as I do (for example, the delineation of both Lincoln and Douglas)—*Patricide* carries a precious grain of truth and therefore is an important book. To others it will only engender bewilderment—in Turgenev's words "The courage to believe in nothing." Among the former is the jury of the 1972 Allan Nevins Award.

Forge develops his clear, coherent argument with a sure hand. Many caveats are nonetheless in order and at least a glimpse of some is needed here. The Freudian theoretical framework is, of course, vulnerable—but so is any theory. Within that framework Forge minimizes the first half of the *anschauung* "lieben und arbeiten"—to love and to work—to the point where he dismisses the absence of an American Jocasta in his Lincoln-Oedipus analogy by reminding us that "Oedipus got more than his father's wife; he also got his father's job. Moreover, the wife came with the job . . ." (p. 285). At times Forge is quaintly old-fashioned both in his theory and its specific applications. He even speaks of a "maternal persona" (p. 167). But Lincoln's mother—surely an all-important influence—is not mentioned.

Forge makes no attempt to place his work in the structure of previous historical literature, leaving, perhaps wisely, the job of integration to his colleagues. It should be noted, however, that if we accept the metaphor of parricide, Forge's work, too, must be seen as an act of parricide, above all toward his mentor Don E. Fehrenbacher (and also David M. Potter), for this is "a book on the origins

of the American Civil War that deals hardly at all—and then indirectly—with the sectional conflict, slavery, or political parties . . ." (p. ix). Indeed, predecessors in general are excessively, though obliquely, in target. Marvin Meyers, for example, should not have been left out of the footnotes. Edmund Wilson deserved more credit.

Forge's reasoning wobbles in places: the idea that Douglas did much to destroy the fathers' peace contradicts in part—for the historian if not the psychologist—the notion that he was a mere scapegoat. A single paragraph on page 82 contains three statements indefensible in the face of chronology. The crucial use made of seemingly unimportant words—slips of tongue perhaps—is acceptable if we accept the psychoanalytic framework; but the indifference to context with which Forge sometimes uses these words is not (for example, p. 273 n. 56). Most significantly, the import postulated for the past in the American mind will not be easy to credit for students of comparative history: Americans are a profoundly ahistorical people.

But no matter. Forge's work deserves not only attention but extension (and thus testing). After all, Washington's generation—the fathers—were also the children of fathers, and the children were also the fathers of children. Is parricide a perennial factor in historical change? And, if so, can it be demonstrated? Some years ago Jacques Barzun lamented, "It is so easy to make what looks like a book, by taking notes instead of thought and by following an outline instead of an inspiration. Everything—time, current practice, and even our scholarly virtues—tempts us to imposture." It is a pleasure to report that Forge is no impostor.

GABOR S. BORITT
Memphis State University

SANDRA S. SIZER. *Gospel Hymns and Social Religion: The Rhetoric of Nineteenth-Century Revivalism*. (American Civilization.) Philadelphia: Temple University Press. 1978. Pp. xi, 222. \$15.00.

This study discusses revivalism in nineteenth-century America. Sandra S. Sizer proposes a "historical sociology of religious language," which is "a perspective which employs insights and methods from several disciplines, especially those varieties of anthropology and literary criticism that emphasize cultural phenomena as linguistic phenomena intimately related to particular social settings" (p. 10). The book's real strength is that it draws the lyrics of gospel hymns (in particular those from the publications of Dwight L. Moody and Ira D. Sankey), novels of the period, the revivals themselves ("the Moody and Sankey fever," one source called them), and other aspects of our history into a relationship that is often illuminating.

Yet, as with many studies that attempt to relate various disciplines, Sizer's work is only partially successful. One of the problems is something that the author has set up intentionally. She does not discuss the music that is an integral part of the gospel hymns, explaining that "our current theories of musical meaning and our histories of American popular music are too sketchy to allow a nonmusicologist to draw conclusions about the relationship of the music to other cultural trends." Three justifications are given for ignoring the music—namely, that the lyrics often existed as poems in their own right, that in the mid-nineteenth century "tunes began to derive their names from the words with which they were associated," and, finally, that "singers and musicians themselves paid a great deal of attention to the words" (p. 9). Of course it is true that the lyrics are interesting in themselves. But the music provides "conclusions" that are important and that would illuminate the points the author is making. It is questionable that current work in American vernacular music is so sketchy that consideration of the music is an impossibility for one who is not a professional musicologist, and the reasons for omitting a study of the music, at any rate, are not credible. The finest studies on opera libretti, for example, rely constantly on the music that was set to words. When the author does venture into brief considerations of musical matters, the result is not satisfactory. Perhaps some of these problems could have been alleviated if the title of the book had been changed to *The Texts of Gospel Hymns and Social Religion*. At least then we would be aware of the book's focus at the onset.

Some of the discussions of hymn texts state the obvious, and it is a pity the author did not venture more critical opinions on the poems and novels she analyzes. Often the discussion gives longer than necessary plot synopses (such as that of Timothy Shay Arthur's *Ten Nights in a Bar-room*). Although the notes are extensive, there is no bibliography.

The overall impression, however, is positive. Sizer reaches interesting and useful conclusions about her subjects, and she tells us much about the "fever" that made revivalism such an American situation and such a product of its time.

GARRY E. CLARKE
Washington College

DEBORAH PICKMAN CLIFFORD. *Mine Eyes Have Seen the Glory: A Biography of Julia Ward Howe*. Boston: Little, Brown. 1979. Pp. 313. \$15.00.

Deborah Pickman Clifford's biography of Julia Ward Howe appraises the life of one of the most well-recognized and admired women of the nineteenth century. During her long and productive life,

which spanned the years 1819–1910, Howe published several volumes of poetry and prose, lectured throughout the United States and Europe, served as an editor of the American Woman Suffrage Association's weekly magazine, the *Woman's Journal*, aided in the forming of women's clubs throughout the country, and became a national celebrity after authoring the "Battle Hymn of the Republic" in 1861. This long list of accomplishments, plus many others, is noteworthy in and of itself. What makes it truly extraordinary is the fact that Howe engaged in these activities while raising six children and tending to the needs of her husband, Samuel Gridley Howe, who voiced numerous criticisms of his wife's work outside the domestic sphere.

Drawing primarily from Howe's personal papers, Clifford does a commendable job of relating how Howe juggled family responsibilities with an active career as a writer, lecturer, and reformer. During her early years of marriage in the mid 1840s, Howe bemoaned the isolation she experienced as an intelligent young woman who felt overwhelmed by the responsibilities of motherhood, while her husband remained away most of the day pursuing his own profession. Although Howe came to enjoy motherhood, she found it much more difficult to reconcile her yearning for a career with her husband's desire that she remain exclusively a devoted wife and mother. Clifford shows how she gradually came to realize that it was necessary to develop her own talents, even if this meant angering her husband. Thus, from the publication of Howe's first book of poetry in 1854, until the death of her husband in 1876, her marriage was wracked with conflict and crisis.

Clifford is less successful in relating the life of Howe to the emerging nineteenth-century woman's movement. For example, in explaining why Howe was not elected to a leadership position in the newly formed National American Woman Suffrage Association in 1890, Clifford suggests that this can be partially attributed to the fact that many suffragists did not feel that she "was single-minded enough about the woman's movement" (p. 250). Unfortunately, the author never fully explores this idea. Because Clifford is evidently unfamiliar with the complexities of the nineteenth-century woman's movement, she misses an opportunity to discuss a very important aspect of the life of Julia Ward Howe.

JUDY BARRETT LITOFF
Bryant College

JOHN R. WUNDER. *Inferior Courts, Superior Justice: A History of the Justices of the Peace on the Northwest Frontier, 1853–1899*. (Contributions in Legal Studies, num-

ber 7.) Westport, Conn.: Greenwood Press. 1979. Pp. xvi, 246. \$18.95.

This little book contains what many of us always wanted to know about justices of the peace but never expected to find in one place. John R. Wunder begins with an encapsulated history of the office from the twelfth through the nineteenth century and then illustrates the principal lines of development with a comprehensive case study of JPs in the territory that is now the state of Washington. Although JPs often exceeded their commissions throughout these seven hundred years, their communities and higher authorities (however various) usually ratified their use of power because they provided necessary and valued services: maintaining law and order and supplying judicious decisions at the local level. An institution that originated in medieval England thus proved equally useful in modern America, and it was part of the cultural baggage that accompanied the settlement of the continent. The author concludes that the myth of lawlessness on successive American frontiers has little basis in fact and that the very prevalence of the office is itself evidence that contradicts the conventional wisdom.

In surveying the existing literature about conditions along the borders, Wunder frequently reaches conclusions different from those of earlier writers. The reason for this variance lies in the sources. Too many accounts have been based upon romantic notions about the wildness of the West or exaggerations of Turnerian theory, but Wunder's is grounded on verifiable data and that is the significance of this monograph. Long after everyone has incorporated the meat of part one in lecture notes, the demonstration in later chapters that mythology falls before evidence should remain to challenge the profession. American legal history is on the verge of maturity: the point where reality replaces romanticism. Wunder has joined a small but growing number of scholars whose microcosmic studies will someday provide the framework for a more accurate view of this important part of our national experience. One hopes that at that point legal history—like other specialized fields—will be incorporated into a more comprehensive understanding of our past.

The book has some oddities of language, style, and organization; a few instances where the author does not follow his own advice to examine primary sources; and some judgments that may, not stand upon re-evaluation. In its emphasis upon the legal aspects of the office under federal territorial jurisdiction, it complements Robert M. Ireland's *County Courts of Antebellum Kentucky*, which emphasized the political aspects within a state. Together these

books suggest a fruitful field for comparative studies.

MARY K. BONSTEEL TACHAU
University of Louisville

MORRIS F. TAYLOR. *O. P. McMains and the Maxwell Land Grant Conflict*. Tucson: University of Arizona Press. 1979. Pp. xvi, 365. Cloth \$16.50, paper \$9.50.

Oliver P. McMains was born in Milford, Ohio in 1840. When he was fifteen, his mother died and his father went to California, leaving him in the care of his aunt, Debbie Mount. He learned the printing trade, at nineteen became a Methodist minister, and then served pastorates in Illinois before going to Cimarron, New Mexico in 1875.

Here he acquired a ranch and a wife, Mary Messmer. On September 14, 1875, one of McMains' friends, the Reverend Franklin J. Tolby, was murdered. McMains set out to find the killer, and during this search he became involved in the Maxwell Land Grant dispute. Spanish and Mexican land tenure was quite different from that of the United States. Those governments made large community and family grants. Each settler had his own individual plot, and there was a large area for the common use of all for grazing, hunting, and wood gathering. The Treaty of Guadalupe Hidalgo promised to recognize these grants in the territories obtained by the United States from Mexico, but the United States was slow to survey the land.

Lucien Maxwell had secured a Mexican grant of over 1,700,000 acres in New Mexico and Colorado territories. In 1870 Maxwell sold his grant but reserved 96,000 acres. By 1877 the company had surveyed about 1,800,000 acres, although many Americans had homesteaded the land believing it to be in the public domain. Railroads and other corporations had also made large claims.

McMains and the settlers claimed that only 96,000 acres of the claim were valid. In the Taelin Supreme Court case the Court held that, in spite of the invalid Mexican claim, since the United States Congress had approved the survey and patent, it was now a claim *de novo* and was therefore legal. When Congress approved the Maxwell survey, McMains and all other settlers had either to come to terms with the company, whose tactics were not always fair, or to fight. McMains became the leader of several hundred settlers who chose to fight.

In the long fight against the Maxwell Land Grant, McMains made fourteen trips to Washington, one of which trips included an interview with President Cleveland in 1886. During the fight he served in the New Mexico Territorial Legislature, was tried and acquitted of manslaughter, but was

convicted of conspiracy and served six months in prison.

The long fight ended in failure when, on April 18, 1887, the U.S. Supreme Court ruled that the land was not in the public domain and the settlers had no rights superior to those of the original grantee. This was not just a struggle over land ownership but a battle against greed, injustice, and corruption. In 1894 McMains was defeated as a Populist candidate for the Colorado General Assembly. He died on April 15, 1899.

The original McMains papers were destroyed but not before the late Morris F. Taylor had made copies. Taylor used many primary sources including interviews with participants, church records, Land Office records, newspapers, pamphlets, and scores of secondary materials, all of which are documented in the work. The book also has a good index.

THOMAS LLOYD MILLER
Texas A & M University

LAWRENCE H. LARSEN. *The Urban West at the End of the Frontier*. Lawrence: Regents Press of Kansas. 1978. Pp. xiii, 173. \$12.50.

Until recently historians have neglected the examination of urbanization as a unifying theme in the history of the far western American frontier. In the last two decades, they have, however, produced a number of specialized local and regional studies of western cities and western urban topics. Lawrence H. Larsen skillfully draws on these studies, old local histories, and extensive primary material to present a fascinating description of cities and urban life west of the 95th meridian in the period primarily from 1880 to 1890. The frontier, of course, was officially pronounced at an end in 1890, and the period he chose permitted use of one of the most remarkable of our public documents, the unique "Report on the Social Statistics of Cities," which was a part of the 1880 census. This massive compilation of individual accounts of 222 American cities is Larsen's main source, and no one has used it to better purpose.

Larsen synthesizes and elaborates a number of themes about western urban development suggested by recent urban historians: the importance of urban promotion and rivalry in the location and growth of American western cities, the ethnic diversity of urban populations, and the emergence of a system of cities centered around the regional metropolises of Kansas City, San Francisco, and Denver. But the central thesis he develops is that western urban life was not distinctive, that western cities were calculatedly modeled on those to the east. The frontier did not reshape urban institutions; the mis-

takes and accomplishments of earlier urban society were repeated time and again. "The cities of the West," he emphasizes, "represented an extension of a process older than the Republic—exploration, settlement, and growth, a process that resulted in a nation of cities" (p. 121). Larsen proves his point, but his debate throughout the book with Frederick Jackson Turner over the innovative effects of the frontier may have already been won by others. His framework, however, does enable him to find a leitmotif in the vision of William Gilpin, a western promoter of Kansas City and Denver who offered a Turnerian and yet explicitly urban conception of the American frontier.

Although Larsen's examination of general themes is clear and sound, the strength of his book lies in his concrete descriptions of urban activities and institutions in the twenty-four western cities above eight thousand in population. He refreshingly avoids the mathematical examinations of mobility and related questions that are a part of an occasionally trite "new urban history" and is at his best when he writes of such down-to-earth matters as buildings, street cars, and garbage. Although his work is brief, it presents the same type of rich detail to be found in the works of Richard C. Wade and Carl Bridenbaugh on other urban frontiers in American history. Examining themes about western cities to be found in movies and television, he shows, for example, how little the "bucket brigade," "the young physician who saves the town," and of course the "brave, bold lawman," reflect the reality of fire protection, public health, or law enforcement in the western town and city. Larsen's work is more limited than Wade's or Bridenbaugh's and is not as original. But he makes a similar contribution. His sound and lucid book demonstrates that the frontier urban process to be found in Bridenbaugh's colonial cities and in those of Wade's Midwest persisted in the West of the late nineteenth century.

CHARLES N. GLAAB
University of Toledo

JOHN W. BAILEY. *Pacifying the Plains: General Alfred Terry and the Decline of the Sioux, 1866-1890*. (Contributions in Military History, number 17.) Westport, Conn.: Greenwood Press. 1979. Pp. xiv, 236. \$18.95.

Alfred H. Terry is known to history as the scholarly lawyer-general who in the Civil War took Fort Fisher, North Carolina, after Ben Butler had failed and then, in the postwar years, commanded the Department of Dakota during the most intense hostilities with the Sioux and Cheyennes. Terry served on the commission that negotiated the Fort Laramie treaty of 1868 and is best known as George A. Cus-

ter's commander in the ill-fated Sioux campaign of 1876.

John W. Bailey has done a workmanlike job of chronicling Terry's career and assessing his significance. The portrait that emerges confirms previous images of a quietly competent and unusually thoughtful soldier, lacking the contentiousness and flamboyance of many of his contemporaries.

Beyond the usual official records, Bailey has used the Terry Papers at Yale University. Unfortunately they do not contain the riches to be expected from so literate a figure and thus do not permit significant new insights into either the personal or official dimensions of Terry.

Bailey sees Terry as a decisive voice in the deliberations of the Peace Commission of 1868, although he may have been as much simply the one who expressed the thoughts of Sherman, Augur, Harney, and other of the military members. Also, Terry took a much softer, more compassionate line on the Black Hills issue than Sherman, Sheridan, and Crook, believing that troops should be used to keep the miners out of this gold-rich region as long as the Indians owned it. This view, of course, did not prevail, and it does not appear that Terry urged it very persistently. In treating the Little Bighorn controversies, Bailey judges Custer harshly, much more harshly than did Terry himself. On this and other topics, Bailey would have profited from a deeper reading of secondary sources. Especially important would have been John Gray's recent *Centennial Campaign*, which has much to say about Terry.

Bailey's volume is a competent biography of a figure who has long deserved one and a useful addition to the literature of the Indian wars.

ROBERT M. UTLEY
Advisory Council on Historic Preservation

PAUL KLEPPNER. *The Third Electoral System, 1853-1892: Parties, Voters, and Political Cultures*. Chapel Hill: University of North Carolina Press. 1979. Pp. xxii, 424. \$21.00.

The Third Electoral System is the most important voting-behavior study to appear to date. In it, Paul Kleppner builds upon, but goes far beyond, his first full-length work, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850-1900* (1970). That pioneering monograph analyzed group voting behavior in three states before and during the electoral-political realignment of the 1890s. The present study treats all sections, particularly the Northeast, Midwest, and Border, from the realignment of the 1850s that gave birth to the Third Electoral System, through the Democratic upsurge of 1889-92 in the

Northeast and Midwest that revealed the continued potency of long-standing cultural issues in those areas and the Populist upsurge of 1890-92 in the West that anticipated the sectional-economic cleavages of the national realignment of the 1890s.

Kleppner makes clear his commitment to historical analysis as a means of developing social theory. Primarily interested in studying "the social bases of mass partisan support," he deals with party officeholders and workers only as they relate to the party-in-the-electorate. He contends that important sources of differences between the political parties lay outside of the parties themselves in antagonisms among social groups. Kleppner analyzes election and population data from counties and minor civil divisions in many states to establish the voting behavior of social groups, and a variety of primary sources to illuminate social-group conflicts and to explain why some became politically relevant in particular contexts and others did not. Kleppner argues, as he did in *The Cross of Culture*, that within the nonsouthern electorate, the primary conflict of political significance pitted evangelical-pietistic against ritualistic religious groups. However, his growing understanding of social groups in the Midwest and elsewhere has led him to refine his formulation of the pietistic-ritualistic continuum and to take into fuller account historical and contextual factors—for example, ethnocultural conflicts, status-group distinctions, political-generational experiences, economic conflicts and conditions—in explaining group voting behavior. That explanation is sophisticated and generally persuasive.

Approving the body of Kleppner's analysis does not necessitate accepting all its parts. The discussion of the electoral realignment of the 1850s does not make clear the relative importance of voter conversion and recruitment. Kleppner probably exaggerates the plight of the northern Democracy at the end of the Civil War. He offers scant evidence of an "accelerating" wartime Republican attack on Catholics. (More to the point in his treatment of "a revived Catholic question" during the 1870s.) Kleppner deals with many, but not all, of the issues raised by Melvyn Hammarberg's *The Indiana Voter* (1977). One wonders why Yankee counties shifted with, not against, German units during the Democratic revival of 1889-92.

Kleppner writes clearly, though he is repetitive and sometimes given to jargon (for example, "impacted," "repercentagized"). The University of North Carolina Press has served all well by providing footnotes, tables, and maps. In short, this book is a model of research, analysis, and presentation. It should influence all subsequent work in the field.

SAMUEL T. MCSEVENEY
Vanderbilt University

PAUL BOYER. *Urban Masses and Moral Order in America, 1820-1920*. Cambridge, Mass.: Harvard University Press. 1978. Pp. xvi, 387. \$18.50.

In the preface to this rich, widely ranging book, Paul Boyer recalls his boyhood in Dayton, Ohio, where he attended Sunday school at an urban mission founded by his grandfather, learned to swim at the local YMCA, and spent free time at a supervised playground run by the city. Now, after excellent studies of book censorship and of the repression of the Salem witches, he has produced a panoramic account of how the kinds of moral uplift and civic betterment that shaped his own early experience came into being. As in his previous books, Boyer adopts a social-control perspective, in which fears of disorder and of lower classes stand out as motives for doing good. This approach, while limiting sharply *what* he examines, is executed with subtlety and flexibility. The result is an insightful, important, multi-faceted history of reform.

Boyer perceives the modern city as a place of anonymity and diversity, in which Protestant reformers, from Lyman Beecher and the Tappan brothers onward, tried to recreate the cohesion and rectitude they associated with the small town. Their principal achievements in the Jacksonian era were tract societies and Sunday schools. Boyer gives us a fascinating analysis of how the tracts deliberately played on their readers' memories of an intimate village community, while the Sunday schools instilled orderly, deferential patterns of behavior. This section of the book shows how young tract volunteers and Sunday school teachers derived from their pious labors a comforting discipline and sense of fraternity among themselves.

Following the depression of 1837, increasing disorder among the urban poor, plus the setbacks that temperance reformers and Sunday school promoters met, led to more specifically focused enterprises: the Association for Improving the Condition of the Poor, the Children's Aid Society, and the YMCA. These mid-century initiatives were followed by the Salvation Army, the charity organization movement, and in the 1890s the political crusades of Charles Parkhurst and others. Each of these endeavors is sensitively described; but not until we reach the Progressive Era, to which the last third of the book is devoted, do the basic strategies of urban moral reform become altogether clear.

According to Boyer, two styles of reform, which earlier had intermingled, became sharply differentiated in the early twentieth century. A negative and coercive style, typified by efforts to outlaw the brothel and the saloon, concentrated on eradicating evil institutions. Another type of reformer, whom Boyer labels "positive environmentalist," repu-

diated repression in favor of environmental changes that would call forth more spontaneously the good behavior and the social harmony that all desired. This was the spirit of some housing reform (though tenement laws seem to belong to the first category more than the second), campaigns for public parks and supervised playgrounds, and above all the City Beautiful movement. Boyer is especially good in showing how civic beautification and the subsequent rise of city planning sprang from the belief that a physically attractive urban environment would inspire in the population at large a new upsurge of civic loyalty. Esthetic harmony would result in moral order. These environmental strategies, however, allowed reformers to stand aloof from the urban masses. By operating on the people indirectly, do-gooders gave up the personal contact their nineteenth-century predecessors had characteristically sought.

A final chapter on the 1920s celebrates a decline of the traditional American hostility toward the city and a concomitant fading of the old Protestant impulse to impose a single morality on a heterogeneous society. With the acceptance of urban diversity by American intellectuals the book comes to a happy ending. My own emotions were somewhat more mixed. I had learned a great deal about a variety of reform movements in an urban context, as understood in the light of a traditional social ideal their promoters shared. On the other hand, the fearful and backward-looking attitudes examined in these pages did not seem to me to embrace enough of "America's moral response to the city" (p. vii) to fulfill the larger aim of the book. Boyer's story is one of continuity and uniformity; but the ultimate challenge of his subject would seem to be an understanding of the ways in which and the extent to which the city has diversified our moral universe.

JOHN HIGHAM

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DAVID R. JOHNSON. *Policing the Urban Underworld: The Impact of Crime on the Development of the American Police, 1800-1937*. Philadelphia: Temple University Press. 1979. Fp. viii, 240. \$15.00.

By describing three categories of criminal behavior in the nineteenth century and certain practices of early-nineteenth-century policing, David R. Johnson has tried to show how criminals determined police behavior. His thesis is that police, "supposedly controlled" by "public and administrators," were in fact at the mercy of changing patterns of crime. But the book contains virtually no evidence to support this interesting thesis, except in its description of detectives' corrupt relationships with vice and certain

theft operations. It is not surprising that his thesis cannot be supported, because very little of nineteenth-century policing was concerned with crime control. Most police work had to do with non-crime-related services; and professional criminals and their counterparts in the police departments, detectives, while media sensations, simply never made up much of the sum of the activity of the urban police. While it may be fair to say that the goals that police administrators and, perhaps, the public set for the police were never attained, the reasons for this failure range from faulty theory to flawed implementation. Including criminal behavior in an account of nineteenth-century policing can be instructive, but it cannot hope to provide either necessary or sufficient conditions to explain the behavior of urban police.

Certainly this book's major contribution to social history has nothing to do with its thesis. Rather, its value is as a description of certain kinds of crime, mainly in Chicago and Philadelphia, with occasional material on New York or other cities. Basing his work primarily on newspaper sources, Johnson has also pulled together frustratingly scattered descriptive material on juvenile gangs, the methods of professional thieves, and the evolution of urban gambling and prostitution in the first three-fourths of the nineteenth century. Particularly in the area of gambling, he has described the growth and operation of syndicates, the emergence of faro and policy, and the changing forms of betting on horse races. These areas of social behavior, for obvious reasons, had a good deal of secrecy surrounding them, and by pulling together material from newspapers, buttressed by memoirs and earlier histories, Johnson seems to have written a credible description of these kinds of criminal behaviors, particularly vice.

The question of sources, however, becomes of great concern when describing such things as the first gambling syndicate (C. H. Murray and Company, New York, 1861). Johnson too eagerly accepts newspaper accounts and self-serving memoirs at face value. Given that such sources are the only sources, one must be cautious in accepting his or nineteenth-century observers' estimates of the numbers of prostitutes, brothels, or the annual cash intake of gamblers. At some points Johnson's uncritical use of sources leads to inaccurate descriptions. For instance, he asserts that police officers kept arrest tallies to justify promotions, citing sources for New York and Chicago. Yet of the two Chicago sources, only one mentions arrests per officer. Criticizing some officers for arresting too many people (thirty in a month) says nothing about arrests and promotions (pp. 129, 219). Similarly, the book abounds with unsupported factual assertions, many of which should have been important questions for

empirical research: for example, urban rioting declined after 1860; assaults on police peaked in the early 1850s; arrests on "suspicion" caught "numerous" thieves; and police collusion with vice entrepreneurs began in the 1840s (pp. 134, 136, 131, 170).

Possibly the best parts of Johnson's book are the remarks and observations scattered throughout on the notion of crime prevention. Here he discusses how the prevention idea implied an ability to predict (p. 19) and points out that for police to achieve the semblance of successful crime prevention they had to opt for "criminals' cooperation to achieve the appearance of law enforcement" (p. 185). Simply by emphasizing how little today's law enforcement techniques have changed from those of the nineteenth century, Johnson demonstrates that the social complexity of criminal behavior will not admit easy deterrence solutions. This perspective is one that many criminologists have come to recently. That a historian, working with recalcitrant nineteenth-century materials, can come to the same position bodes well for the relevance of historical scholarship.

ERIC H. MONKKONEN
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JOHN T. CUMBLER. *Working-Class Community in Industrial America: Work, Leisure, and Struggle in Two Industrial Cities, 1880-1930*. (Contributions in Labor History, number 8.) Westport, Conn.: Greenwood Press. 1979. Pp. xiv, 283. \$22.95.

Labor historians have long known the important differences between textile workers and those employed in shoe factories. In this convincing comparative study of laborers in Lynn and Fall River, John T. Cumbler points to similarities as well. In both Massachusetts cities, Cumbler finds nineteenth-century working-class communities that integrated newcomers, provided fellowship, offered support in adversity, and nurtured solidarity against employers. For different reasons, these two communities became less cohesive in the twentieth century. But factories were not to blame for individual isolation and alienation, as some critics have argued. Rather factories provided the web of human relationships that sustained people in these cities.

Social institutions in Lynn and Fall River absorbed newly arrived French-Canadians, some of whom had come as strikebreakers. Through contact at work, in boardinghouses and taverns, at lodges and clubs, at dances, parades, and rallies, these immigrants learned to identify with others in the working class. In many cases, in spite of opposition from their priests, French-Canadians joined unions

and picket lines. Especially in Fall River, the strikes were usually broken. But Cumbler demonstrates that the working-class unity that helped sustain strikes endured until economic and demographic changes in the twentieth century disrupted it. Then workers in Fall River, fearing for their jobs and for the future of their industry, did not accept Polish and Portuguese newcomers. The shift in Lynn from the production of shoes to electrical equipment modified social institutions and habits, partly because the General Electric plant was geographically remote from the central part of the city where shoe workers had lived and interacted. "Consumerism," Cumbler asserts, and the attempt to secure personal economic security replaced the working-class community that once characterized both cities.

This book bears the stamp of the "new" labor history, now about two decades old. In these studies, trade unions, labor leaders, and reform politics—once the central concerns of labor historians—take the subordinate place they occupied in the lives of workers themselves. The scholarship focuses on specific local data; Cumbler's is the third recent work on laborers in Lynn. But the imaginative use of these data—including photographs in this book, which are in fact visual evidence and not merely diverting illustrations—is broadening our understanding of American social history.

The book has defects. Cumbler needed more editorial assistance than he received, and his publisher badly needs a new proofreader. The index is perfunctory. The explanation for the failure of Fall River workers to welcome Polish and Portuguese immigrants is not entirely persuasive, and "consumerism" is not a rigorous analytical concept. These shortcomings, however, do not diminish the central part of Cumbler's study in any fundamental way.

HENRY F. BEDFORD
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CARLOS A. SCHWANTES. *Radical Heritage: Labor, Socialism, and Reform in Washington and British Columbia, 1885-1917*. (Emil and Kathleen Sick Lecture-Book Series in Western History and Biography.) Seattle: University of Washington Press. 1979. Pp. xviii, 288. \$14.95.

The Pacific Northwest, long judged a center of radical socioeconomic experimentation, has frequently appalled middle-of-the-road American and Canadian reformers, and one exasperated New Dealer even assertedly characterized Washington state as America's "Soviet." Carlos A. Schwantes is the first to tap a rich lode of archival material as well as thesis and dissertation literature to describe the historical origins of that tolerance of radical activism

and to account for the traditions and other particularisms that have sustained a "radical heritage" in the Northwest, particularly in British Columbia and Washington. There, residents were heavily late-nineteenth-century newcomers who, in searching for a better social order, joined world-savers in testing virtually all reform proposals generated during the Gilded Age. Eventually the ideas of labor reformers, socialists, and agrarian radicals won the greatest esteem.

Since lumbering, mining, and commerce sustained the Northwest economically, labor reformers there cultivated a fertile vineyard in which plentiful and conflicting labor union movements proliferated. *Radical Heritage*, therefore, is primarily an account of those movements. The narrative revolves around Schwantes's thesis that unionists, their farmer allies, and some Socialists fashioned a distinctive "regional unionism," which unionists maintained in the twentieth century, although the American Federation of Labor eventually absorbed many British Columbia and Washington unions. Unable to obliterate the Northwest unionists' unique beliefs and practices, which conflicted sharply with AFL orthodoxies, hapless AFL hierarchs and their Northwest emissaries could only grince. Washington unionists persisted in tugging in union with agrarian rebels, and both British Columbia and Washington AFL members saw no heresy in their industrial union philosophies. Likewise, British Columbia unionists discounted pronouncements from AFL President Samuel Gompers that socialism was "silly" and pioneered labor-socialist political action.

Other activists contributed measurably but secondarily to establishing the Northwest's "radical heritage." Among them were agrarian radicals, assorted utopians with communal experiments on their minds, and middle-class Progressives who, doubtless in great mortification, share a chapter in *Radical Heritage* with Wobblies. More importantly, by Schwantes's reckoning enigmatic and eternally disputatious socialists supplied ideas and creative leadership for the enduring and sometimes unabashedly radical quest of Northwest residents for a more equitable society. But for reasons that the author might have made clearer, British Columbia socialists and agrarian socialists in Washington pre-empted socialism's laurels in the Northwest and exercised vastly greater influence than did the crowd of urban socialists in Washington.

Probably some readers will deny that Northwest unionism was so distinctive; others may insist that nonlabor groups bear more responsibility for Northwest radicalism. Nevertheless, this book offers plenty that is worthwhile. Thanks to Schwantes's expertise on laboring men and their organizations, we now have a reliable history of pre-1917 North-

west unionism. His lively chapters on Northwest socialists contain new material and invite additional research. Schwantes's sketches of agrarian radicals open the door to further study of Northwest farmer-labor movements.

HUGH T. LOVIN
Boise State University

WALTER F. PETERSON. *An Industrial Heritage: Allis-Chalmers Corporation*. Milwaukee: Milwaukee County Historical Society. 1978. Pp. 448. \$17.50.

Allis-Chalmers, now a large manufacturer, started out modestly in 1861. That year, Edward P. Allis, a New Yorker who had moved to Milwaukee, bought the Reliance Works, manufacturers of milling, sawmills, and cast iron products. Allis had also purchased in Milwaukee twenty acres on which he later moved the Reliance Works and built other factories. After several periods of fiscal difficulties and friction both internally and with organized labor, the corporation merged with three others, one of which was Fraser & Chalmers, and in 1901 formed the Allis-Chalmers Company. A power struggle was resolved with the appointment in 1912 of General Otto H. Falk. The company, a combination of divisions, had taken the form we know today. Allis-Chalmers has preserved its independence, relations with the labor unions are stable, and sales in 1976 reached a peak of \$1.5 billion.

Two figures predominated: Edward Allis and Otto Falk. They had a common pattern of operations: hard work, good equipment, accountability, able personnel, diversity, flexibility, and venturesomeness. More unusual was their emphasis on research, almost constant physical and product expansion, and divisional structure. The original divisions of sawmill, flour milling, and steam engines became virtually autonomous units in research, production, and sales. Falk's special achievement was adding the farm tractor division, which by 1938 was the major source of company profit.

Both authors are familiar with engines and with Allis-Chalmers. Walter Peterson, now President of University of Dubuque, provides ten carefully organized, clearly written chapters bringing the story to 1965. Although there is no mention of the connection anywhere in the volume, Peterson's role as consultant to the company since 1959 might have provided special insights. It is unfortunate that either he or his publishers has chosen to be silent on this connection, raising questions where clarification might have provided even greater authority for his very full treatment. Edward Weber, a dean of business administration, prepared the epilogue which brings the account down to 1977. The most exciting sections describe how the tractor division

turned defeat into success and how the company fought efforts by conglomerates to take over. The bibliography is fuller and more varied until 1945; thereafter it depends heavily on the annual reports of the company. Although the tone of the main part of the book seems to lack sufficient criticism, the work is certainly a useful addition, for more histories of companies are badly needed.

WILLIAM CHAZANOF
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Fredonia

ROBERT EMMETT CURRAN. *Michael Augustine Corrigan and the Shaping of Conservative Catholicism in America, 1878-1902*. (The American Catholic Tradition.) New York: Arno Press. 1978. Pp. xiii, 547. \$34.00.

In another of its remarkable contributions to the literature of American history, Arno Press has produced a series of volumes on American Catholicism, including Robert Emmett Curran's fine Yale doctoral dissertation on New York's Archbishop Michael Augustine Corrigan. Scholars like Gerald Fogarty, Thomas Wangler, and Margaret Reher have been rewriting the history of American Catholic liberalism in the late nineteenth century, and Curran has now examined the major conservative spokesman of the period. Students of the Americanist movement have found American Catholic liberalism to be intellectually serious and more politically organized than was thought to have been the case. Curran finds that the conservative side, represented by Corrigan, had considerably less intellectual sophistication than their liberal adversaries, but at least equal political skills.

In the aftermath of the Third Plenary Council of Baltimore in 1884, serious divisions developed in the American hierarchy. Liberals like John Ireland, Denis O'Connell, and John Keane questioned the need for a highly organized Catholic subculture, pushing instead for the rapid assimilation of immigrants and creative engagement with secular culture. Hopeful and optimistic about the church's future in a democratic and progressive era, they were Americanists both in theory and practice. Their ideas, and by implication their policies, were eventually rejected by Rome, a defeat that in part resulted from the deliberate combat waged against them by Corrigan. Rome was the real arena of the struggle, and influence with the new Apostolic Delegate was the key to success. Initially, the liberals enjoyed favor with the curia and influence with the delegate; but Corrigan formed his own alliances with curial factions on the basis of contacts made during his student days. In the end, his theological conservatism, his concern for diocesan autonomy and episcopal control, and his pastoral priorities

corresponded far more closely to the goals of the ultramontane papacy than did the venturesome Americanism of his opponents. Corrigan's victory meant, according to Curran, the formation of a rigid Catholic subculture that "abandoned any attempt to find an equation between Catholicity and Americanism."

Curran is strongest in relating Corrigan's policies to his personality. Sheltered as a youth in a prosperous family, lacking pastoral experience, and experiencing ecclesiastical advancement because of his Roman connections, Corrigan was an unusually insecure bishop. In New York he confronted a well-organized group of very popular and aggressive priests headed by Edward McGlynn. His clash with McGlynn over the latter's political activism led to McGlynn's excommunication, followed by a long series of actions designed to embarrass and humiliate Corrigan's clerical opponents and force his priests to accept his domination of diocesan affairs. According to Curran, Corrigan "was almost pathologically prone to a fear of ubiquitous conspiracies" and not at all reluctant to use deception, spying, loyalty oaths, and preferential assignments to secure his authority and power. Although Corrigan's personality played an important role, one suspects that his victory over the Americanists reflected the requirements of a maturing American Church confronted with massive waves of new immigrants and the needs of a triumphant Roman Ultramontan-ism. As a case study of the politics of the church and the personality of an individual bishop, Curran's candid, thorough essay can hardly be matched in the historical literature of American Catholicism.

DAVID J. O'BRIEN
Holy Cross College

MICHAEL N. DOBKOWSKI. *The Tarnished Dream: The Basis of American Anti-Semitism*. (Contributions in American History, number 81.) Westport, Conn.: Greenwood Press. 1979. Pp. x, 291. \$22.50.

The Tarnished Dream is a striking book. Not since the 1950s have American historians engaged in serious analysis of the roots of anti-Semitism in the United States. Now Michael N. Dobkowski, assistant professor of religious studies at Hobart and William Smith Colleges, once again explores the subject more broadly and deeply than Handlin, Hofstadter, and Higham did a generation ago. Like them he focuses primarily on the Gilded Age and Progressive Era, but unlike his predecessors he demonstrates the pervasiveness of anti-Semitism in American society. Demolishing as "a misleading oversimplification" the myth that "overt and articulate anti-Semitism . . . rested primarily with the anti-Semitic rhetoric of the Populist movement" (p.

174), Dobkowski argues that Handlin, Hofstadter, and Higham displayed "an inclination to deny the anti-Jewish character of the most obvious and flagrant stereotypical expressions that appeared frequently in literature, on the stage, and in print" (p. 6).

One can certainly not accuse Dobkowski, however, of this transgression. His extensive discussions and bibliography testify not only to the thoroughness of his reading but also to the ubiquitousness of anti-Semitism in the United States. To reach for an understanding of this complex phenomenon, Dobkowski analyzes, in single chapters, religious origins, images of the Jew as criminal and lover of gold, patrician sourness, the impact of the immigrant influx, and the attitudes toward the radical Jew.

What Dobowski has done in his treatment of anti-Semitism during the fifty-year period between the 1870s and 1920s should be a model for historians examining anti-Semitism in other eras of the American past. To date, sociologists and psychologists have produced the most insightful works on the subject (for example, T. S. Adorno, Charles Y. Glock, Gertrude Selznick, Rodney Stark, and Stephen Steinberg). We know too little about anti-Semitism in colonial America and the young republic. Isolated snippets about *Niles Register* and the oft-repeated order by General Grant ridding his military zone of Jews during the Civil War do not provide sufficient data for historical scrutiny. But careful study of literary, journalistic, autobiographical, and other sources should shed light on anti-Semitism in the United States before the 1870s.

I cannot conclude this review without re-emphasizing the importance of Dobkowski's contribution. He has not only proven the existence of a deeply rooted, widespread anti-Semitism in the United States in the Gilded Age and after, but also his accomplishment immediately ranks him with the best and most promising younger scholars of American Jewish history: Leon Jick, Steven Hertzberg, and Hasia R. Diner.

LEONARD DINNERSTEIN
University of Arizona

TIMOTHY P. WEBER. *Living in the Shadow of the Second Coming: American Premillennialism, 1875-1925*. New York: Oxford University Press. 1979. Pp. vi, 232. \$14.95.

Until fairly recently American conservative Protestantism in the post-Civil War era had been terra incognita to most historians. The stiff-necked traditionalism of many churchgoers in that epoch seemed far less worthy of attention than the innovative responses of social gospellers and theological liberals to the challenges of a rapidly developing

industrial society. The evident anti-intellectualism of those committed to revivalism, the holiness movement, or pentecostalism also probably caused historians of American religion to turn to other, more congenial, topics. Over the past decade or so, however, detailed monographic studies of scholars like Ernest Sandeen, Timothy Smith, and William McLoughlin began to reveal in detail the historical lineaments of late-nineteenth- and early-twentieth-century conservative Protestantism. Now the process of recovery of this tradition may be advanced a step further as contemporary conservative churches, experiencing a new respectability in the public eye, spawn their own group of young scholars determined to explore essential elements of a common past.

The book under review, by Timothy P. Weber, a young church historian at Conservative Baptist Theological Seminary in Denver, seems to fit such a mold. Weber's essay is a reworked Ph.D. dissertation from the University of Chicago (anathema to conservative Protestants just a generation ago). The book explores in detail premillennialism—one of the key intellectual-theological movements that shaped post-Civil War Protestantism. Weber is interested primarily in explaining how premillennialists of the late nineteenth and early twentieth centuries lived out the peculiarities of their faith, how "beliefs were translated into daily life" (p. 8). The author examines the premillennialists' distinctive emphases upon eschatology and the hovering imminence of a cataclysmic second coming of Christ and the effect of such beliefs on personal approaches to evangelism, to social activism and reform, and to the events of World War I. The early sections of the book are interesting, but they also include many repetitious passages and too often lack historical specificity.

Weber's analysis becomes more effective as the narrative proceeds. He includes a fascinating chapter analyzing the premillennialists' special (sympathetic) views toward Jews and the early Zionist movement. He also includes a detailed discussion of the premillennialists' role in the fundamentalist-modernist controversy of the 1920s; his thoughtful comments add considerably to our understanding of fundamentalism, a movement that continues to attract scholarly attention.

The author's exploration of the origins of post-Civil War premillennialism is conventional and too brief. He needs to explore much more thoroughly the powerful millennial strain in Calvinist thought in the colonial and early national eras and to seek connections between these earlier theological tendencies and the attitudes of premillennialists of a later time. Weber's use of terminology also at times is confusing, especially when he essentially equates dispensationalism—a special variant in the move-

ment he studies—with premillennialism yet also discusses nondispensational premillennialist groups. Although the book has flaws, it remains an important contribution to the steadily growing literature on conservative American Protestantism since the Civil War. It is also a testimonial that the contemporary conservative Protestant community, long regarded with suspicion by professional scholars, is ready to offer its own serious contributions to efforts at reconstruction of the past.

JAMES F. FINDLAY
University of Rhode Island

ROBERT C. TWOMBLY. *Frank Lloyd Wright: His Life and His Architecture*. New York: John Wiley and Sons. 1979. Pp. x, 444. \$19.95.

Frank Lloyd Wright, who passed away twenty years ago, was one of this nation's—and the world's—most important practitioners and theoreticians of environmental planning and design, firmly implanted as much in the popular mind as in scholarship. As Robert C. Twombly amply shows, Wright was one of those larger-than-life figures whose work and thought continue to speak to us with clarity and feeling. The various currents of Wright's career originated in the nineteenth century and crested at consistently more creative levels in the twentieth. They were fed by hopes and fears concerning Western civilization as evident today as a half-century ago.

Wright's achievement was to evolve out of his own genius, personal experience, and wide reading in the seminal thought of his age—for example, Henry George, Thorstein Veblen—a "radical" interpretation of society. Simultaneously, he formulated a "modern" understanding of environmental forms—the house, community, city, region—incorporating this thought. Most distinctive, of course, was the design quality of his built structures. These were as technologically innovative as they were functionally and visually satisfying to users and viewers. He left a legacy in concept and artifact that will continue to be important so long as the issues that concerned him remain to challenge us—such as unplanned urbanization, a declining environmental quality, wasteful use of energy, need for sound and attractive low-cost housing, and international cooperation based on regionalism.

Among a large and growing literature, Robert Twombly's biography remains the most complete one-volume description of the person and his work. (This is a substantial revision of Twombly's earlier study, *Frank Lloyd Wright: An Interpretive Biography* [1973].) In fourteen dense, well-written, and illustrated chapters, organized chronologically and topi-

cally, the author consistently holds the reader's attention even as he manages to keep it fairly equal balance several different themes explaining Wright's life and work.

What we are offered in addition to an account of a life replete with personal tragedy and enormous professional success—early experienced—is a very perceptive analysis, based on exhaustive documentation, of different aspects of Wright's career. The analysis includes the psychological motivation of Wright's work, the historical forces that were reshaping such basic institutions as the family, Wright's consistent efforts to devise for himself and others a usable social and urban theory, and a sensitive description of the design composition of Wright's architecture, always relating it to the other themes being considered.

Twombly is as interested in the process and technology of planning as in the design attributes of the product itself. The reader is informed as to who Wright's clients and patrons were, the various technological innovations he pioneered in heating and air conditioning, the use of new materials in construction, the truly "radical" and "modern" implications of the interior spaces he created in private residences and commercial office buildings, his remarkable plans for governmental centers such as the Marin County Civic Center, the use and shape of the land as an integral feature of the design, and the relationship between Wright and those many others involved in the creative and cooperative process of planning and design.

In sum, this is an important work about an unusually creative and courageous American who has given us so much to appreciate and to think about regarding the quality of this nation's environmental past and its future.

ALBERT FEIN
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RICHARD E. TITLOW. *Americans Import Merit: Origins of the United States Civil Service and the Influence of the British Model*. Washington: University Press of America. 1979. Pp. vii, 343. \$11.25.

Confucian teaching tells us that "the essence of righteous rule is moral guidance." In Western societies, as in China, civil service reform typically has sought to bring some semblance of moral authority to the everyday conduct of governmental affairs. By the time reformers in the United States were strong enough to press their demands with some reasonable chance of succeeding, the British civil service had progressed to a point where "righteous rule," if not assured, at least had routed the worst of the

spoilsmen and other civic predators. The Americans carefully examined the systems whereby several nations managed their governments; but they turned to the British, in the end, for practical guidance in developing the civil service system that the Pendleton Act authorized in 1883.

In this study of foreign influences on American civil service, Richard E. Titlow first discusses briefly the Chinese, Prussian, and French models, then offers a chapter survey of the British experience with civil service and reform at home and in the empire. There is nothing new in the recapitulation of American administrative history that follows, although it is useful in bringing the reader well informed to the main thrust of the book: an examination of the interaction between British and American reformers in the years after the Civil War. Titlow observes that the movement in the United States should be viewed in the context of Anglo-American developments, not exclusively as an American political phenomenon. Beginning with Charles Sumner and Thomas A. Jenckes, American reformers consciously patterned their efforts and proposals after the British models, and the influence continued into the actual administration of the system after passage of the Pendleton Act. Indeed, that landmark measure incorporated all the main elements of the British administrative system, and the American system today still strongly reflects the debt to the "Old World."

Americans Import Merit is a competent and comprehensive, if hardly inspired, administrative history, developed from an impressive array of resources on both sides of the Atlantic. In the end, however, the author's thesis regarding the interaction of British and American reformers continues to rest largely on inferences and common sense. The problem is not so much his as it is the dearth of specific materials to document the relationship (which he readily acknowledges), particularly personal correspondence containing specific interchanges of views. Doubtless, this lack also helps to explain another weakness of the book—a failure to interweave the two national experiences in such a way as to elaborate significantly upon Robert Kelley's delineation of an Anglo-American community in the nineteenth century. Titlow knows the community existed; his practice of treating its two elements separately does not bring it into clear perspective.

We have here a full and well-documented history of the origins of the American civil service movement. Had the author done some badly needed cutting and revising or had the book fallen into the hands of a good editor, we might also have a history to appeal to readers other than specialized students of the subject.

JOHN G. SPROAT
University of South Carolina

GLENN C. ALTSCHULER. *Andrew D. White—Educator, Historian, Diplomat*. Ithaca, N.Y.: Cornell University Press. 1979. Pp. 300. \$17.50.

Along with Eliot and Gilman and Harper and Washington, the name of Andrew Dickson White has ranked high among the educational entrepreneurs of the Gilded Age. His successor as president of Cornell University, Charles Kendall Adams, once wrote to him, "When we are all passed away the thoughtful scholar who visits these hills will pronounce blessings on your name." The judgments that Glenn C. Altschuler pronounces on White make a mockery of Adams's pious forecast. His book consistently diminishes the complexity of the challenges White confronted across his long and varied career. Above all, the creation from scratch of a large, high-quality, coeducational, nonsectarian public university in the cockpit of post-Civil War educational politics was an organizational chore of awesome subtlety for a man of White's genteel background and soaring ideals. It merits an assessment of equivalent subtlety. Hugh Hawkins' account of Eliot's similar (though in many ways less complicated) achievement at Harvard springs to mind. While Altschuler treats White's years in Ithaca with factual care, his interest in the man clearly lies elsewhere. Early on, he opens hostilities with White's nineteenth-century mind.

White shared his generation's faith in linear human progress and believed that education and trained reason were important means to this end. His biographer uses these convictions as the standard for a relentless critique of White's actual behavior as an educator and public servant. The result is an indictment of White's talent for compromising his own ideals in the interest of survival and personal fame. The portrait emerges of a self-serving, disingenuous administrator who shunned authentic female equality, championed didactic Christianity, was guilty of occasional intellectual dishonesty, and tolerated crimes against academic freedom. White earns even lower marks for his ministries in Berlin and St. Petersburg. His failure to perceive emerging conflicts of interest between the United States and the new Germany he so greatly admired, his inability to understand Russia, and his inept handling of specific controversies such as the Bering Sea seal dispute of 1892 and American-Germany rivalry in the Philippines in 1898 betray in Altschuler's eye a persistent capacity for naive, myopic blundering.

Finally, Altschuler finds little to admire in White's personal character and social attitudes. His blatant elitism, racism, and shortcomings as a husband and father receive close attention. It is as historian that he fares best, but even here he is faulted for his moral absolutes and mental confusion. At

the heart of White's problems, we learn, were his deeply conservative vision of a good society, his faith in education as a panacea for its imperfections, and his stubborn rhetorical commitment to progress in the teeth of all evidence to the contrary. The onset of world war in White's last years provides the climactic refutation of his life-long hopes. Death and destruction clinch the analysis. History is what happens to White's presumptions, and he is flattened by the outcome.

White has waited a long time for an adequate biographer. Despite its caustic tone, Altschuler's effort is well documented, aggressively argued, and thematically consistent. It is far from a definitive "life," but one senses that its publication must be a very satisfying victory for the author over his subject.

GEOFFREY BLODGETT
Oberlin College

RICHARD W. FOX. *So Far Disordered in Mind: Insanity in California, 1870-1930*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xvi, 204.

In the recent revival of interest in social deviance and institutional solutions to human problems, flimsy generalizations about mental illness and mental institutions have been fashionable. The kind of work that Richard W. Fox has done in this fine monograph has been in short supply, which is why it is an important book. His conclusions, though informed by sophisticated awareness of social theories, rest on detailed, careful study of data; they reflect creative use of source materials. With the aim of delineating "the social, institutional, and professional network within which thousands of deviant individuals were officially declared insane" and of elucidating "the civil commitment process itself" (pp. 3-4), Fox analyses the commitment records of the insane for San Francisco City and County from 1906 to 1928. Although the scope of his study is broader, this material is its heart.

Working with a sample of the commitment records, Fox draws a social profile of persons considered insane. On the whole he handles with care the sometimes small portions of his sample that represent certain social attributes. Sometimes, however, his penchant for statistics gives an exaggerated accuracy to data that involve complex issues and problematic evidence, such as the percentages purporting to show how many people considered for commitment actually met the legal requirements for involuntary hospitalization. Fox's general conclusions, which are convincing, tend to corroborate known patterns of state hospital populations: there was a high concentration of working-class persons, the foreign born, and women, and the hospitals were dumping grounds for troublesome, indigent,

homeless people—the aged, the senile, and the mentally handicapped, as well as the “bona fide” insane. Compared with other states, California seems to have been exceptional in the high proportion of the total population that were committed to mental hospitals, a situation caused in part by the absence of alternative institutions that were common elsewhere.

Fox is critical of current revisionist approaches, most notably Thomas Szasz's stance that mental illness is merely a social construct and David Rothman's thesis that mental institutions served primarily as devices for social control. In studying the commitment process, Fox believes he is analyzing a real, not a pseudo-problem (as Szasz would have it), and he perceives mixed motives for commitment and multiple social roles for mental institutions. Yet he is himself not exempt from polemicizing. His work has an antipsychiatry bias that blinds him to the full significance of evidence indicating that psychiatrists were the only influential organized group opposing the use of state hospitals in California as human warehouses. After virtually making the same point, he attributes the effort of psychiatrists to gain control over admissions to self-interest, their desire to build more hospitals and provide more jobs for themselves. But could not their self-interest also encompass a need to exclude inappropriate patients from their overcrowded hospitals so that they could gain professional respect by acting like physicians and helping patients to recover rather than having to act like prison wardens? Fox himself shows that laymen and general medical practitioners forced the hospitals to accept the people they sent and thus made them repositories for all sorts of social deviants, against the wishes of hospital superintendents.

It is to Fox's credit that one can thus argue with his interpretations. Notwithstanding his admiration for Michel Foucault and like-minded critics, Fox's work stands apart from theirs, especially Foucault's, in presenting the evidence upon which his conclusions are based and thereby enabling readers to draw their own conclusions. This is no small distinction today when so many writers produce ideological or political tracts disguised as research.

NORMAN DAIN
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NORMA FAIN PRATT. *Morris Hillquit: A Political History of an American Jewish Socialist*. (Contributions in Political Science, number 20.) Westport, Conn.: Greenwood Press. 1979. Pp. xi, 272. \$18.50.

Norma Fain Pratt's *Morris Hillquit* narrows a major gap in the historiography of the American left, of-

fering the first full-length study of a man who was a veritable fixture in the socialist movement for some forty years. Hillquit, after playing a key role in the rebellion against DeLeonite dominance of the Socialist Labor Party in 1899, led his defeated SLP faction into union with the Social Democratic Party of Eugene Debs and Victor Berger to create the Socialist Party of America in 1901. And from the SPA's founding until his death in 1933, he exercised enormous influence on party affairs, serving for many years as a pivotal member of the national executive, representing the SPA at international socialist gatherings, carrying the party standard into several election campaigns in New York, and engaging actively in propaganda work as a writer, speaker, and debater. Paralleling Hillquit's political career was his busy legal practice, which saw him defending radicals in civil liberties cases and acting as counsel for fledgling unions in the turbulent garment industry.

Pratt relates Hillquit's story in the mode of traditional political biography, focusing on Hillquit the public figure and never straying far from straight narration. Accordingly, there is little here that is novel or provocative. But she gives us a thoroughly researched account in clear, if somewhat flat, prose and does not shrink from critical judgments. While detailing Hillquit's abilities and contributions, Pratt also reveals her subject's manipulative and opportunistic tendencies, his penchant for the good life, and his pronounced elitism. She develops this negative side of her portrait particularly in her discussion of Hillquit's paradoxical relationship with his political and professional base, New York's working-class, Jewish immigrants. A product of the Yiddish-speaking milieu of the sweatshops, Hillquit was nevertheless an assimilationist with no faith in the viability of a distinctive *Yidishkeit* and a man of bourgeois sensibilities who could neither identify with nor completely trust rank-and-file workers.

Unfortunately, Pratt declines to plumb beneath the surface of such contradictions and render more of Hillquit the man. Her book would have been far better had she provided it with a thematic core that attempted to explain motivation and perhaps place Hillquit's experience in some larger context. As it is, her treatment brings us no closer to a resolution of the knotty question of why socialism failed to establish itself as a vital force in American society, a question that Pratt states to be one of her study's more general concerns. The pity of it is that a convincing motif was within the author's grasp. Interpreting Hillquit's behavior as the consequence of thwarted youthful ambitions to achieve middle-class status in his native Latvia, a subsequent striving for upward mobility in America, and a desire to preserve the rewards of that climb would have been suggestive and quite plausible.

Despite its shortcomings, however, this study is a welcome addition to the literature.

L. GLEN SERETAN
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CRYSTAL EASTMAN. *On Women and Revolution*. Edited by BLANCHE WIESEN COOK. New York: Oxford University Press. 1978. Pp. xiv, 388. Paper \$4.95.

WILLIAM L. O'NEILL. *The Last Romantic: A Life of Max Eastman*. New York: Oxford University Press. 1978. Pp. xix, 339. \$14.95.

For historians who would use biographies not as chronicles recording the lives of "great men" but as vehicles illuminating important facets of the American experience, Max and Crystal Eastman are apt subjects. The offspring of two Congregational ministers, both son and daughter were strikingly handsome, extraordinarily vital individuals whose personal and intellectual lives—Max's especially—intersected some of the most significant political movements of this century. Activists both, they helped in pre-World War I years to forge that union of political radicalism and personal (and sexual) liberation so characteristic of the cultural rebellion that flourished in Greenwich Village and elsewhere. After the war, Crystal, who, as an attorney, had pioneered in the field of labor legislation and industrial safety, continued her career as a journalist and crusader, working tirelessly on behalf of feminism, socialism, peace, and civil liberties until her death in 1928 at the age of forty-six. Her articles and essays from these years have been perceptively edited by Blanche Wiesen Cook, who intends a biography when all family papers are available. Even these brief journalistic pieces convey a militance sustained by a tough-minded intelligence that gives the writings on feminism especially a contemporary relevance quite apart from their utility as historical documents, for Crystal Eastman knew that a feminist revolution, like a socialist revolution, was, in Cook's words, "a process not an event." Although scholars of the period have been aware of the broad parameters of Crystal's activism, we are indebted to Cook and to Oxford for making so readily accessible writings that will better enable us to evaluate her significance.

Max's career was longer; he died in 1969. It was also more complicated by virtue of its ideological span, for Eastman, although consistent in his vocations as journalist, poet, and libertarian, moved from the joyous radicalism of the prewar *Masses* to unabashed Trotskyism in the twenties, a "premature" anti-Stalinism in the thirties, and finally in the forties and fifties to a vigorous anticommunism and defense of free enterprise espoused from the

pages of the *National Review* and *Reader's Digest*. This intellectual journey coupled with a richly textured life—it was Max Eastman who smuggled Lenin's Testament out of Russia—invite a first-rate biography. Such a study would combine a compelling portrait of a compelling individual with a penetrating understanding of a mind grappling with important intellectual and cultural concerns. (The relationship between political democracy and socialism and the use—or misuse—of art as a political weapon were but two of the many problems that aroused Eastman's intellectual passion.) O'Neill's study, despite its strengths, disappoints on both counts. As biographer, he provides us with a brief foray into psychohistory. At issue are Eastman's intense linkage to an extraordinary mother and sister, his inability to form "mature" emotional attachments to the many other women who filled his life, and his search for father figures in Dewey and Trotsky—father figures whom Eastman, according to his biographer, always felt compelled to "overthrow . . . with his pen." But speculations about an unresolved Oedipal crisis do not substitute for the painstakingly crafted portrait of the secret self that Leon Edel argues is the art of biography.

Given the fullness of Eastman's own memoirs, it is perhaps understandable why O'Neill chose to concentrate on the writings and Eastman's relation "to what might be called the social history of ideas." Here, too, the results are mixed. Given Eastman's preoccupation with Marxism and its implementation, contemporary responses to his works are important. O'Neill, however, allows quotations from reviews to figure disproportionately in his analysis. By the same token, sales figures tell us something about the culture but little about the intellectual quality of the publications themselves. With respect to content, O'Neill's discomfort in the handling of Eastman's amorous meanderings is also evident in the treatment of the ideological course transversed in the works themselves. Perhaps as a result, comments intrude that reveal more about O'Neill's political preferences and mode of analysis than they do about the contours of Eastman's mind and the intellectual influences and tradition that shaped it. For example, O'Neill writes of Eastman's later years, "Had he not become so conservative, he might have completed his argument in this way. Socialism remains a beautiful dream. In real life it has been proven unnecessary by the welfare states of Scandinavia." To object to such judgments, whether stated explicitly as above or implied—as with the prefacing of a chapter on Eastman's criticism of Stalinism and Marxism during the forties with a quotation from *The Gulag Archipelago*—is not to ask for "value-free" history devoid of any trace of current preoccupations or personal commitment. Rather, it is to ask for the kind of thoughtful, pene-

trating analysis that allows us to understand and assess the man, his ideas, and his concerns and to do so in such a way as to enhance our grasp of larger issues, such as the nature of radicalism in America and the role of the intellectual in American life. Thus, while *The Last Romantic* may serve as an introduction to Max Eastman's life and thought, it will hardly suffice as the last word.

JANE DE HART MATHEWS
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Greensboro

MICHAEL DE L. LANDON. *The Honor and Dignity of the Profession: A History of the Mississippi State Bar, 1906-1976*. Jackson: University Press of Mississippi, for the Mississippi Bar Foundation. 1979. Pp. xi, 195. \$17.00.

The Mississippi Bar Foundation, the research arm of the Mississippi State Bar Association, commissioned Michael de L. Landon to prepare this modest history of the state bar. It is essentially an overview of the history of the association, largely drawn from its own published reports. Nonetheless, with the dearth of good material on the history of the state bar associations, it is welcome, despite its obvious limitations. To a great extent it would be more accurate to identify this book as a review of the annual meetings of the association and a survey of the major issues facing the association in the years covered. The author makes no attempt to present this work as definitive and in fact suggests that deeper and more thorough research is needed. At the same time, Landon is frank in showing the shortcomings of the association in dealing with such delicate issues as the rights of black Mississippians and prohibition. What one looks for in vain is any interpretation or attempt to relate the experience of the Mississippi State Bar Association to that of other state associations in the same region.

Similarly, this reviewer wishes that the opening pages on the predecessors of the Mississippi State Bar Association were more fully explored, as well as the activity of Mississippi lawyers in the American Bar Association from 1878 to 1906.

NORBERT BROCKMAN
Marianist Training Network

MICHAEL O'BRIEN. *The Idea of the American South, 1920-1941*. Baltimore: Johns Hopkins University Press. 1979. Pp. xvii, 273. \$16.00.

Michael O'Brien has attempted in *The Idea of the American South, 1920-1941* to explore why Southern intellectuals between the two World Wars "felt the need to express themselves in terms of a Southern

tradition" (p. 27). Such a topic is appropriate for intellectual history, and O'Brien raises our expectations for its fruitful pursuit with his introductory chapters, in which he seems not only to be aware of the multiplicity of Souths and southern identities but also to want to bring to them an interesting combination of historical, literary, and sociological methodologies. I particularly applaud his intention to relate the idea of the South to broad nonnational, nonsectional intellectual currents: Romanticism, Victorianism, and modernism.

Unfortunately, O'Brien only gives biographical sketches of six Southerners: Howard Odum, John Wade, John Crowe Ransom, Allen Tate, Frank Owsley, and Donald Davidson. Odum, to whom he gives the most detailed study, represents the New South; the others had connections with the Nashville Agrarians; all spent their careers in or in the shadow of the academy. Wade was a poor choice as the transition figure; William Alexander Percy would have served better. Each sketch ends abruptly at 1941, except that of Davidson, whose White Citizens Council activities apparently proved too tempting to be ignored. O'Brien explores Tate's idea of modernism and the dissociation of sensibility without examining Tate's most complete and least reduced embodiment of that idea and its relation to the South in his novel *The Fathers*. O'Brien fails to see the extent to which the South was primarily a metaphor for Ransom's essentially esthetic concerns. Indeed, of the Nashville group, only Davidson had a deep commitment to the Agrarian South as an actuality, a commitment expressed in social action that proved to be the source of his tragedy.

Such essays—and the ones on Odum, Wade, and Owsley are excellent—do not define the complex idea of southern identity. Their subjects represent only Georgia, North Carolina, Tennessee, and Kentucky. Missing are the "Deep South," the South of the Gastonia strikes, of Erskine Caldwell, of Broadus Mitchell, of Lilian Smith, and of Agee and Walker's studies. And a study of the idea of the South using, among others, literary sources but ignoring William Faulkner is lacking one of the forces that imprinted a special view of the region on Southerners and non-Southerners alike.

Furthermore, the ideational structure of the essays is marked by O'Brien's tendency toward Hegelian oppositions, without clear syntheses: Vanderbilt versus Chapel Hill, Agrarianism versus New South, regionalism versus nationalism, idealism versus atomization, race versus modernism. Such dichotomies have the ultimate effect of falsifying simplifications. The author's use of Karl Deutsch's abstract definition of nation and his use of Romanticism as though it had an exact and recognizable referent further weaken this work.

As an Englishman, O'Brien has, he thinks, "no partisanship on the competing moral claims of these various versions of Southern identity" (p. xi), a position that allows him to play down the central role of the Negro to a remarkable extent. He also has too limited a knowledge of the realities of the region whose idea he would define.

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PAUL BONNIFIELD. *The Dust Bowl: Men, Dirt, and Depression*. Albuquerque: University of New Mexico Press. 1979. Pp. xi, 232. \$12.50.

One of the bleakest and most enduring images of the Great Depression is the drought-stricken Great Plains. In this work Paul Bonnifield examines the heartland of the dust bowl, the Oklahoma and Texas panhandles and adjacent areas of Kansas, Colorado, and New Mexico. The author knows the region well and admires the people who withstood drought and depression and remained on the land. In researching their story he discovered a need to revise conventional views of the dust bowl.

Bonnifield shows that, when the depression struck, the area was still a frontier and was changing from diversified crop and livestock farming to a concentration on wheat. This shift was not caused by the high wheat prices of World War I, but came in the 1920s, primarily because of the mechanization that made large-scale production possible. Unfortunately, there was no concurrent advance in the methods of controlling wind erosion. Indeed, farmers, government, and scientific agriculturalists all completely overlooked the wind's potential danger until the dust bowl was well established. When wheat planting was curtailed after prices plunged in 1931, large uncultivated areas were left exposed to the ravages of wind and drought.

Although Bonnifield does not minimize the hardships of dust-bowl times, he points out that in many respects the area's business and agricultural conditions were no worse than those of other sections. His account of the dust bowl as an expanding petroleum frontier is particularly informative.

The author condemns most federal efforts to cope with the dust bowl, especially the land-use planning of the Agricultural Adjustment Administration and the Resettlement Administration, which aimed to remove submarginal land from cultivation and return it to grazing. He contends that the area's soils should not have been classified as submarginal and argues that wheat production is a viable economy for the region if practiced with strict wind erosion control. He credits dust-bowl farmers with developing techniques and implements for such control

in contrast to the poor record of the Soil Conservation Service. These arguments, backed by ample technical information, deserve consideration in the literature of the dust bowl.

Unfortunately, the book lacks perspective regarding the intent of federal policies. For example, the author sees the AAA, the RA, and the credit and relief agencies working deliberately to drive people off the land in accordance with a federal grand design for land use. This exaggerates the coordination of those agencies and also distorts the overall program of the Resettlement and Farm Security Administrations. There is also an erroneous assertion (p. 118) that the "Jerome Frank-Rexford Tugwell faction" gained control of the AAA. While Bonnifield is concerned with the local applications of federal policy, these local effects need to be seen in a broader context. Bonnifield's heavy reliance on local sources, truly a strength in much of his book, does not supply the needed overview.

On the whole this is an informative book on local dust-bowl conditions. It is generally well written and includes some good illustrations.

PAUL E. MERTZ
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Stevens Point

GEORGE NORRIS GREEN. *The Establishment in Texas Politics: The Primitive Years, 1938-1957*. (Contributions in Political Science, number 21). Westport, Conn.: Greenwood Press. 1979. Pp. xiii, 306. \$22.50.

In this first thorough study of mid-twentieth-century Texas politics, George Norris Green begins by analyzing the historical sources of Texas conservatism: Texas nationalism and states' rights, the South Texas patrón system, the southern heritage of white supremacy and fundamentalism, the western heritage of individualism, and local elites. In the 1930s the state's business establishment strengthened these conservative tendencies as it achieved political dominance on the basis of new oil, banking, and insurance wealth as well as by public relations techniques. Yet in the early years establishment influence remained incomplete and sometimes ineffective.

Green devotes chapters to the state administrations and to the major campaigns of the period. Governors W. Lee O'Daniel, Coke Stevenson, Beauford Jester, and Allan Shivers all received business support but varied in accomplishment. Although O'Daniel proved inept, Stevenson and Jester showed moderation on some civil rights and labor issues. Shivers promoted public services, although primarily with consumer taxes, and supported Eisenhower for president to get the oil tidelands returned from federal to state control. In the 1940s

Texas businessmen opposed Roosevelt's renomination, financed organizations that fought civil rights and labor organization, sought to muzzle the University of Texas faculty, and formed the Texas Dixiecrats—but with only limited results. Congressman Martin Dies pioneered red scare politics later used by local elites in Houston and San Antonio. Business interests proved most successful in avoiding taxation. In the 1948 Senate race, which involved fraud on both sides, Green believes a fair count would have favored Stevenson over Lyndon Johnson. Shivers played upon fears of communism, labor, and civil rights to win reelection over liberal Ralph Yarborough in 1954. Green sees Texas politics turning away from emotional campaigns toward moderation in 1956 and 1957, when Johnson and Sam Rayburn kept the state Democratic Party loyal to the national party, Price Daniel was elected governor, and Ralph Yarborough won a Senate seat. In a final chapter, the author surveys Texas politics in the 1960s and 1970s. He concludes that establishment states' rights rhetoric of the forties and fifties represented a failure to meet responsibly the needs of the state.

Green has researched widely and his analysis is generally well balanced. While the author does not employ statistical methods, he draws upon such studies to conclude that Texas voting followed socioeconomic lines through 1956. Some topics, however, seem worthy of additional discussion, for example, the incipient Republican Party and the impact of religion and culture on Texas voters. Relationships within the establishment remain vague at times, so it is not clear why wealthy Texans divided and gave some support to liberal candidates such as Jimmy Allred and Ralph Yarborough. Finally, more comparison of Texas politics with those in other southern states would have enhanced the reader's perspective. Yet, if this volume is not the final word on all aspects of mid-twentieth-century Texas politics, it will be the basic study from which any future investigations must begin. Certainly it should stand with the better political studies of southern states in this period.

ALWYN BARR
Texas Tech University

GRZEGORZ BABIŃSKI. *Lokalna Społeczność Polonijna w Stanach Zjednoczonych Ameryki w Procesie Przemian* [A Local Polonia Community in the United States of America in the Process of Change]. (Biblioteka Polonijna, numer 1.) Wrocław: Ossolineum. 1977. Pp. 207. 42 Zł.

This largely contemporary review of the small, heavily Polish suburb of Wallington, New Jersey, is a welcome addition to the growing bibliography of

Polonia communities. Similar to the recent studies of the group in Los Angeles by Neil Sandburg and the one in Detroit by Paul Wrobel, this work by a young Polish sociologist of the Polonia Research Institute in Cracow is a solid, meticulous, and comprehensive sampling of changing Polish-American generational attitudes and values in a town satellite to the industrial centers of Passaic and Clifton. Although the small size of his subject area—seven thousand Polish-Americans in a total population of twenty thousand—does handicap the author somewhat (he never adequately explains the selection of Wallington), the suburb does have a significance greater than its numbers would suggest. The community is really two ethnic settlements, one consisting of older working-class inhabitants who arrived around and just after World War I and another made up of a more affluent middle-class contingent who entered after World War II. Thus Wallington's Poles span the entire suburbanization process and Grzegorz Babiński utilizes the differential to advantage.

Babiński's conclusions are rather conventional, although perhaps not to his East European audience; his revision of Warner's social mobility thesis and Reisman's *Lonely Crowd* will not surprise American sociologists, historians, or current students of ethnicity. Polish ethnic cohesion has changed its character and composition rather than disappeared; institutions are no longer central to group identity, which is now more private, personal, and individual; intermarriage is more acceptable, although within rather than outside of Roman Catholicism; upward job mobility has occurred recently, particularly between the second and third generation, and younger members value education more than the earlier working class. Among other late ethnic critics, Andrew Greeley summarized many of these points in his recent *American Catholic* (1977).

American historians will find the work less valuable than sociologists, as Babiński ignores almost all of the important studies outside his discipline, although he uses many historical records and logically periodizes his review. Oddly, while an Old World academic, he has nothing to say about the European origins of his subjects' attitudes. His bias is evident, however, in referring to the pathology of assimilation, which is asserted rather than demonstrated.

Despite these limitations, this reviewer found two of Babiński's findings in particular well worth the effort in reading the work. First is his prediction (based in part on preliminary conclusions) that Polonia is at the threshold of a cultural, rather than political or nationalist, renaissance. Second, and particularly outstanding, is his tracing of how the group has been so adaptable to change, especially

to crises. He recounts the dramatic irony that Poles out of work survived the depression by starting family businesses—thus proletarians became bourgeois when capitalism nearly failed! Despite the regrettable absence of a map and due to a totally inadequate English summary, the work merits a full English translation for an audience that needs such ethnic community studies.

VICTOR GREENE
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BERT COCHRAN. *Labor and Communism: The Conflict that Shaped American Unions*. Princeton: Princeton University Press, for the Research Institute on International Change, Columbia University. 1977. Pp. xiv, 394. \$25.00.

The author, once an auto worker and UAW official and now associated with the Research Institute on International Change at Columbia, has written the most detailed and generally the most sophisticated book on this subject yet published. He does not argue the thesis implied by the subtitle. That is fortunate because, while the Communist Party certainly affected unions and unions affected the party, their relationship was not "the conflict that shaped" this country's unions.

The book's overwhelming emphasis is on the period from about 1929 to the early 1950s, years when the party's line changed frequently, usually only by a few degrees but in 1939 and again in 1941 by sudden and complete reversals of direction. The unions' problems, needs, and strategies also changed but were far more consistent than the party's. In the CIO's early days some unions welcomed Communists as experienced and dedicated organizers or at least warily tolerated them. Some unions were becoming anti-Communist even before the Nazi-Soviet pact, and, while the pact was in effect, tension between the principals became intense. After the German invasion of Russia and the Japanese attack on Pearl Harbor, the Communist Party played the role of Dr. Win the War so vigorously, even advocating a piecework wage system (the euphemism was "incentive pay") that many union leaders were furious about what they regarded as Communist subversion of labor's interest. When the Cold War began to take shape, union anti-Communism grew rapidly and by the early 1950s the party had lost almost all influence in the important unions. Bert Cochran splendidly describes the complexities and nuances of these party shifts and their effects on the relations of the party with the unions.

Other strengths are the brief interpretive observations scattered throughout the narrative, sometimes about party-union relations and sometimes about

other aspects of the labor movement. Many of these acute comments result from the author's association with the UAW and with working men. He is indeed a "labor intellectual." But the final chapter, an essay entitled "Postscript: Concepts of Labor Development," does not impress this reviewer. Its ambitions to be cosmic get it into trouble, and its petty criticisms of the Perlman thesis (p. 336) are at odds with what the author later writes (pp. 342-43), which is entirely consistent with what Perlman said in his classroom in the late 1940s.

As intelligent and useful as this book is, it has its flaws. The UAW was the CIO's largest union, but it receives disproportionate attention. Especially in the several sections on the auto workers, the reader has difficulty keeping his eyes on the forest because of the attention to the trees and even the leaves and branches. The style in general is adequate, and some sentences are wonderfully pithy and clean. Others, however, are pitiful humpbacked things that unnecessarily make the reader stumble and reach for his editorial blue pencil. Cochran also has a weakness for "literary" pretensions; some readers will be put off by the use of relatively uncommon French terms and such arcane allusions as, for example, a "Gadarene plunge down the abyss" (p. 132). Further, the absence of a bibliography is a real nuisance; the backnotes do not even provide full bibliographical information after the first citation.

Insufficient perspective is a most difficult problem for historians of the recent past. For the most part Cochran avoids the language of partisanship that often appears in the writings of historians who were contemporary observers of events treated in their works. He is often critical of major party leaders and important unionists, whether anti-Communist or not. For example, neither Browder nor Reuther escapes censure. Yet one closes the book with the feeling that the Communists receive the harsher treatment.

Cochran has written the most emotionally detached book on this subject yet to appear, but there still is some distance to go. Probably it will be a long time before an American will be able to be dispassionate on this part of our history. Meanwhile, it would be interesting to read the efforts of someone from another nation.

DAVID A. SHANNON
University of Virginia

GEORGE Q. FLYNN. *The Mess in Washington: Manpower Mobilization in World War II*. (Contributions in American History, number 76.) Westport, Conn.: Greenwood Press. 1979. Pp. xi, 294. \$17.95.

In 1944, when President Franklin D. Roosevelt hinted that Paul V. McNutt might make a good

running-mate in that year's election, Secretary of the Treasury Henry Morgenthau replied, "I will crawl from here to the Capitol on my stomach and back again if it will keep you from taking McNutt." Apparently, neither Roosevelt nor Morgenthau took the other's suggestion seriously. George Q. Flynn's study of wartime manpower mobilization helps explain why there was never any chance that McNutt would be the nominee: as Director of the War Manpower Commission he had by 1944 alienated too many powerful interest groups and political leaders.

Flynn describes the considerations that led to the creation of the War Manpower Commission in April 1942, the work it did, and the problems it faced. He offers an appraisal of the controversies over the "work or fight" order, national service, the deferment of farm workers, the proposed "father draft," and reconversion. In addition, Flynn discusses the limited role McNutt played in combating job discrimination against blacks and women. The author dissects the relationships between the WMC and various pressure groups, particularly labor unions and the armed services, that had a direct stake in manpower policies, and he lays bare the truly Byzantine relationships between McNutt and several bureaucrats, particularly Secretary of War Henry Stimson and Secretary of the Interior Harold Ickes, with whom he had to contend.

Flynn maintains that McNutt's policies were based on a belief in voluntarism and localism. He "combined an almost reactionary commitment to the preservation of traditional values with a vision of the war as a new beginning" (p. 12). This commitment limited the impact of manpower mobilization, and nowhere was this limitation clearer than in the case of blacks and women. Manpower policies "pushed blacks into the unskilled laboring class"; indeed, McNutt "played a major role in this campaign of smothering the black drive for equality" (pp. 167-68). Similarly, the WMC did little to contradict the common assumption "that women should not be considered equal to men in economic matters" (p. 173). Yet Flynn concedes that the failure to achieve "reform" was "almost inevitable" given the power of organized farmers and workers, the widespread prejudice against blacks and women, the conservative pulse of Congress, and the special pleadings of the Selective Service System. In view of these constraints, Flynn concludes, "what seems remarkable . . . is that McNutt achieved anything worth remembering" (pp. 255-56).

While the author has a thesis, he does not push it further than his evidence allows. That evidence is drawn heavily from a wide range of manuscript collections, including the diaries of Ickes and Stimson, the papers of McNutt and the War Manpower Commission, and the files of Selective Service Di-

rector Lewis B. Hershey. Flynn has provided an informed and thoughtful account, one that nicely supplements Albert Blum's *Drafted or Deferred* (1967) in explaining the political and social, as well as the economic, dimensions of manpower mobilization during World War II.

RICHARD POLENBERG
Cornell University

MONTE M. POEN. *Harry S. Truman Versus the Medical Lobby: The Genesis of Medicare*. Columbia: University of Missouri Press. 1979. Pp. xii, 260. \$20.00.

This volume is an examination of Harry Truman's efforts to obtain a national health insurance program and the factors behind his inability to do so. Principally, that failure was the result of two matters. One was opposition from the American Medical Association and the allies that organization generated. A second factor was the general climate of public opinion.

In the former case, the AMA once favored compulsory health insurance, but that ardor cooled rapidly in the five years after World War I. By the Truman era this country's richest and most influential post-World War II lobby had taken the position that all such schemes were "socialistic" and "foreign" to the American system of government—indeed were incompatible with protection of the basic liberties of the American people. The AMA-led opposition coalition was sufficiently powerful that the likelihood of gaining enactment of a national health insurance program was virtually nil.

The general climate of opinion reinforced and provided sustenance for that opposition. By 1949 the extreme, indeed frantic, anti-Communist mood that dominated the American scene meant that any legislative proposal that could be identified with the word "socialism" was immediately suspect. National health insurance was so defined by its enemies. Truman was a realist. This state of affairs, combined with the Korean War and unfavorable returns in the 1950 fall elections, persuaded him to move toward a less ambitious plan for government health insurance, one that would apply essentially to the aged. While this decision obtained no real result during the Truman years, Monte M. Poen understands it to mark the beginning of the long legislative battle that reached its culmination with the enactment of Medicare in 1965. This story, notes the author, is not, therefore, one of defeat but, at least in the long run, one of victory—a highly debatable interpretation.

Poen to the contrary, this aspect of Truman's administration can easily be read as a chronicle of failure. If Truman was the first president to provide health reformers with a mandate to act, that man-

date had little consequence. To understand Medicare as the belated conclusion of Truman's health-care efforts is to acknowledge a tragic history of the medical lobby's continuing ability to deny Americans the medical protection they deserve and to point up anew the highly restricted perception of what constitutes appropriate protection that has long prevailed in Washington. In the latter, Harry Truman was no exception. His health-care vision was woefully inadequate for the needs of the time.

This volume is well researched and well written. Despite my quarrel with Poen's perspective on Truman, I regard the work as a valuable contribution to the history of the Truman administration as well as to the history of public health. With respect to interpretation, however, I suspect that many scholars will find, as I do, that the author's depiction of Truman is far too favorable and the case for that view much too weakly drawn.

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CEDRIC BELFRAGE and JAMES ARONSON. *Something to Guard: The Stormy Life of the National Guardian, 1948-1967*. New York: Columbia University Press. 1978. Pp. ix, 362. \$19.95.

Something to Guard: The Stormy Life of the National Guardian, 1948-1967 is as much a history of the struggles of the American left in the postwar period as it is of that particular publication. The authors, Cedric Belfrage and James Aronson, have written an account that highlights their successes and failures as editors of the *National Guardian*. This very personal account reflects a point of view critical of accepted governmental actions. For the almost twenty years of association between the authors and the paper, the *National Guardian* published despite financial problems, governmental harassment, and the deportation of Belfrage. It served a distinct public but provided an alternative to the news that was supplied to the American people.

The paper was often criticized for being uncritical of the Soviet Union, and for that reason it alienated great numbers of liberals as well as conservatives. Belfrage directly confronts these charges by explaining that the policy was to focus on the activities of the American government. He does add, however, that in retrospect charges of repression within Russia should have been given more attention by the staff. The basic dilemma revolved around the fact that the Soviet Union was also the great hope for socialism in the world. Thus, the ideological attachment to a Communist state prevented a more critical stance. In fact, this proved to be a great problem for James Aronson, who left the

National Guardian's editorship in 1967 because of continuing difficulties with and criticism from members of the "New Left." The New Left of the 1960s was much less ideologically oriented toward the Soviet Union than the so-called Old Left of the 1940s.

Despite troubles, the paper had an extraordinary record. It fought against Joseph McCarthy in a way that no mainstream publication did. It highlighted the impending American military involvement in Vietnam long before others paid attention to it. Incidents involving race relations, discrimination, and injustice were constantly addressed. And the *National Guardian* fought unrelentingly to save the Rosenbergs from execution.

The rewards for this work were mixed. By championing causes, the publication was totally involved in presenting its point of view, which was its stated goal. Other goals, however, were never achieved. Aronson, Belfrage, and the *National Guardian's* third executive, John T. McManus, had wanted the paper to become a financially successful, independent publication with a sizable circulation. This never came about. After some initial financial help from Anita McCormick Blaine, the paper was always plagued by monetary problems. The total number of readers also never lived up to the expectations. In fact, in 1967 the paid circulation of the paper was twenty-eight thousand, a far cry from the hundreds of thousands that had been a one-time dream.

In retrospect, although the *National Guardian* never achieved its great readership, it served an important purpose. It challenged the conventional wisdom of the times and left a record of honorable dissent in a time when there was too much conformity.

ALLEN YARNELL
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ROBERT H. CONNERY and GERALD BENJAMIN. *Rockefeller of New York: Executive Power in the Statehouse*. Ithaca: Cornell University Press. 1979. Pp. 480. \$15.00.

For critical or blatantly hostile studies of Nelson Rockefeller, scholars should turn to the prolific accounts by journalists and investigative reporters. Robert H. Connery and Gerald Benjamin's examination of his governorship has offered something more enduring: an analysis of the state of American federalism. As in an anthology they edited for the Academy of Political Science, this book uses New York as a prime example to ponder the ability of state governments "to find solutions to the societal problems" that once inspired remediation from an optimistic nation (p. 18).

The Connery and Benjamin work does for feder-

alism what earlier state and local studies did for American constitutional development, the Civil War and Reconstruction, and, more recently, the New Deal. They accept Donald M. Rober's concept of "positive liberalism" and gladly apply that label to Rockefeller. In a series of well-written chapters, the governor emerges as a master builder and "active-positive" executive. His deep concern for the revitalization of state government amply warranted apprehensions that he was a "big spender." His 1958 election marked a "turning point" in placing "aggressive, problem-solving style in New York's constitutionally strong governorship at a time when state government generally was entering a period of enormous growth and change" (p. 38). Where the federal government lagged, Rockefeller's New York set the pace for medical insurance programs, aid for mass transit, water pollution control, park development, housing, and cultural programs. Converting a moribund state university into a major provider of quality public education was, by most accounts, his major achievement.

Still, the authors argue that politics and federal-state relationships imposed severe restraints on even New York's activist governor. Contrary to other writers, Connery and Benjamin contend that Rockefeller's powers were far from feudal; he did not quite own the legislature. The forty thousand patronage jobs were not all *his* to dispense. The authors, both political scientists, minimize the relationship between Rockefeller's personal fortune and his office, but they do point out the advantages made possible by his great wealth. When trying to get money from Washington, however, he encountered frustration over revenue sharing and was forced to do battle through bipartisan, mutual-interest alliances. Ignoring David Horowitz and Peter Collier's point that Rockefeller's ultimate ambition was thwarted by the tendency of heartland Republicans to see him as the personification of Eastern financial power, Connery and Benjamin argue that he was undone because the conservative, not the liberal, critique was finally decisive. The governor tried to do too much too quickly. Perhaps the final irony is that his passion for building was more easily satisfied in Albany than it could have been in Washington.

The authors relate Rockefeller's reluctance to speak out on Vietnam and Watergate to his need to ensure a friend for New York in Washington. Still, despite wanting the federal government to transfer services to the state, he opposed cutting defense budgets. In tracing his rightward shift, they place more importance on the governor's perception of what was politically feasible for the benefit of the state rather than on his own ambitions. Yet, for New York, Connery and Benjamin acknowledge that the governor's legacy was a budget thrown

askew by the necessity for "back-door" financing. Even the nation's wealthiest state could not solve human needs by "going it alone." Although too often reading like an in-house report of accomplishments, Connery and Benjamin's examination raises important questions for historians trying to fathom the dynamics of American federalism.

HERBERT S. PARMET

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JOHN P. LOVELL. *Neither Athens nor Sparta? The American Service Academies in Transition*. Bloomington: Indiana University Press. 1979. Pp. xviii, 362. \$17.50.

For most civilian scholars the American service academies are probably some combination of riddle, enigma, and puzzle and about as appealing as a Tolkienesque land of wizards and monsters. For John P. Lovell, a professor of political science at Indiana University, the history, contemporary policies, and prospects of the Military, Naval, Air Force, and Coast Guard academies have been career-long interests. *Neither Athens nor Sparta?* presents his years of research in comprehensive, coherent fashion.

Particularly because his study is done on a comparative basis and subtly mixes history and organizational theory, Lovell's work is in a class above the normal drivel written about the academies. His book should interest military historians, scholars of American higher education, and students of organizational change. Solidly based on archival research and interviews and enlightened by the author's sympathetic (but critical) understanding of the American military establishment, *Neither Athens nor Sparta?* is an intellectual achievement of the first order.

Lovell's thesis is that all the military academies share a common problem in self-definition: throughout the twentieth century they have defined their role as providing junior officers with (1) an excellent undergraduate technical and liberal education; (2) personal or "character" training uniquely suited for military leadership roles, especially in war; and (3) the rudimentary skills of lieutenants and ensigns who can go quickly to tactical units. The only problem—discerned by most students of the academies—is that modern military technology has not yet produced either a forty-eight hour day or the organizational wisdom to balance the academies' contradictory goals without conflict.

After reviewing the development of the academies until World War II, Lovell concentrates on the special tensions and incremental reforms that have characterized the decline of the seminary-academy model for the federal military college. Students of

higher education will notice parallels with the changes in church-related liberal arts colleges and large public universities occasioned by cost, sheer growth in student body and faculty, and external cultural pressure, largely produced by urbanization and the rise of the self-indulgent, secularized middle class. Lovell focuses on two specific academy problems, the liberalization of academy curricula and pedagogy and the constant struggle to balance the time demands of the classroom and the drill field. When one mixes in the "duty, honor, country" rhetoric—put into actual operation in the Honor Code with a doggedness that would impress The Three Hundred Spartans—the predictable result has been the cheating scandals and the pervasive frustrations and disappointments that mark the academies' recent history. Although a series of academy superintendents have shown great moral courage and ingenuity in producing reforms that appeased both their internal and external clients, Lovell is particularly good in explaining why the reforms are so limited. "Heroic" leaders may guide an academy through a particular crisis and direct an enlightened immediate "clean-up," but the dichotomy between the Athenian and Spartan models is too great to accommodate deep reform.

In his conclusion, Lovell examines the alternative models for reforming the academies that have been suggested by other students of American officer education. Although Lovell believes that policies followed by the Air Force Academy (the introduction of academic majors) and the Naval Academy (the civilianization of faculty) will provide models of change for the other institutions, he believes it more likely that the academies will cling tenaciously to their military character. The academy military administrators and faculty may hold ever-larger numbers of civilian degrees (and perhaps civilian ideas), but at least some of them know which side eventually won the Peloponnesian Wars.

ALLAN R. MILLETT
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SARA EVANS. *Personal Politics: The Roots of Women's Liberation in the Civil Rights Movement and the New Left*. New York: Alfred A. Knopf. 1979. Pp. xii, 274. \$10.00.

In a book with great conceptual and methodological strengths, Sara Evans has given us an effective and fascinating analysis of the origins of a social movement. She traces the development of the women's liberation movement, the "second wave" of American feminism, from the experiences of women in earlier social movements. The book covers, in historical order, southern black, southern white, and then northern white women in the civil

rights movement; the impact of black power on all of them; and women in the student, community organizing, and antiwar movements of the 1960s. The questions she asks come in part from the analogy to the development of the first wave of feminism out of the abolition and other radical and perfectionist movements of the pre-Civil War period. These informed questions led Evans to a more complex thesis about the relation between feminism and the New Left than has heretofore been common: women were oppressed and subordinated, treated disrespectfully and used exploitatively in the left, and got angry as a result; but from the left women also gained organizational skills, self-confidence, political passion, and a language in which to think about and express their own grievances.

Methodologically, Evans's book is an example of good use of oral history. Most recent "oral history" has been exclusively biographical, and too much of it only barely transcends the level of anecdote, neglecting the analysis, synthesis, and critical evaluation of evidence that transform stories into history. Evans's interviews remain evidence checked against documents and other interviews and ultimately put together to make a coherent and convincing explanation of the collective experience of her subjects. Some of her most powerful findings emerge from discrepancies between the subjects' memories and the documents: a telling illustration of the status of women in the SDS, for example, is that one male leader could not recall the name of a single woman attending a key conference, although the minutes and papers presented revealed several women he knew well had played important roles. The integration of "personal" experience that would not be available in written documents with political events provides unusual insight into the motivations that propel a social movement like feminism. Thorough research and careful interpretation allow Evans to synthesize events not far enough in the past to allow the leisurely hindsight upon which most historical writing depends.

The weaknesses of the book are primarily in two areas. First, it does not connect the particular experiences of the relatively small, though influential, group of women who are its subjects with the socioeconomic changes that affected the general population of women in the U.S. in the relevant period. The first and last chapters, which attempt an overview of factors such as women's employment, family, and demographic change are thin, lacking in complexity, and neglect much new scholarship. The failure to use social class as a category of analysis, both in the background chapters and in the evaluation of the factors that propelled women toward feminism, is a damaging omission.

A second area of weakness results from the author's failure to treat the *ideas* of women's liberation,

specifically the intellectual and political relation to feminism to the New Left. Thus, for example, she presents the tendency of the women's movement toward decentralization, variety, and short-lived groupings as an organizational failure (pp. 222-23). Closer attention to feminist thought, or New Left political thought, would have revealed that this tendency also represented a critique, however inarticulate, of the authoritarianism and hierarchicalism of preceding political organizational forms.

Such weaknesses should not deter readers from the opportunity to learn, both methodologically and substantively, from this remarkable history of a contemporary social movement.

LINDA GORDON
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Boston

YSABEL RENNIE. *The Search for Criminal Man: A Conceptual History of the Dangerous Offender*. (The Dangerous Offender Project.) Lexington, Mass.: Lexington Books. 1978. Pp. xix, 345.

The Search for Criminal Man, a volume in the Dangerous Offender Project, grew out of Ysabel Rennie's commission to produce an annotated bibliography of the principal works dealing with "dangerousness and criminality." Her study became a full-scale summary of ideas about the dangerous offender and the criminal man in Western society from ancient times to the present, with major emphasis on the nineteenth and twentieth centuries. Rennie does an effective job of tracing and classifying theories of criminal behavior over the centuries and from one writer or school of thought to another. Not surprisingly, the book lacks depth of treatment; considering its breadth, the amount of information packed into 275 pages is impressive.

The author notes that what is "criminal" tends to depend on what society considers to be dangerous. According to contemporaries, witches were dangerous criminals during one period, heretics during another, and members of workingmen's societies during yet another. As definitions changed, those activities considered to be dangerous became "criminal." Crime therefore is a natural aspect of society, and Rennie theorizes that justice (rather than an end to crime) ought to be a major social objective. Her historical examination includes those methods by which upper classes traditionally exempted themselves from punishment as well as documentation of the ineffectiveness of severe punishment as a deterrent to crime. Differentiating between American criminologists, who emphasize environmental factors, and earlier European positivists, who found the causes of criminality within the offenders, Rennie concludes that the factors producing criminals are unknown and probably unknowable.

A major shortcoming of the book is the absence of any discussion of organized crime or the professional criminal. The criminal entrepreneur whose criminal activities are his vocation is missing from Rennie's scheme of things. Although criminologists in recent years have taken organized crime into account, Rennie has not attempted to explain the activities of people like Meyer Lansky and Al Capone. This omission is ironic because Rennie quotes Capone in her introduction but does not again mention him or the professional criminal.

As Rennie herself points out, hers is a study of the "dangerous offender," one who poses a danger to society. Society generally views as dangerous the lawbreaker who engages in street crimes, not the syndicate entrepreneur who makes book or deals in stolen securities as an illegal service to a portion of society. Unless society defines as "dangerous" those who perform wanted but illegal services, the problems created and sustained by professional criminals will persist.

Although not innovative, the book is a piece of solid scholarship, potentially of considerable value to criminologists, sociologists, political scientists, and historians.

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DAVID HALBERSTAM. *The Powers That Be*. New York: Alfred A. Knopf. 1979. Pp. 771. \$15.00.

David Halberstam's latest book, he tells us, is about "the rise of modern media and their effect on the way we perceive events" (p. 739). He focuses on CBS, *Time*, the *Washington Post*, and the *Los Angeles Times*, plus numerous references to the *New York Times*. The book is based on what the dust jacket claims as "nearly a thousand interviews." I counted 501 names in the acknowledgments; Halberstam says this is "about 95 percent" (p. 740) of those with whom he spoke. He says he read extensively in secondary literature, particularly Erik Barnouw's *History of Broadcasting in the United States* (1966-71).

When *The Best and the Brightest* (1972) appeared, it was criticized for being overlong, pointless, and gossipy. Halberstam's new book has the same defects; the text could be cut by two-thirds with no loss. Factual details are sometimes repeated four times in ten pages. The absence of a developed thesis, lack of proper documentation, and numerous errors of fact for events before the 1960s suggest that historians will have to use this book with caution. There is much here that might be true and, if true, valuable, but there is also no certainty that sloppy research does not undermine the very parts that seem most interesting.

It is instructive to compare Halberstam's treat-

ment of Vietnam in *The Best and the Brightest* with what he says here, for it reveals a major problem of interpretation. In 1972 he scarcely mentioned television; *The Powers That Be* hardly mentions any political figures save for presidential campaigners. The Vietnam war cannot be comprehended solely from potted biographical sketches of political and military leaders; the media is more than the hirings and firings of reporters or the illness of "Phil" Graham. In 1972 Halberstam claimed that Clark Clifford persuaded Johnson not to seek re-election in 1968; now he talks about Walter Cronkite. Others will have to figure out who influenced whom, though Herbert Schandler's *The Unmaking of a President* (1977) is a fine beginning.

Halberstam eventually tells us that the media, now great bureaucracies, have lost a measure of integrity; he also quotes Lyndon Johnson, who believed that television had "broken all the machines and the ties between us in Congress and the city machines" (p. 6). Halberstam feels that television has become too powerful and that television presidents have too much power, but there is little evidence. Endless tales of reporters getting scoops or editors getting old are not the way to demonstrate how the media have changed the shape of American society.

This book also contains important information. What Halberstam says about television's coverage of the war in Vietnam (pp. 507-14) and about Johnson and television (pp. 6, 139, 428-38) is highly suggestive. And future students of television should find the intuitive assessment of William Paley useful. Halberstam dislikes Paley intensely and has gone to great lengths to dig up dirt. What he says about Edward R. Murrow is conventional and incorrect, but I feel there is truth in the portrayal of Paley. Readers may want to compare Paley's own version in his *As It Happened: A Memoir* (1979).

DAVID CULBERT
Louisiana State University

CANADA

SERGE GAGNON. *Le Québec et ses historiens de 1840 à 1920: La Nouvelle-France de Garneau à Groulx*. (Les Cahiers d'histoire de l'Université Laval, number 23.) Québec: Presses de l'Université Laval. 1978. Pp. 474. \$17.50.

The author's project was not an easy one to realize. Serge Gagnon planned "dégager les rapports d'influence entre la connaissance historique et la société canadienne-française au cours des années 1845 à 1920" (p. 1). He hoped to take the approach of a social scientist looking at written history as a privileged documentary source on the characteristics of a given society at one moment of its evolution and as

a powerful instrument of regimentation in the hands of the governing classes. His attempt was a daring adventure in the intellectual and social history of French Canada for almost three generations.

The plan followed by the author did not help him. The introduction is both an essay on epistemology as applied to historical knowledge and an endeavor to explain how the Roman Catholic Church succeeded in establishing its powerful influence over French Canadian society after the 1840s. The conclusion itself partially repeats the introduction. These two parts are nevertheless incomplete, because the problems dealt with are so numerous and so complex. Only readers already familiar with French Canada's history will understand the author's interpretation and viewpoints.

There are four chapters devoted to different groups of historical works: hagiographies; hero-worshipping biographies; specialized syntheses; and general syntheses. If Faillon's works rightly belong to that of a hagiographer, Auguste Gosselin's *Vie de Mgr de Laval* should be considered a biography. Many readers will be surprised to discover that Benjamin Sulte's *Histoire des Canadiens français*, this collection of forays into French Canada's past, is promoted as a general synthesis in company of Garneau's *Histoire du Canada*, a landmark both in French Canadian and North American historiography. Sulte does not deserve so much honor, but Gagnon has chosen him as a martyr for the cause of liberal and lay thinking in the Church-dominated French Canada of the late nineteenth century. The author limits his analysis of Groulx's approach to New France to his *Naissance d'une race*, an incomplete and unjust treatment of this prolific and many-sided historian.

The main weakness of the book comes from the author's attempt to give his readers all of the fruits, both the ripe and the unripe ones, of his long and original research begun as a doctoral dissertation. Scholars interested in the historiography of French Canada, however, will gather valuable bits of information in Gagnon's book, and many of his commentaries will provoke fruitful reflections and reactions. He has opened new vistas in French Canadian intellectual and social history. His contribution would have been greater if he had chosen a more functional plan and streamlined his global approach to history.

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THOMAS FLANAGAN. *Louis 'David' Riel: "Prophet of the New World."* Toronto: University of Toronto Press. 1979. Pp. ix, 216. \$15.00.

Most of the extensive literature, scholarly, popular, and fictional, dealing with Louis Riel and his two

abortive rebellions, 1869-70 and 1885, concentrates on the political character of the man and his movement. Controversy turns on two topics: the onus of responsibility for the rebellions and the question of Riel's mental condition. Contemporaries were divided on both questions. Most French Canadians held the Macdonald government responsible for the western troubles and contended that their "brother Riel" deserved clemency on grounds of insanity. English Canadians, for the most part, condemned him as a murderous revolutionary who stood in the way of Canada's natural claims to the prairie west. They demanded, and got, his execution. Though historians have remained divided, the weight of opinion, especially popular and liberal-left opinion, has come to view Riel as a martyr even while judging him insane. Yet Riel himself fought the charge of insanity harder than he fought anything. What for others was insanity was for Riel a sense of religious mission, a mission to establish a new religious state in the West, ruled by a new pope, the ultramontane Bishop Bourget of Montreal.

Thomas Flanagan, a political scientist, has chosen to take Riel at his word. He virtually ignores the questions that have long vexed historians and instead examines the *métis* leader as a religious thinker and actor. This is a fruitful and mainly convincing approach. The book is a thorough and balanced examination of the tortuous, confused, chiliastic writings and rhetoric of the "Prophet of the New World."

Flanagan has done an exceptionally able job of piecing together what often seem the incoherent ravings of a mind near the end of its tether. He demonstrates considerable knowledge of Biblical history and millenarian thinking and makes Riel's ideas less outlandish than they have often appeared. Still, many readers will find in this book the final proof of Riel's disturbed mental state. Occasionally one grows impatient, as Flanagan labors to make sense and pattern out of the jumble of visions, nightmares, and sometimes self-serving imaginings of his subject. But, in the end, he does make the clerico-conservative and theocratic cast of Riel's heretical mind more comprehensible than any previous writer.

But the book is not without its weaknesses. First, Flanagan's understanding of Quebec ultramontan-ism is superficial and inadequately researched. Second, he fails to pursue the evidence he has turned up concerning Riel's sexual troubles. A little psychohistory might have supplemented the theology here.

But the most serious criticism must be laid against Flanagan's laudable effort to relate the *métis* rebellion to other millenarian movements. The problem is twofold. First, Flanagan, though he makes a convincing case for Riel as a religious

thinker, fails to show any extensive influence of Riel's religious ideas on his followers. Their interests were much more mundane. Second, Flanagan looks in the wrong place for parallels—to Norman Cohn's medieval millenarians and contemporary Rastafarian-like phenomena. He mentions the Sioux Ghost Dance only in passing, though in time and place it was very close to Riel, and he completely ignores the Handsome Lake Religion, so brilliantly examined by A. F. C. Wallace. Indeed, a consideration of Riel within the framework of revitalistic movements as set out by Wallace years ago might have transformed Flanagan's useful corrective to the standard accounts of Riel into a work of truly imaginative scholarship.

RAMSAY COOK
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R. D. CUFF and J. L. GRANATSTEIN. *American Dollars—Canadian Prosperity: Canadian-American Economic Relations, 1945-1950*. Toronto: Samuel-Stevens. 1978. Pp. 286. \$14.95.

Historians should not infer from the title of this slender but significant monograph that its utility is confined to specialists in Canadian-American relations or to students of Canadian history, particularly those concerned about the responsibility of Liberal governments for United States domination of Canada's economy. Scholars interested generally in the diplomacy and economics of the period will find it both informative and provocative. The chapter entitled "Canada and Containment" not only shows how Canadian officials skillfully exploited anti-Soviet anxieties then prevalent in Washington to direct the United States into multilateral association whereby they might influence American policies but also dissects the debate in Ottawa over the motivations and objectives of Moscow's behavior, a debate that featured the variant analyses of Escott Reid, Maurice Pope, and Dana Wilgress. Similarly, R. D. Cuff and J. L. Granatstein demonstrate how these same officials used the Korean War to further the desired ends of American commitment to a Europe-First strategy and a program of rearmament with weapons procurement in Canada.

Primarily, however, the book details the interaction between officials in Washington and Ottawa as they sought to solve the problems resulting from the economic decline of Great Britain and Western Europe and the emergence of the United States as the world's most powerful economic nation. Critical to Canada were the immediate postwar shortage and continued loss of American dollars and, because of the increasing north-south commercial relationship, an unfavorable trade balance that chronically threatened its dollar reserves. Drawing

largely on previously unexplored government documents, private collections, and interviews with surviving principals, the distinguished authors effectively portray the lobbying skill of Canadian diplomats and a sensitive but politically cognizant American executive as they develop such topics as the imposition of selective restrictions on imports from the United States, off-shore purchasing under the Marshall Plan, the abortive Canadian-American customs union discussions, the southward movement and stockpiling of strategic minerals, Washington's agreement to procure arms in the dominion, and Ottawa's resistance to American pressure for a greater financial contribution to European recovery once the Canadian economy turned upward. Their presentation of Canadian negotiations with the growing imperial power of the United States is traditional: they paraphrase or deftly weave into the text quotations from memoranda of conversations, dispatches, and position papers, an approach that affords readers a glimpse of bureaucratic policy making.

The authors or editors occasionally slipped. Senator Milton Young is introduced as a Nebraska Republican. Several grammatical errors escaped detection. A line was omitted on page five. Robert Taft is termed an isolationist when "unilateralist" might have been a more accurate appellation. Perhaps they quote too frequently and at too great length from the documents. Perhaps, also, they could have developed more meaningfully the nationalistic overtones in Congress and the divisions between the Departments of State and Agriculture as they affected Canada. These minor criticisms aside, the authors have produced another cogent study, one that will stand until additional documents become accessible and provide a different perspective from which to view these problems and the penetrating questions Cuff and Granatstein raise.

RICHARD N. KOTTMAN
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LATIN AMERICA

ANDRÉ GUNDER FRANK. *Mexican Agriculture, 1521-1630: Transformation of the Mode of Production*. (Studies in Modern Capitalism.) New York: Cambridge University Press. 1979. Pp. xiv, 91. \$13.95.

André Gunder Frank has written an exercise in right thinking. He would correct erroneous interpretations of the nature of the Mexican economy and society during the sixteenth and seventeenth centuries by François Chevalier, Eric Wolf, myself, and others. He agrees that there was a shift from a pre-Conquest to a colonial, Hispanized economy

but objects to the concept of a dual economy and substitutes that of a single economy tied to the world market. He agrees that the Spaniards appropriated much land for haciendas and that these haciendas ultimately derived labor through debt peonage. He objects strongly to characterization of the haciendas as feudal, in Marxist usage, holding that they were capitalist. He further objects to the concept of depression in the later sixteenth and most of the seventeenth centuries and counters that these were years of prosperity in which the large estates developed in response to brisk demand and rising prices.

The entire essay is seriously flawed in two ways. In the first place, much of the controversy Frank wants to excite or settle rests upon definitions. What, for example, is meant by dual economy or unified economy? The Indians obviously were organized differently from the Spaniards but contributed production and labor to the Spanish sector. Neither Chevalier nor I use the term feudal in the Marxist sense. Use of the term by others means that the haciendas are considered exploitive. Chevalier does see the hacendado in the North as a leader rallying his employees for defense against the Indians and dispensing a rough justice. Depression or prosperity become also very much a matter of definition. Frank does not dispute that Indian agricultural production fell drastically; I do not dispute that Spanish production rose. That the increase in the latter could compensate for a drop on the order of nine-tenths in the former would require demonstration. Chevalier's concept of a retreat into agriculture applies to the North, where mining did contract and could hardly provide an expanding market.

In the second place, a great deal of fine detailed study, based on archives and business accounts, is altering our knowledge of these questions. Frank, as he admits (p. xi), is aware of the new work but has not taken it into account. So he writes of occupation of land without reference to Lesley Simpson's fundamental study of 1952, argues the role of mining without Peter Bakewell's fine study of Zacatecas, bases his references to sugar raising on Sandoval rather than Ward Barrett, appraises the Valley of Oaxaca without knowledge of William Taylor, and ignores new studies of haciendas, Indian communities, and the question of dual economy and society.

WOODROW BORAH
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WILLIAM B. TAYLOR. *Drinking, Homicide, and Rebellion in Colonial Mexican Villages*. Stanford: Stanford University Press. 1979. Pp. 242. \$16.50.

William B. Taylor's *Landlord and Peasant in Colonial Oaxaca* (1972) and his prize-winning article, "Estate and Class in a Colonial City: Oaxaca in 1792" (1977), co-authored with anthropologist John K. Chance, have established him as an authority on colonial land patterns and society in southern Mexico. In his present work, Taylor again focuses on colonial Oaxaca but has broadened the scope of his research to include Central Mexico.

Drinking, Homicide, and Rebellion examines social behavior in Indian peasant communities during the late eighteenth century. It is based primarily on archival research in several *ramos* of the *Archivo General de la Nación* (Mexico), state and municipal records in the Valley of Oaxaca, Inquisition trials, civil litigation, and documents relating to Indian affairs. Printed sources in the bibliography also reveal the author's familiarity with a wide range of literature dealing with the history, sociology, and anthropology of peasant communities throughout the world.

The first of four chapters provides the colonial setting and geographical limitations of the study. A total of twenty-nine *alcaldías mayores* (administrative units) and seventy-two towns in the intendancy of Mexico and the region of Antequera (Oaxaca) comprise the loci of Taylor's study. These are probably the best examples of Mexican colonial experience, for they include the heartland surrounding Mexico City as well as the important geographical and cultural subregions that lie within the Valley of Oaxaca. Separate chapters are devoted to the incidence of drinking, homicide, and rebellion within the two areas.

In his conclusions on drinking in peasant communities, Taylor questions that a "plague of alcoholism" swept the Mexican countryside after the conquest. The anguish and despair of the displaced Aztec elite, well documented in the *Florentine Codex* and *The Broken Spears*, did not necessarily produce a commensurate malaise among the lower classes who sought relief in the consumption of intoxicants. Evidence compiled by Taylor does suggest that Indians drank more and drank more often after the conquest, but he does not find conclusive evidence of unrelieved drinking as an escape from social pressures.

Homicide appears to have been prompted more by personal animosity than by group consciousness, class war, or political motivation in both areas, although intracommunity homicide was less prevalent in Oaxaca. Rebellions in rural communities of both regions were essentially at the same level, but were far less common than in the post-Independence era. The crises in national leadership that spawned the revolts of 1810 and 1910 appear to have been absent in the eighteenth century.

Taylor's painstaking research and careful conclusions make this book required reading for Latin

American scholars. It also forces us to reconsider our generalizations about colonial Indian society.

DONALD CHIPMAN
North Texas State University

OAKAH L. JONES, JR. *Los Paisanos: Spanish Settlers on the Northern frontier of New Spain*. Norman: University of Oklahoma Press. 1979. Pp. xv, 351. \$22.50.

Oakah L. Jones, Jr., approaches the well-plowed field of Spanish borderlands history with a desire to rescue the civilian settler, the common man of New Spain's northward advance, from the relative obscurity to which he has been condemned by many historians. Following in the footsteps of the late Herbert Eugene Bolton, historians have often preferred to concentrate on the deeds of explorers, missionaries, and soldiers. These luminaries appear in Jones's volume but in the unfamiliar position (for them) of background to the unheroic struggle for existence of the *paisano*.

The author divides the vast region of the northern frontier of New Spain into four parts: the northeastern frontier, consisting of Coahuila, Nuevo León, Texas, and Nuevo Santander (Tamaulipas); the north central frontier of Nueva Vizcaya (Durango and Chihuahua) and New Mexico; the northwestern frontier of Sonora and Sinaloa; and the Pacific frontier of Baja and Alta California. He differs from the Boltonians in making Sonora and Sinaloa into a separate area of northward advance, and he gives New Mexico proportionately more space than other single regions. He omits Louisiana and Florida.

The book deals with such matters as occupations and work habits, education, mail service, crime, Indian problems, disease, class differences, and other aspects of the social history of these frontier areas. There gradually emerges from the kaleidoscope of pictures the outline of the stoic figure of the *paisano* and his family occupying the land for Spain despite poverty, isolation, and lack of information.

The author is at pains to contradict the black legend still clinging to Spanish settlers as large landowners who exploited docile Indians. Indians like the Apache and Comanche of northern New Spain were anything but docile, and lack of labor forced the Spanish settlers (most of them Mexicans, as the author points out) to do their own work. Jones frequently emphasizes that these Mexican families on New Spain's frontiers were hard working but, although he cites the Jesuit father Ignaz Pfefferkorn's opposite view of the situation in eighteenth-century Sonora, he makes no comment on it. He does not mention similar criticisms that were made in Alta California, where the mission Indians were later said to be doing much of the manual labor that

kept the provincial government going. Jones also has some remarks of interest on the influence of the northern frontier on Mexican history, apparently following the ideas of Silvio Zavala.

Much of the book is based on material selected from standard primary sources on and secondary literature for the region, but the author has gone to local archives for fresh material on demography and other questions of social history such as prices. The book contains a useful bibliographical essay, six rather roughly drawn maps, and twenty-nine illustrations. The author has tackled his difficult assignment methodically and conscientiously. His work should stimulate further interest in an important subject.

C. ALAN HUTCHINSON
University of Virginia

WILLIAM L. SHERMAN. *Forced Native Labor in Sixteenth-Century Central America*. Lincoln: University of Nebraska Press. 1979. Pp. xvi, 496. \$18.50.

The publishers of this impressive volume rightly contend that the social history of the sixteenth-century conquest of Latin America during the formative decades when class structure was evolving has yet to be written. This book goes a long way to present the first truly comprehensive investigation of the relationship between forced native labor, class structure, and colonial society in that part of the Viceroyalty of New Spain that today is Central America. The volume traces the social, economic, and demographic conquest of early Central America and it details the rupturing of Indian traditions as the Spanish conquistadores and crown officials began the unscrupulous process of exploitation of the only real source of wealth in the sixteenth century, that of native labor. It was in Central America that the Spanish crown made the first serious attempt to reform the labor system in the New Laws of 1542 and to humanize Indian policy. The book details the struggle to improve conditions in face of strong opposition of the settlers.

William L. Sherman's study is an important contribution to the field of Latin American history. Forced native labor as a topic pervades all aspects of Central America in the sixteenth century—social, economic, political, and demographic. The author manipulates a mass of archival materials drawn from Spain, Central America, and Mexico very skillfully, and he writes in a lucid, graceful, and witty style. Although the thrust of the book is on slavery and various other forms of forced labor in sixteenth-century Central America, the work is primarily a social history that treats the consequences of forced labor. As such the study will be of great interest in all areas of Latin American historio-

graphy as well as to anthropologists, sociologists, and ethnohistorians. There is also a lot of early political history in this work—material that is entirely new or else fuzzy or cryptic in other accounts. There is great value in Sherman's biographical detail on the character of the early *audiencia* judges, the governors, and other officials. A chapter on Indian women is unique—a real contribution to social history. The chapter on native nobility is unusually good and contributes vastly to our knowledge.

The great strength of the study is in the extensive use of original manuscripts, most of which have never been used before. Sherman has spent many years on these sources and has hewn his account out of a mountain of archival rock. He has relied primarily on documents dealing with specific local issues rather than abstract Spanish legal codes—thus bridging the gap between theory and practice so evident in other "theoretical" studies of Spanish colonial labor institutions. In my view Sherman's account may well be the definitive study on colonial labor in Central America. What is so impressive in his treatment of the abundant documentation is his reliance on many cases or examples for each of his generalizations. This is sound scholarship based on the most rigorous canons of our craft. Sherman's interpretations are judicious and balanced. He takes neither an Indian nor a Spanish view of labor institutions and Indian policy. In his preface and introduction he lays out the methodological considerations of his study, preferring to let his documents define the boundaries of the work rather than starting with a prefigured model or conceptual framework. As such, Sherman's work falls into the "traditional" category, but it does indeed synthesize and interpret masses of data on sixteenth-century Central America.

The lengthy section of notes (eighty-seven pages), the excellent appendixes on Indian and Spanish population, the full bibliography of manuscript and printed materials, as well as a functional index, testify to the book's importance.

RICHARD E. GREENLEAF
Tulane University

ROGER NORMAN BUCKLEY. *Slaves in Red Coats: The British West India Regiments, 1795-1815*. New Haven: Yale University Press. 1979. Pp. xi, 210. \$17.50.

Desperate military circumstances in the West Indies during the wars of the French Revolution occasioned the creation of black slave regiments in the British army. The planter class in the Caribbean colonies opposed the formation of these regiments, preferring the maintenance of white troops and fearing dreadful consequences from arming black

slaves to defend a slave society. When the planters refused to cooperate in the recruitment of slave troops, partly for economic and partly for sociopolitical reasons, the imperial government proceeded with its plan and purchased raw slave recruits from Africa. In 1807, slave soldiers were manumitted, though they were retained in the ranks on lifetime service. These forces were satisfactorily organized, trained, and disciplined; they were used with effect against foreign enemies and insurgents; and they exhibited no reluctance to police and defend slave society in the British Caribbean. These points form the basis of Roger Norman Buckley's brief history of the West India regiments, 1795-1815.

Buckley is probably correct in asserting that academic historians have neglected military history and that military historians have neglected the Caribbean. His work adds new dimensions to two important historical issues. First, he amends the thesis of D. J. Murray's *The West Indies and the Development of Colonial Government 1801-1834* (1965) by asserting that the decision by the imperial government to garrison the plantations with black slaves was an important episode in the progressive reassertion of metropolitan control over the political destinies of the Caribbean colonies. Second, he argues that Pitt's government made an expedient decision to delay the abolition of the slave trade on grounds that slave shipments from West Africa were a necessary recruitment base for the West India regiments. His evidence on the second point is circumstantial. The decision to recruit black troops from slave ships was taken at the highest level, and in 1806 the government hastened to contract for a large number of slaves before the trade became illegal. But, if it was reasonable to delay abolition until 1807 on the grounds of military expediency, why was it not expedient to delay it longer? Buckley's argument is an important one, and it may shed vital light on a long-standing problem, but his point needs further elaboration and firmer evidence to show that Pitt placed highest priority on recruitment for the West India regiments and that he and his colleagues were convinced that the type of recruitment operations established after abolition, principally at Sierra Leone, would not have sufficed before 1807.

The limitations of Buckley's work derive in part from his failure to afford a comprehensive overview of the military struggle between France and Britain at the turn of the century. There is no full or clear statement of the strategic importance of the West Indies or of the military operations pursued in the Caribbean theater.

Buckley explores the social context of regimental life, disease problems, civil-military relations, and the eventual disbandment of the regiments. The book relies heavily on War Office and Colonial Office archives, and it is generously seasoned with mi-

nor points of considerable interest to imperial and Caribbean historians.

WILLIAM A. GREEN
Holy Cross College

SAKARI SARIOLA. *The Puerto Rican Dilemma*. Port Washington, N.Y.: Kennikat Press. 1979. Pp. 200. \$15.00.

This book should not be dismissed as just another rehash of the seemingly ageless *Paradox* (1947), *Plight* (1935), or *Unresolved Problem* (1945) so familiar to the student of Puerto Rico. Sakari Sariola has adorned his description of the Puerto Rican dilemma in very modern terminology and unconventional dialectical analysis. From the outset it must be stated that although the author recognizes the importance of historical perspective, he uses history "as a sociologist focusing on the system qualities of the collectivity at hand . . ." (p. 7). Yet Sariola stresses in the opening paragraph of the preface that his methodology does not follow the conventional pattern: "Instead, I have stressed reactive, willful, and ideology-bound elements" (p. 2).

Thus forewarned, the historian can pass over the erroneous dates (pp. 53, 97, 103), the mistaken names (pp. 94, 104), the extensive use of an 1847 German translation of a French work reissued in 1973, or an occasional error in fact (p. 158). And the social scientist need not worry about objectivity or "an apolitical appraisal." Yet, heretically enough, this is precisely the value of the book, perhaps not for the historian or the conventional social scientist but certainly for the astute and incisive social observer or the dialectical political analyst.

"This book focuses on the search for answers by Puerto Rico's vanguard to the question of political identity and on the compromise solutions they have proposed for implementation" (p. 14). The author brings into sharp focus the multitudinous statements of the intellectual elite of the "independentist" movement ranging from the early revolutionaries of the past century to the current most-wanted members of the clandestine *Fuerzas Armadas de la Liberación Nacional* (FALN). His focus is less sharp, and less generous, when expounding on the also revolutionary and, in the opinion of many Puerto Ricans, brilliant concept of sovereignty contained in the *Estado Libre Asociado* before it was emasculated by a jealous Congress.

The distinction between *Patria* and *Nación* is a basic framework provided by the author for the Anglo-Saxon reader who too often is mystified by the flood of Latin rhetoric. His analysis of the ideas of Pedro Albizu Campos is excellent, revealing their clarity and logic as well as their archaic and prejudiced quirks. Indeed, throughout the book the intellectuals' case for independence is most eloquently

put. For the reader of independentist persuasion one question becomes most pressing as he proceeds to the final pages: "Where is the dilemma?" The dilemma comes at the very end because in the final analysis Sariola cannot ignore the socioeconomic reality that over three million people are being sustained by an incredible outpouring of federal largesse. The author is too much of a realist to suggest that these millions are about to throw out their meal ticket. He does not, however, rule out the possibility of drastic change: "discontinuance through ideological overturning"!

THOMAS MATHEWS
University of Puerto Rico

PETER MARZAHN. *Town in the Empire: Government, Politics, and Society in Seventeenth-Century Popayán*. (Latin American Monographs, number 45.) Austin: Institute of Latin American Studies; distributed by University of Texas Press. 1978. Pp. xxiv, 218. Cloth \$14.95, paper \$5.95.

Peter Marzahl's study of seventeenth-century Popayán is a valuable, though in some ways disappointing, addition to the historical literature on colonial New Granada. On the basis of original research in the archives of Popayán, Seville, and to a lesser extent Quito and Bogotá as well as an examination of the available printed materials, he offers a fully documented account that both describes conditions as they existed during the period and endeavors to analyze and explain at least tentatively the changes that occurred. It is topically organized, with separate chapters on "Estates, Mines, and Commerce," "Cabildo Government," "The Church and the Settlers," and so forth. There are a useful glossary and appendixes; some tables, mostly of names not numbers; and maps, one of which has a disconcerting and quite indefensible mixture of German and English legends.

If from the different chapters one unifying theme emerges, it is that of steady consolidation of local leadership in the hands of a "settler" aristocracy composed of family networks and always open through marriage to new recruits. The same aristocracy receives the bulk of the author's attention, whether he is treating political, economic, or social affairs, and though there are good reasons for such a focus, it naturally does mean that the picture given of life in Popayán is far from complete. In dealing with the aristocracy and with the state and church officials with whom it interacted, moreover, Marzahl devotes perhaps too much space to rehashing the petty squabbles that are conveniently mirrored in *residencia* records and the like. Roughly half of his chapter on the church relates squabbles between governors and bishops—after which he says that

these constituted "the exception, not the rule" (p. 150). If that was the case and if, as Marzahl further suggests, the church was in many ways a stronger institution than the state, such treatment of its role seems less than adequate.

A final problem concerns the relative emphasis on "town" and "empire." In the introduction, Marzahl indicates that he proposes to investigate Popayán both in its "own terms" and "in relation to the empire as a whole" (p. xviii). He successfully brings out a number of special features, including the impact of a shifting mine frontier and the way in which Popayán was subject to the competing powers of attraction and overlapping jurisdictions of Quito and Bogotá. But the insights that he gravely offers concerning the nature of the larger imperial system of which Popayán formed part—or the administration of "the empire as a whole"—seem to add little that is new. For most readers, therefore, a bit more "town" and less "empire" would have been desirable.

DAVID BUSHNELL
University of Florida

JOSÉ HONÓRIO RODRIGUES. *História da história do Brasil*. Volume 1, *Historiografia colonial*. (Brasiliana, number 21.) São Paulo: Companhia Editora Nacional. 1979. Pp. xxii, 534.

This is a book of bio-bibliographical comments and essays (organized under appropriate subject headings) of surpassing interest for the study of Brazilian colonial history. It is not a calendar of "documents" as such but an impressive series of descriptive analyses of the lives and writings of priests and laymen who themselves survived the colonial experience and put pen to paper in their own fashion.

José Honório Rodrigues promises two more volumes to round out his comprehensive survey of Brazilian historiography, one on the national period and one on historiography and ideology. The latest foray into bibliography by a master of the genre will not be the last of his scholarly undertakings, but if it were, nothing would more fittingly end a distinguished career. Bibliography and instruments of scholarship, rather than monographs built upon patient archival research, have been his forte right along, a field of endeavor where his talents, despite outbursts of peevishness, have best been displayed.

If anyone has ever doubted the abundance or fascination of the historical literature of Brazil's Portuguese past, the present array of sources will force him to think again. For the book does indeed do very well what it set out to do. Every student of Brazil and of the overseas expansion of Europe since 1500 will be grateful to the author for his fine over-

view of the history of the history, if not of Portugal's eldest daughter, certainly of its traditional milk cow.

This is not to say that I was pleased with every aspect of the grand compilation. I admired the author's ability to place each item in historical perspective. I liked the tribute that he pays throughout his pages to the memory of his late lamented mentor, Capistrano de Abreu, the most perceptive of the modern historians of colonial Brazil. I liked much less the tendency to make partisan judgments that serve merely as personal statements. His denunciations of the Portuguese are aggressive and abrasive, more to be expected from a self-righteous moralist than from a historian. He is equally at odds with the missionary enterprise, with which he is hardly in sympathy, and upbraids an ancient monarchy that managed to endure precisely because it elicited enormous loyalties from its Brazilian vassals.

There are appalling typographical errors and, what is more serious, altogether too many slips that careful editing would have caught. Rodrigues knows that we cannot transpose the patriarchate of the eighteenth century from Lisbon to Salvador, or identify Oporto as the archdiocese that it never was, or create the diocese of Olinda-Recife before its time. He also knows the risk of using words that lexicographers have thought best to ignore. Above all, his excessive dipping of the pen in bile makes us question his perceptions of the colonial period and wonder whether or not the realities of Baroque structures may best be perceived through populist and "progressive" spectacles.

If one overlooks the exclamations and touchiness, what remains is a superb catalogue of historiographical sources, a much needed bibliographical survey of the colonial period, of history as told by those who lived it, the product, in short, of a man of great reputation who examined everything and now shares his insights with us.

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JOSÉ HONÓRIO RODRIGUES. *O Conselho de Estado: O quinto poder?* Brasília: Centro Gráfico do Senado Federal. 1978. Pp. xv, 417.

The 1824 Constitution of the Empire of Brazil specifically provided for four "powers," or branches, of government: legislative, executive, judicial, and moderating. The moderating power, assigned to the crown, gave the emperor a large measure of control over the other three branches, enabling him to veto legislation, dissolve the Chamber of Deputies, appoint senators, appoint and dismiss cabinet minis-

ters, commute judicial sentences, and pardon convicts. To assist the monarch in wielding the moderating power, the constitution provided for a Council of State, composed of life-term appointees named by the emperor. José Honório Rodrigues, in *Conselho de Estado: O quinto poder?*, contends that the Council of State was, in fact, "the fifth power" of government in imperial Brazil.

According to Rodrigues, there were three councils of state during the imperial period. He argues, unconvincingly, that the first was the *Conselho de Procuradores* convoked by Dom Pedro I at the beginning of his rule over *de facto* independent Brazil. This body, however, was actually a board of provincial representatives and bore little resemblance to the later councils of state. The *Conselho de Procuradores* was suppressed shortly after the convening of a broader representative body, the Constituent Assembly of 1823. The first Brazilian Council of State, denominated as such, was created with the emperor's dissolution of the Constituent Assembly in 1823; this was a ten-man committee that, on Dom Pedro's orders, drafted the document that in 1824 was proclaimed the Constitution of the Empire of Brazil. The Council of State was retained under the constitutional reign of Dom Pedro I as an advisory body that the emperor and his ministers were required to consult on important matters. During the Regency, in 1834, the Council of State was suppressed, but, in 1842, early in the reign of Dom Pedro II, it was reconstituted; it expired with the monarchy in 1889.

Rodrigues's book concentrates on the debates, especially in the imperial Senate, on proposals to create, abolish, or modify the Council of State. Paraphrased parliamentary speeches and long quotations are interspersed with the author's lucid commentary. There is, however, too much repetition of both ideas and rhetoric (for example, one five-line quotation appears in the text twice, pages 172, 176). Although the concept and the legal structure of the Council of State are more than adequately covered, Rodrigues provides little information on what the council actually did, on what went on at its meetings. Only three Council of State sessions are specifically mentioned, one in 1824 and two in 1889, and the author contradicts himself on the consensus of the first (pp. 115, 130). Among the volume's valuable features are biographical sketches of all the imperial Councillors of State. The usefulness of the seventy-two biographies of councillors of the Second Reign, however, is limited by Rodrigues's omission, except in the case of Sales Torres Homem, of any reference to party affiliation.

The composition of the Council of State indicates its power. With a few notable exceptions—including Zacarias de Góis, the baron of Cotegipe, and José Antônio Saraiva—the great statesmen of the

Second Reign were Councillors of State. They, according to Rodrigues, exercised the "fifth power" and were the "guardians of the fourth power," or moderating power. "The fourth and fifth powers," Rodrigues writes, "were ingenious creations of the dominant Brazilian minority," devised "to subjugate the masses," which the elites "feared—and even detested." However admirable the intellectual attainments of its members, the Council of State, Rodrigues insists, "helped enslave the Brazilian people" (p. 281). The matter warrants further investigation.

NEILL MACAULAY
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VERA BLINN REBER. *British Mercantile Houses in Buenos Aires, 1810-1880*. (Harvard Studies in Business History, number 29.) Cambridge: Harvard University Press. 1979. Pp. xi, 206. \$17.50.

Vera Blinn Reber's *British Mercantile Houses in Buenos Aires, 1810-1880* is published as the most recent of the series Harvard Studies in Business History. The focus of the book is business history; Reber is best in her presentation of the operating conditions and practices of important firms in Buenos Aires. Her work draws on extensive research in British and Argentine archives, and Reber has often chosen to let the facts speak for themselves. As a result, she does not discuss at length the works of other authors writing in this field, general theories of business and economic history, or economic development as applied to Argentina.

Background for Reber's treatment of British mercantile houses is provided in the first three chapters of the book: "A Perspective," "Risk and Opportunity in Argentine Trade," and "Politics and Society in Buenos Aires." Chapters four through seven, "The Organization and Operation of the British Mercantile House," "Operating in Argentina," "Merchants, British Investment, and the Economic Development of Argentina," and "British Merchants as Entrepreneurs," provide many helpful examples of the significance of the operation of merchant houses. Initial British success led to many imitators: the British share of import-export houses in Buenos Aires fell from 33 percent in 1825 to 18 percent in 1866 (p. 56). Reber indicates that current problems faced by foreign businessmen had nineteenth-century origins, observing that "the problems created by the Argentine political system could be decreased by forming partnerships with influential nationals" (p. 64). But she also shows that the traditional unlimited liability of partnerships involved considerable risks, since, in the case of failure, "claims were made upon the entire personal estates of all the partners," and bankruptcy of one

partner therefore could cause the remaining partners' other ventures to fail (p. 65).

Occasionally, Reber lets a dramatic opportunity slip by. The dictator Juan Manuel de Rosas insisted that everyone wear his own red colors and the blue and white colors of the revolutionary years were banned. Wearing them thus brought far too great personal risk to make purchase of blue cloth practical. Reber does not mention this ban but merely indicates that "successful marketing of goods depended on . . . the merchants' ability to compete in terms of . . . good will." And she notes that a merchant complained that a "recent consignment of textiles had ignored all advice, as it comprised mainly green and blue colors unsuited to the market for political reasons so that shopkeepers would not touch them. He added that competitors had recently received preferred patterns in red and pink colors and felt that Fielden's shipment would be unsalable unless the prices were reduced" (p. 89).

Reber concludes that "foreigners succeeded when they learned how to operate in the political and social spheres" (p. 144) and that, "although merchants acted in nearly every case from self-interest, the over-all effect on Argentina was positive" (p. 145).

This book is recommended to scholars of business history, as a supplement to H. S. Ferns's basic *Britain and Argentina in the Nineteenth Century* and to L. Randall's *A Comparative Economic History of Latin America, 1500-1914*, vol. 2, *Argentina*.

LAURA RANDALL
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CARL E. SOLBERG. *Oil and Nationalism in Argentina: A History*. Stanford: Stanford University Press. 1979. Pp. xiv, 245. \$15.00.

Carl E. Solberg presents in his second book an effective, traditional "case study" of Argentina's oddly named *Yacimientos Petrolíferos Fiscales* ("State Petroleum Deposits"), better known as YPF. The importance of YPF lies in the fact that it has been a powerful force in Argentine economic, political, and ideological history from 1922 to today and that with its establishment Argentina was the first state, excepting the Soviet Union, to found a vertically integrated, state-owned, petroleum industry.

The 183 pages of text focus predominantly on the years from the first major oil discovery on state land in Argentina, in 1907, to the early 1930s, when the nation underwent a military-political seizure of power and entered a new and continuing era of national destabilization.

The monograph is organized chronologically. Chapter one is interesting because it shows "The Struggle for State-Owned Oil, 1907-1914," which

was not much of a struggle, in fact, because of the minimal interest of the national government in its petroleum lands—a lack of concern that resulted from the deeply rooted, liberal-international economic ideology of the Argentine elite. But the author also examines the opposing views of a few early economic nationalists, who sought to defend and extend the national petroleum lands and production against private investors, domestic and foreign. Of the latter, one of the most important, and the most controversial for many decades, was Jersey Standard (Exxon).

Chapter two analyzes the energy crisis caused by World War I, which coincided with the rise to national political power of President H. Yrigoyen and his middle-class Radical Party. The author skillfully traces the failure of that more liberal leadership to grapple with the petroleum crisis and the emergence of a few military leaders who began to state Argentina's energy problems in terms of economic dependency on foreign powers.

The vigorous Colonel (later General) E. Mosconi emerged during the war and properly plays a large part in the remainder of this narrative. Mosconi became head of YPF in 1922 and gave it the administrative leadership and ideological importance that (at least the latter) still shape its role in Argentina. The labyrinth of petroleum politics is marvelous to observe (when one is permitted) in any context. Non-Latin Americanists will also benefit from Solberg's dissection of the Argentine case, with its objective analysis of the links between multinational corporations and Argentine state and national governments. The establishment of these links had the important side effect of injecting new life into the old federalist-centralist controversy. Solberg also describes astutely such important related issues as the living conditions and strikes of the oil workers and the characteristically polarized positions of Argentine military leaders.

The final chapter is a sketchy review of Argentina's petroleum problems—and some achievements—from the 1930s to 1977. Its limited substantive scope raises the question of why the author chose to restrict his account essentially to the period prior to 1930. Much oil has gone through the pipelines since then, and the most dramatic political confrontations have occurred. Yet those who have worked on Argentine history, in Argentina, know the frustrations of trying to obtain documents. (Exxon also frustrated Solberg.) Perhaps Solberg or someone else can push the work forward some day.

Oil and Nationalism is a fine study in petroleum, politics, and dependency versus nationalism. And, like all Stanford University Press books, it is handsomely presented.

THOMAS F. MCGANN
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GABRIEL GUARDA. *Historia urbana del Reino de Chile*. Santiago: Editorial Andrés Bello. 1978. Pp. 509.

Gabriel Guarda has long been known for his contributions to Chilean urban history. This study, which draws on his earlier work, other secondary material, and research done principally in Spanish and Chilean archives, provides an overview of urban developments from 1535 to 1826 within the confines of the old captaincy-general of Chile. The initial chapters move, after a very brief discussion of the Indian and European backgrounds, from the early Spanish settlements, through the destructive effects of the Indian wars and the subsequent primacy enjoyed by Santiago during the seventeenth century, to the revival and dispersion of urban centers under the Bourbons. The remainder is arranged topically, concentrates on the eighteenth century, and delves into urban Chile's economy, inhabitants, culture, and lifestyles.

Guarda is at his very best when he discusses planning, the physical layout of cities, and urban architecture. He is particularly strong in his descriptions of religious buildings and in analyzing the effects of a defensive posture on the design and character of urban centers. Some readers may be troubled in spots by rather broad statements and inadequate treatment. His assertion, for instance, that Chileans held most of the major colonial public posts (p. 184) glosses over a very complex subject. His discussions of groups such as urban blacks and Indians are not well developed. He might have avoided these and similar difficulties by exploiting notarial records further. The work is, however, an impressive synthesis. Through it, Guarda intended not only to focus on a secondary area in the Spanish Empire and thus to rectify an alleged bias among urban scholars toward New Spain and Peru but also to demonstrate that Chilean towns and cities were vibrant and played an important role in the region's history. He succeeds convincingly in that.

The book is beautifully illustrated. Guarda has pulled together an imposing array of town and city plans, urban maps, sketches and photographs, drawn mainly from depositories in South America and Western Europe, and he includes them along with the text and in an extensive appendix. It is one of the best visual collections on any group of Latin American cities available in print. Surprisingly, there is only one map of Chile with its cities (for 1575) to orient the reader. A special index to settlements, with citations, and a very detailed bibliography of published and unpublished materials, organized by topics, round out the volume. They serve, as does the text, to make this a significant study and an important resource for anyone interested in Spanish American urban history.

KEITH A. DAVIES
Vanderbilt University

BARBARA STALLINGS. *Class Conflict and Economic Development in Chile, 1958-1973*. Stanford: Stanford University Press. 1978. Pp. xviii, 295. \$18.50.

This is an important and interesting contribution to the growing shelf of books seeking to answer the question, "What Happened in Chile?" Barbara Stallings is both a political scientist and economist and approaches the question from a broadly Marxist perspective.

The book starts with a theoretical discussion of "the political economy of development" in which the author underscores the major importance of three elements: the role of different social classes, the part played by the state, and the nature and participation of "the foreign sector." This is followed by a short discussion of the historical background, in both economic and political terms, of the period with which the author is principally concerned.

Most of the book is taken up with the development performance of the three regimes that spanned the 1958-73 period. These were, "of course, the rightist alliance directed by the National Party, the centrist alliance directed by the Christian Democratic Party, and the leftist alliance directed by the Communist and Socialist parties" (p. 53). Each of these regimes represented a coalition of different class elements and therefore followed different policies. The rightist alliance under President Jorge Alessandri (1958-64) was a coalition of upper- and middle-class elements. The centrist regime (to use the author's term) of President Eduardo Frei (1964-70) tended more than the other two to cut across the various classes, since "the Christian Democrats were the only real multiclass party in Chile" (p. 55). Finally, the leftist regime of President Salvador Allende (1970-73) was based fundamentally on the working class, particularly the blue-collar workers, although it sought backing from the lower middle class.

The development strategies differed because of the class differences among the backers and leaders of the three regimes. Alessandri's major emphasis was on investment rather than redistribution, on the role of private enterprise rather than the state, and on providing the widest possible freedom for foreign investment. In sharp contrast, the Allende government put major emphasis on redistribution rather than investment, sought to establish the overwhelming predominance of the state in the economy, and largely squeezed foreign investment out of the picture. The Frei government's policies incorporated elements of both the Alessandri and Allende approaches. It put more emphasis on redistribution than did Alessandri's regime, although much less than did Allende's, while putting very strong emphasis, too, on investment. It fostered a

much greater participation of the state in both mining and industry but continued to lay emphasis on the major role to be played by private entrepreneurs. Finally, although limiting severely the participation of foreign investors in their traditional mining sector, it sought new foreign investment in manufacturing and brought it in as a partner of the state.

The author goes into all of these matters in some detail. She has a chapter each, for instance, on the similarities and differences in both the approaches and results of the three different development strategies. Some readers might be surprised more by the discussion of the similarities than by that of the differences. The author's conclusion is that all three strategies failed. She poses the question of what alternative is left, and answers that, "by process of elimination, the Chilean case points to a socialist model but of a different type than that practiced by the UP: a socialist model with greater emphasis on investment—and therefore on planning and project implementation—and less on the rapid increase of money income" (p. 237). My biases being different from those of the author, I am dubious about this prescription. Still, this book will lead anyone concerned with Chile to a much better understanding of what happened and led to the catastrophe of September 1973.

The book has certain weaknesses. One the author recognizes: because it concentrates on the urban areas, particularly greater Santiago, it misses much of what went on, particularly in the Frei and Allende periods. Interpretations of events might have been different had the rural areas been included in the discussion. One can quarrel also with the sharpness of class lines the author draws, with the importance she gives to the factions within the Christian Democrats, and particularly with her class analysis of these factions.

This is nonetheless a serious and informative book. It should be widely read.

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MATEO MARTINIC BEROS. *Historia del Estrecho de Magallanes*. Santiago: Editorial Andres Bello. 1977. Pp. 261.

In what he calls "a chronicle of a geographic accident," Mateo Martinic Beros presents for the first time a full account of the history of the Strait of Magellan. Author of several books on the subject, former intendant of Magallanes, and director of the Institute of Patagonia, Martinic is well versed on the strait and has produced an excellent study.

There are five divisions in the book. The first con-

siders the geology and geography of the strait, after which Martinic moves on to his second division, which covers purely historical matters. There is a brief discussion of possible pre-Magellanic voyages, followed by a detailed account of Magellan's discovery and the subsequent voyages, especially the valuable contributions of Sarmiento de Gamboa. Attention is given to foreign penetration, and the Dutch and English receive their fair share of credit for hydrographic studies. Martinic includes a good cartographic history of the strait, which began, he asserts, with its appearance on an Italian planisphere of 1523, the "Padrón Real de Turín." A third division considers the presence of man in the strait, from the aborigines and Spain's ill-fated efforts to found a colony to Chile's foundation of Punta Arenas in 1849. Efforts at economic development receive adequate treatment.

An extensive discussion of navigation in the strait makes up the fourth part. From the difficult and hazardous voyages of the age of sail Martinic takes his readers to the golden age of steam navigation, 1900 to 1914, which was followed by a period of decline that ended only in 1952 after the discovery of oil. Curiously, he ascribes this decline primarily to the imposition of customs duties and the First World War rather than to the obvious competition from the Panama Canal. There is an interesting section on shipwrecks and accidents from the wreck of the *Sancti Spiritus* in 1526 to that of the supertanker

Medusa, which spilled eight hundred million liters of oil into the strait in 1974.

The fifth division is given over to the rule of the strait. A good Chilean, Martinic makes a strong case for his country's dominion. Beginning with Ladrillero's taking possession in 1558 in the name of the king and the governor of Chile, he traces Chile's claim through the centuries to the diplomacy that led to Argentina's acquiescence in 1881. A one-page final tribute to "The Men of the Strait" concludes the work.

Martinic has written an admirable book. It is well organized, contains excellent maps and illustrations, and is frequently enlivened with such fascinating trivia as descriptions of the bottle, barrel, and large zinc box that served successively as a "post-office" in the strait. There are useful statistical tables on navigation and a valuable bibliography. Three appendixes provide a list of fifty-nine maps of the strait, a list of ships that anchored in the Punta Arenas area from 1843 to 1867, and a three-page list of shipwrecks and accidents that highlights the hazards of the passage. Errors are minimal; perhaps most startling is the statement (p. 50) that Balboa named the ocean "Pacific." For those interested in discovery and exploration, this book fills a long-felt need. It should long remain the definitive work on the subject.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

WALTER L. GOLDFRANK, editor. *The World-System of Capitalism: Past and Present*. (Political Economy of the World-System Annuals, number 2.) Beverly Hills, Calif.: Sage Publications. 1979. Pp. 312. Cloth \$18.50, paper \$8.95.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

I would like to reply to Joseph F. Kett's review of my book, *American Medical Education: The Formative Years, 1765-1910* (AHR, 82 [1977]: 729-30). First, I would comment on Kett's assertion that my book provides a "rehashing of familiar material." I admit that much in the book is not "new," but it is an attempt to provide a synthesis that is unavailable in any other single volume. Other more knowledgeable specialists in the history of medicine have been more generous in their comments. Dr. Lester S. King (in *JAMA*, 236: 2674-75), for instance, stated that the book "will have great value for both the cursory reader, who wants a rapid overview, and the scholar, who will be grateful for the workmanlike presentation of data." Dr. George Rosen (in *Change* [April 1977]: 57-58) likewise indicated that, although the material was not "new," "Kaufman has provided a synthesis that can be used by anyone who wishes to understand how American medicine reached its present state of scientific eminence."

Then, I want to comment on Professor Kett's hypothesis regarding the relationship between economics and the low standards in the nineteenth-century medical schools. Among other things the standards did reflect the economic state within the profession, but Kett's statement is too simplistic to be accepted by anyone with any familiarity with the sources. Other factors included the changing state of medical knowledge, the transition from apprenticeship to more formal training, and the competition among the colleges. Kett really oversimplified in his assertion that I indicated that change came after 1900 because reformers now had "a few allies." When those allies included state li-

censing boards, perhaps they only needed a few allies! A school that refused to improve often found that its graduates could not be licensed to practice medicine, and that was quite an incentive to reform. In reality, however, I indicate in my book that there were other reasons for reform coming after 1900. First, there had been a tremendous increase in medical knowledge; now a modern medical education included extensive use of laboratories and hospitals. It was becoming more difficult and more expensive to provide a medical education. When the licensing agencies insisted on reform, many institutions simply could not afford to remain open for business. This was the death knell of the proprietary medical schools that had been financed solely from student fees.

Finally, if Kett's theory is correct, that standards reflected the meager resources of students and the meager compensation of practitioners, increased standards should have reflected a prior increase in compensation. This was not the case, not until the 1940s and 1950s, as a result of the forces set in motion by the Flexner report. Kett's theory, then, is more of a shot in the dark, and it is not supported by the sources.

MARTIN KAUFMAN
Westfield State College

PROFESSOR KETT REPLIES:

The favorable reviews cited by Martin Kaufman describe his book as workmanlike and useful. My own review called it a careful study. Were Kaufman working in an undeveloped field, a workmanlike summary of existing knowledge might well merit accolades. Were this Kaufman's fledgling effort, a reviewer might well have pulled his punches. But there exists an abundant literature on the field of medical education and Kaufman had already published a book on homeopathy, which I gave a favorable review. I expected more from his second book—and found less.

Professor Kaufman's retort to my review provides additional evidence of the dismaying insularity that

marks his study of medical education. Let one example suffice. Like all others who have studied the history of medical education, Kaufman recognizes the critical importance of the period between 1890 and 1920 and the attendant upgrading of medical standards. Yet in explaining the change he never develops a perspective on reformers different from the one they had on themselves. He ignores the growing literature on professionalization, ignores opportunities to make comparisons to the experiences of other professions during the same period, and ignores political, social, and economic history. When confronted by criticism, he takes two tactics. First, he dismisses economic considerations by noting that only during the 1940s (after the transformation of medical standards) did the annual earnings of physicians surpass those of other professionals. But reference to such aggregate statistics scarcely settles the question of whether certain types of medical education were beginning to pay dividends in the form of superior earnings during the 1890–1920 period. Although this question does not exhaust the range of social and economic considerations that Kaufman neglects, it is significant that he never even asks the question. Second, Kaufman reminds us of the important role played by licensing boards and advances in medical knowledge in the upgrading of standards. In my review, two-thirds of which summarizes Kaufman's thesis, I did justice to these factors. But it is still reasonable to ask how these licensing boards were able to acquire such power so quickly against the vested interests spawned by a half-century of proprietary medical schools. Here again Kaufman neither provides an answer nor poses the question.

JOSEPH F. KETT
University of Virginia

TO THE EDITOR:

My *Hitler among the Germans* may make a good target, but it is beyond Geoffrey Cocks's range (*AHR*, 82 [1977]: 1009–10).

Its thesis is not—doubly not—that Hitler's anti-Semitism and expansionism "were the products of a traumatic reliving of his mother's death." Its thesis is that Hitler's anti-Semitism resulted from his mother's death itself while his expansionism originated with his German public. Again, that Hitler prompted the iodoform treatment of his mother by Dr. Bloch is no "link" in my argument. My "source" for that prompting was not Kubizek's testimony, but Hitler's own (see my pp. 16–19, *passim*). Nor do I "prove" the traumatic effect on Hitler of his mother's death just by his "phobic references to cancer": that is artless caricature.

Professor Cocks remarks parenthetically that my "whole argument involving overdosage of iodoform

has been disputed by Bloch's daughter"—close parenthesis. That is disingenuous. Bloch's records show lump-sum charges for daily applications of iodoform gauze. I factored out the iodoform content assuming that Bloch bought 5-meter strips and used about a meter at a time. His daughter contends that a doctor would have discarded the unused remainder of a 5-meter strip. But, first, this contention is false: gauze strips were packaged for use bit by bit. Second, it points nowhere, as Bloch recorded daily applications corresponding to those lump-sum charges. And, third, the iodoform content would come to roughly the same massive overdose even if he bought the gauze in 1-meter strips instead. That is the long and short of the dispute by Bloch's daughter, as Cocks must have known from *History of Childhood Quarterly*, 2 (1974): 265–68.

My concern in *Hitler among the Germans* was "to isolate the few decisive inner demands and constraints" behind Hitler's politics (pp. x–xi). Cocks objects that Hitler's and Germany's "fears" were "overdetermined." But I was expressly distinguishing determinants from overdeterminants. Further, Cocks cites Peter Loewenberg to fault me for ignoring "intrapsychic fantasy as a major motivating factor in human behavior." Understandably, Cocks fails to say what Loewenberg meant. This is that the facts of Dr. Bloch's treatment do not matter—that only Hitler's ensuing "intrapsychic fantasy" does. With that, Loewenberg and now Cocks would take the history out of psychohistory.

Professor Cocks approves me for eschewing "the simple application of psychoanalytical concepts to history." Then sure enough, he himself simply invokes "good" theory to affirm, as against my reading of the Hitler records, that "Hitler's fears" "coursed developmentally from personal to cultural discontents" and the like. It is fun when a pot shot boomerangs like that. But did not my "energetically intelligent contribution" deserve a review in kind?

RUDOLPH BINION
Brandeis University

PROFESSOR COCKS REPLIES:

Rudolph Binion's disavowal of Kubizek's evidence as a link is an indication of the overweening confidence he has in his subjective analysis with its exclusive claim to objective validity. In downgrading Kubizek's "testimony," he elevates "Hitler's own" as the source for his psychohistorical analysis. Such conviction renders my paralytical mention of Mrs. Kren's challenge "disingenuous."

Beyond establishing that Klara Hitler died from iodoform poisoning, Professor Binion must show that this "trauma" was the "determinative" cause for Hitler's career. Although it is clear that Binion knows more than anyone else about the minutiae

surrounding the events of 1907 and that he can muster a slew of Hitlerian utterances in support of his thesis, the vital issue here is not quantitative but qualitative—that is, the evaluation of the meaning of Hitler's words and deeds. Given the mass of material we have on and from Hitler, it is not surprising that historians have found therein almost whatever they have wished to find. Such is the nature of history. Binion turns vice into virtue. In an essay entitled "Doing Psychohistory" (*Journal of Psychohistory*, 5 [1978]: 313–23), he reported happily, "My own odd experience has been that the key pieces always survive, however little else does" (p. 314).

The almost unrestricted espousal of empathy won through the exhaustive investigation of documents is for Professor Binion the psychohistorian's credo in opposition to what he sees as the ahistorical application of theory and model. The issue raised is a valid and important one, but, and here is the failing in Binion's attitude as I see it, the resultant defense of his work suffers from an absolute conviction that parallels the search within the work itself for certain causes rather than conditions. Thus, Binion's distinction between determinants and overdeterminants begs the crucial question of the certainty with which we may accept his thesis that Hitler's "Bloch complex" in its unwinding and entwining with the German trauma was the cause for his actions. History cannot be replicated and psychohistorical interpretation cannot be verified like a scientific hypothesis. In terms of having his thesis empathically confirmed by other historians, Binion is therefore left with a tautology: "If you see things my way, you'll agree with me." This is one great danger in too great a reliance upon empathy.

Professor Binion himself, in any case, is not free of what he sees as the burden of theory, since his interpretation of Hitler's words and deeds is informed by a variation of orthodox psychoanalysis peculiarly attractive to the historian who seeks strict cause and effect. In this model of historical causation the trauma is an "event" in service to a singular certainty and as such omits the appreciation for multiple causes, process, and change that marks the best historical inquiry. If, however, this model is so compelling, what is it about other models, theories, and cases of human behavior that make them less so? Modern psychoanalysis, more in line with historical inquiry, investigates the total character within a psychosocial life-history rather than reducing symptoms back to a trauma. I have no objection to declaring a central cohering purpose in Hitler's life—his vision of the Jew certainly fits the bill—but the reliance upon a single mechanism whose operation may be detected by a subjective immersion in his words is much too problematic. Binion's lame dismissal of the Soviet autopsy and his hesitant espousal of the testimony of Hitler's physicians re-

garding the number of *Führerhoden* is an example. If Bromberg and Waite, who are the chief proponents of the cryptorchism theory, are right, then Binion's claim to exclusive authority is empty. The limitations upon the use of empathy are internal as well. The historian knows how things are going to turn out. That's at once an advantage—that is, knowing what to look for, but at the same time a circumscription of the ability to become one with the subject aside from whatever psychological baggage the historian brings along. What is crucial is balance: a proper concern for letting the documents speak for themselves, the coincident use of objective and subjective interpretation, and the careful application of theory and case study. The reward is greater: Hitler and the Germans do not just touch at the points of trauma (or does a trauma have edges?) but reflect and reveal each other in a complex of conditions and happenings that does justice, even in its tragic moments, to the richness of history.

GEOFFREY COCKS
Albion College

TO THE EDITOR:

In his review of my book, *The Frankfurt School* (AHR, 83 [1978]: 978–79), Paul Breines accuses me of equating sociology with functionalism without giving any proof for his nonsensical statement. His error is somewhat mitigated by the fact that as a historian he does not know the field well enough to make any valid judgment.

I was glad, however, to see that he was unable to come up with any substantive error in my book; I am sure he was searching for it as only a conscientious positivist researcher could. He found my only shaky relation to the facts in the "unfounded assertion" that the Frankfurt School accepted Dimitrov's definition of fascism, a definition that supposedly appears "too often." How often, may the reader ask? Exactly twice. And, since quotations are provided on pages 74–75, the closeness of the definitions of Dimitrov and Horkheimer can easily be compared by the readers of the book. I find therefore that Professor Breines's own "relation to the facts of my book are not always felicitous."

Professor Breines erroneously claims that my notion of Marxism, against which I measure the alleged Marxism of the Frankfurt School, is based on "the hollow assumption that Marxism is what Kautsky or Plekhanov or Zhdanov said." Had Breines really read the book he would have found that its notion of Marxism is based on the theories of Marx, Engels, Lenin, Lukács, Korsch, and Bloch. Should he still harbor some doubts, he could consult the name index and find out that Zhdanov appears only in his imagination and that Kautsky and Plekhanov are mentioned in one footnote.

According to Professor Breines, I saw the diverse writings of Horkheimer and Adorno in the 1930s and 1940s as "contradictory phases" in their careers instead of regarding the essays as "historical components of a unified account of fascism." I wrote about the shifting emphases of the authors; indeed, prior to 1939, their essays had very little to say about fascism specifically. Breines further complains that I ignored the central place of Hegel in the work of the Frankfurt School and its attempt to fuse it with Jewish insistence on the primacy of ethics. I wish he would enlighten us and *demonstrate* the central place of Hegel in the life-work of Horkheimer.

In conclusion: it is gratifying to know that in judging the work of the Frankfurt School I am in the good company of some of the seminal thinkers of European intellectual history such as Lukács, Bloch, Leszek Kolakowski, and Michael Landmann. True, Lukács and Bloch were not "impartial observers" of twentieth-century European history (intellectual and other), but neither is Paul Breines an impartial observer in view of his pronouncements of ten years ago, which he may now term as "youthful aberrations" but which still influence his evaluation of a work (and the result of its analysis) that does not conform to his ideological bent.

I agree with Professor Breines that my book is not a substitute for the books of Jay and Rusconi, but it was never intended to be. I attempted a *sociological analysis of the sociological theories* of Horkheimer and Adorno, as I clearly stated on page 10. If Breines is not familiar with the discipline, he should have declined to review the book.

ZOLTAN TAR
City College,
City University of New York

PROFESSOR BREINES REPLIES:

I was honored that in his book, *The Frankfurt School*, Zoltan Tar saw fit to cite a pronouncement of mine from ten years ago. It was part of an enthusiastic look at some of the late Herbert Marcuse's ideas and their impact on young intellectuals in this country. I do not know why he suspects that today I might consider it to have been a youthful aberration. For the record, I stand by it still, as I stand by every line of my brief review of his own book. As to the good intellectual company in which Zoltan Tar believes he is traveling, the claim eludes me. In their published judgments on the Frankfurt School thinkers, Georg Lukács and Ernst Bloch limited themselves to nasty little remarks, while Leszek Kolakowski's reckoning with Marcuse in particular is part of his larger critique of Marx, Engels, Lenin, Lukács, and Marxism *tout court*. I am sorry Zoltan Tar felt compelled to impugn my ability to ap-

praise his book. The review was not *ad hominem*, nor will this rejoinder be.

PAUL BREINES
Boston College

TO THE EDITOR:

In his review of my book, *From Saladin to the Mongols* (AHR, 83 [1978]: 1305-06), Michael Dols sharply criticizes two elements of my interpretation of Syro-Egyptian political history in the period 1169-1260. These criticisms are in my judgment not merely erroneous but seriously misleading.

First, Professor Dols rejects my thesis (given on pp. 3-4) that the period under study saw the emergence of a decisive change in the political and administrative relationship of Egypt and Syria. I argue that this change ushered in a long period (from 1260 to 1516) when Syria was embedded within a stable and highly integrated administrative system that subjected it to the direct control of a government resident in Cairo. Dols baldly asserts that this long period represents simply a revival of "the customary relations between Egypt and Syria" and that "Egyptian hegemony over Syria-Palestine" is almost a constant feature of the Islamic era. To this assertion two responses may be made. First, as Dols admits, there was a long hiatus in this traditional pattern—a hiatus extending from 1075 to 1260, or 185 years. One cannot explain the reversal of such a long-term trend by saying that it represents simply a return to "customary relations." Obviously something happened to usher in a new era of Egyptian hegemony, and my book is in part an attempt to identify and analyze this "something." Dols regretfully says not a word about this analysis; he does not even allude to its existence. The second response to Dols's assertion is somewhat different. That Egyptian rulers often felt it necessary or desirable to try to dominate Syria I do not dispute, and this point is very plainly made at many places in the book. I do deny, however, that such attempts very often succeeded or that *before 1260* they were ever based on durable, effective, and highly articulated administrative institutions. Anyone who compares the long-lived Mamluk state (1250-1517) with the Tulunid (868-905) and Ikshidid (935-69) polities will see what I mean. Nor is the desperate Fatimid attempt to control Syria between 969 and 1075 in any way comparable to the Mamluk administration in that region. Again, the Mamluk achievement must be related in some way to trends or processes that occurred in the time of its Ayyubid predecessors, and this issue—which is central to the whole book—is not even mentioned by Dols.

Professor Dols's second criticism is aimed at a related thesis, which in fact underlies the one just discussed: that is, a change "in the political goals of the

Ayyubid elite . . . from conscious decentralization to unitary rule." Dols rejects this, saying that "the goal was consistently one of political unity despite the demands of dynastic rule." The whole point of the book is precisely the conflict between decentralization and unity and how this conflict was ultimately resolved in favor of the latter. My argument (which Dols seems not to perceive) can be given in a few sentences. From the 1180s, when Saladin divided his kingdom among his children and kinsmen, down to the death of al-Kamil in 1238, the Ayyubid polity is best understood as a family confederation—as a structure in which political sovereignty was thought of as belonging to the ruling house as a whole and in which each of the more prestigious princes of the ruling house was given a territory to rule as an autonomous principality. The only effective principle of unity was the deference and respect of these territorial princes for the senior member of the ruling house, who was chief of the confederation. On the other hand, this latter figure had to establish some degree of cohesion and common policy, if only to ensure the survival of his house's sovereignty. But insofar as unity was achieved, it was through personal influence, the exploitation of kinship links, diplomacy, or force of arms; down to the death of al-Kamil, there was never any attempt by the head of the confederation to replace the other princes with his own officials—nonmembers of the Ayyubid family who would be dependent solely on him for their rank, status, and power. Only with al-Salih Ayyub (1240–49) was there a serious effort to evolve formal administrative machinery that could support a centralized autocracy, converting former principalities into provinces. With al-Salih Ayyub, we have a clear and definitive change from political decentralization to unitary rule. But this was *not* a change of goal—the goal had always been unity—but a change in the means used to attain that goal. This theme is developed at many points in the book (for example, pp. 10, 62–63, 66–75, 125, 161, 199, 239–240, 283–301); it is unfortunate that Dols overlooked it.

Finally, Professor Dols contests my focus on Damascus, which he believes has the effect of obscuring a political evolution "that was taking place most clearly and significantly in Egypt." Exactly what was occurring in Egypt he does not say, so it is hard to respond to this. I will say that I chose a Syrian focus for my work because it demonstrates that resistance to Cairo's attempts to impose unity was not the work of fractious underlings but of established princes of the ruling house who regarded themselves as rightfully endowed with their possessions and legitimately seeking to define their own interests and establish mutual ties with one another as they saw fit. Dols says that this approach gives

the impression of "interminable warfare" among the princes. Could any other impression be valid when Damascus was subjected to siege eleven times between 1193 and 1260—every time but one by Ayyubid forces?

R. STEPHEN HUMPHREYS
University of Chicago

TO THE EDITOR:

Reviewers have a heavy responsibility. If they fail to exercise their critical faculties, a gullible public may be led astray. But, if they overexercise them, a serious work of scholarship may be ignored, and their readers may be deprived of intellectual stimulation. It would be a tragedy for all concerned if transatlantic scholars formed their decisive impression of Alfred P. Smyth's important work, *Scandinavian Kings in the British Isles, 850–80*, from Roberta Frank's dismissive review (*AHR*, 84 [1979]: 135–36). In writing this rejoinder, I have no wish to offer a new review or to rebut the (few) points of intellectual substance in Frank's. My own review may one day emerge from the strike-bound presses of the *TLS*, and Smyth is well capable of defending himself. I merely wish to demonstrate that Frank's review is neither accurate nor just (as is indeed hinted by its sneering tone) and to urge readers to discover this for themselves.

The main impression conveyed by Professor Frank is that Smyth is rehearsing familiar and discredited theories about the place of Ragnarr Loðbrók and his sons in Viking activity around the British Isles, without acknowledgement, and without regard for the difficulties raised by others. It is admittedly unfortunate that Smyth had no chance to give more than a cursory glance to R. W. McTurk's careful paper for the *Seventh Viking Congress* (a fact concealed by the delay in his book's publication), and it is true that Smyth prefers to deal directly with the evidence rather than debate it with fellow scholars. Nevertheless, it is quite unfair to imply that he conceals difficulties. To read Frank's review, one would never guess that Smyth *does* make clear that Ari was the first to speak of Ragnarr Loðbrók as "one and the same individual" (pp. 263–65); that he is far from committed to the identification of Ragnarr with the Reginheri who sacked Paris in 845, even if he does not discuss the difficulties here (pp. 31, 98); and that he does indeed attempt to explain how the Ragnarr legend proliferated, and very interesting the explanation is (pp. 36–67, 98–100).

I also share some of Professor Frank's irritation with Dr. Smyth's lack of intellectual generosity in a few cases (notably Professor Byrne, but also Sir Frank Stenton). But there *are* several truly original

insights here—for example, in Smyth's neat removal of Ragnarr from the main line of ninth-century Danish kings, which solves most of McTurk's problems at a stroke (pp. 1–35); in his acute observation of the Viking tendency to attack on church festivals (pp. 148–49, 155–56, 181–82); in his remarkable discussion of the Scandinavian slave trade (pp. 154–68). More important, those of Smyth's suggestions that have been made before have never, as Byrne would be the first to admit, been put so fully. It is precisely because of this width of reference that the whole edifice does *not* collapse, “like a house of cards if anyone is churlish enough to sneeze.” For example, Byrne's few crisp sentences had already virtually disposed of Mrs. Chadwick's objections to the identification of Inwaer in the English sources (where he is not a “Danish chieftain” but one of two leaders of the 865 *micel here*) with Imhar in the Irish (where he is not “one of the leaders of Norwegian Dublin” but *rex Nordmannorum totius Hiberniae et Britanniae*). But it is Smyth's detailed exposition here and in his work on Ivarr's descendants at Dublin and York that has put it beyond all reasonable doubt. (Incidentally, the most important evidence for Inwaer's leadership of the Great Army is not the *Anglo-Saxon Chronicle* but, as Smyth failed to note, the Life of Archbishop Oswald, grandson of a member of the army; this *Vita* was written at Ramsey Abbey, the residence 985–87 of Abbo, whose *Passion of St. Edmund* was arguably the source of all subsequent English knowledge of Ivarr's role.) If, from historical sources alone, the founder of the Danelaw can be shown to have been the King of Dublin, then quite regardless of the sagas the *onus probandi* is shifted onto those, like Frank, who would doubt Smyth's other identifications chiefly on the grounds that the wide-ranging Viking activities involved were absurd *per se*.

I do not say that Dr. Smyth is always right, or that there are not serious difficulties involved in some of his ideas. But it is extremely important that his views are carefully analyzed and discussed by those, like Professor Frank, with the Old Norse expertise to do so, and it will *not* do to misrepresent or dismiss them out of hand. I would add that I write as a friend of Smyth's without any special qualification other than that of being a historian of early medieval England and Europe. But, lest it be thought that this discredits my intervention, I am authorized to say that these are also the views of Peter Sawyer, the leading English expert on the Viking Age and the first to put powerful arguments against sanguine use of saga evidence. *Caveat lector* is a tag more appropriately applied to Professor Frank's review than to Dr. Smyth's book.

C. PATRICK WORMALD
University of Glasgow

PROFESSOR FRANK REPLIES:

C. Patrick Wormald says he shares my concern with some of Alfred P. Smyth's scholarly procedures. He mostly scolds me for my sharp tongue and pretends to demonstrate that my review is neither accurate nor just. I dared to complain, for example, that the author tries to spare his readers facts that seem to confuse or contradict his account. The points of rebuttal raised by Wormald illustrate rather than undercut my complaint. He confirms that Smyth waits until the final three pages of his narrative to acknowledge that the names Ragnarr and Lodbrók are not found together until the twelfth century and to admit that the historicity of this legendary figure is “difficult to establish.” I find so late a retreat disquieting in a book that from page one assumes the historical existence of a single Ragnarr Lodbrók. I am glad to learn, on Wormald's say-so, that the author is far from committed to the identification of Ragnarr with the Reginheri who sacked Paris in 845 (the contemporary *Annales Xantenses* reports that the Viking leader died soon after the raid, thereby making it inconvenient for him to visit England around 850); why, then, if it is unnecessary for his argument, does Smyth insist (pp. 31, 98) that the identification of Ragnarr and Reginheri is “plausible,” “probable,” and “chronologically possible”? This concealing of difficulties (to use Wormald's phrase) may be part of a larger problem: Smyth seems to have a theatrical flair for highlighting whatever late or inconclusive evidence is available and dispatching good evidence to the wings (a kind of Gresham's law of scholarly hoarding). Wormald tacitly comments on this tendency when he introduces his own proof for Inwaer's leadership of the Great Army—from a source, he notes, not mentioned by Smyth.

As for the remaining factual objection raised by Dr. Wormald, I am quite aware that Dr. Smyth attempts to outline the early development and transmission of the Ragnarr legend. My criticism, which the letter misrepresents, is twofold: the author did not try hard enough to distinguish chronologically the various layers of source material at his disposal, nor was he able to explain to my satisfaction the strong European interest in the legend around 1200. Unlike Wormald, I am not as worried about Smyth's “lack of intellectual generosity” as I am about his revival of a haphazard eclecticism that was long ago made obsolete by the advent of systematic source criticism.

Dr. Wormald lists what he considers to be three original insights of Dr. Smyth's book. We all have our favorite nuggets; his do not strike me as glistening so brightly as all that. For example, the chapter on slave trading makes much of the shipment of captives from the British Isles to the Islamic west

and even Byzantium, while failing to find contemporary evidence that a single such shipment was ever made. Some may applaud Smyth's chutzpah and call it "remarkable"; others, like myself, will prefer proof to innuendo and consider Smyth's account unwholesome and misleading. I also differ from Wormald on the utility of the author's "width of reference" (my review says "to flesh out"); the chief result, as I see it, is an entertaining narrative that borders on the sensational. No one reading my colorful description of Smyth's book could possibly conclude that the latter is lacking in interest or stimulation.

The final sentence of Dr. Wormald's third paragraph has me genuinely puzzled. All serious students of the period recognize the wide-ranging, international character of ninth- and tenth-century Viking enterprises; I do not doubt Smyth's identifications on the grounds of Viking immobility, and I cannot believe that this is what Wormald really means to insinuate. Is he saying that an acceptance of the Inwaer/Imhar link compels one to approve automatically Smyth's subsequent identifications (for example, Óláfr inn Hvíti/Óláfr Geirstaðaálfr; Caittil Find/Ketill Flatnef) and that anyone who balks has the burden of proving that they are not one and the same? I hope not.

It is too bad that Dr. Wormald's own review of Dr. Smyth's book has been delayed. Nevertheless, I believe that he underestimates the sophistication of transatlantic medievalists if he thinks that they will form their "decisive impression" of Smyth's book from a review limited to 500 words (shorter by some 250 words than his own letter). Dr. Smyth's friends have no reason to fear that his contributions (and theirs) are unknown in North America and that his work will be ignored. Smyth's previous book, which roused high expectations for the current volume, has received long and thoughtful appraisals in specialized journals. R. W. McTurk's careful, four-page review in the *Saga-Book of the Viking Society* (1977) is particularly impressive. Writers of bland books get bland notices; studies as ambitious and provocative as Smyth's run other risks.

ROBERTA FRANK
University of Toronto

TO THE EDITOR:

Nagged by the suspicion that he gave it no more than a casual perusal, I take little delight in Sheldon Harris's grudging admission that my book, *The Golden Age of Black Nationalism, 1850-1925*, is "well-researched, well-written, and most interesting" (*AHR*, 84 [1979]: 262. I wish that he had troubled himself to quote me accurately and that he had discussed the contents of my book rather than just the title.

Professor Harris states the obvious when he observes that "the period under study, 1850-1925, would hardly seem to be a 'golden age' for black Americans, no matter what their pan-African views, . . . and even the enthusiastic author of the blurb on *Golden Age's* book jacket concedes the period 'would hardly seem to mark a golden age.'" These words indeed appear in the publisher's blurb, but they are taken almost verbatim from the second sentence of my preface (p. 9). Far from being the concession of an enthusiastic publicist, they mark my own awareness that a golden age in a people's literary or intellectual history may coincide with a dismal period in their social history. J. A. Hobson, Hans Kohn, Elie Kedourie, and others have long recognized that the flourishing of nationalist ideology often coincides with periods of tribulation. I am amazed that anyone finds this a controversial or troublesome idea.

I am even more amazed that Professor Harris refers to Alexander Crummell as an obscure figure. Certainly he has not forgotten August Meier's observation in *Negro Thought in America, 1880-1915* that Crummell is "generally regarded as the leading nineteenth century Negro intellectual" (p. 42). Although Theodore Draper displayed unconscionable ignorance of him in *The Rediscovery of Black Nationalism*, historians like W. E. B. DuBois, Benjamin Brawley, Otey Scruggs, and Hollis Lynch have long recognized the magnitude of Crummell's contributions.

I cannot respond to Professor Harris's attack on my definition of nationalism, for he does not state his objection. He simply asserts that my definition is "murky" and "frequently contradictory." Perhaps he simply missed my point that classical black nationalism, from Delany to Garvey, was indeed murky and contradictory. In this respect it was no different from other nineteenth-century nationalisms. Whatever Harris meant, he would have had ample room for clarification, if he had not chosen to quote me for twenty-six lines of a seventy-one line review. Under the circumstances, I might have been relieved that the reviewer allowed me to speak for myself, but Harris misquotes me at the beginning of his second column, where he mars my prose with an illogically constructed sentence. I make enough mistakes of my own without taking the blame for his faulty parallelism.

As for the assertion that my book is a collection of essays, I fear that the thematic unity of my chapters and the process of change that they describe eluded Professor Harris. He seems unaware that I am using black nationalism as a vehicle for discussing more general problems in nineteenth-century intellectual history, for example, the confusion of the concepts of culture and civilization. Among other evidences of carelessness and superficiality, there is the failure

to include all publication details. The book is 345 pages long, which information he should have realized was missing when he saw the proofs of his review.

WILSON J. MOSES
Cambridge, England

idea what it weighs. But as professional historians we should be more concerned with the quality of a study rather than its weight or length.

SHELDON H. HARRIS
California State University,
Northridge

PROFESSOR HARRIS REPLIES:

I would hope that Wilson J. Moses is a better historian than he is a seer. As with any responsible reviewer, I read his book quite carefully. That I was less impressed with his study than he is is unfortunately true. My review, however, is balanced and fair. I find no reason for apology.

As for specific points in Professor Moses' unhappy lament: (1) I do quarrel with the appropriateness of his title. Even accepting his thesis concerning a golden literary age with a dismal period of social history, his study covers the wrong period. 1850–1925 was a dismal era for all black Americans, and to call any aspect of their experience "golden" reveals an egregious lack of sensitivity on the part of the person making such an assertion. Should we expect someone to write a sequel to Moses' study and entitle it "The Golden Age of Racism, 1850–1925"? (2) With all due respect to August Meier, Alexander Crummell was not a familiar household name in the period under review, nor is he today. (3) I did not "miss" the point that "classical black nationalism, from Delany to Garvey, was indeed murky and contradictory." Moses never really defines black nationalism. Anyone and everyone is thrown into a kettle labeled "Black Nationalism." Surely, he is familiar with Professor Redkey's study as well as Professor Rudwick's and Bracey's works. And why begin with Delany? Paul Cuffe and other Afro-Americans of an earlier era contributed to a definition of black nationalism. Moses is indeed "murky" in his definition and use of the term "Black Nationalism." (4) I did quote extensively from *Golden Age*. The purpose of the quotations was to inform the reader of what the author was trying to say. Since Moses is unhappy with my written comments, he should be grateful that I devoted so much of my review to direct quotation. Misquotation is in the eye of the beholder. I fear that the "illogically constructed sentence" that Moses refers to is only a reflection of his own writing style. (5) Moses believes he has written a book that has thematic unity. I do not. He has written a series of essays, some of great value, that are held together solely by a very loose use of the term "Black Nationalism." (6) Moses' complaint concerning omission of publication details should be lodged with the editor of this journal and not with me. I am not responsible for the *AHR*'s review format. I should add, however, that I did not place his tome on a Toledo scale. I have no

TO THE EDITOR:

Renate Bridenthall's review of my book *Frauenarbeit im "Dritten Reich"* (*AHR*, 84 [1979]: 485) makes it difficult for me to recognize my own arguments. I do not want to list the many inaccuracies and distortions in what the reviewer pretends to be a reproduction of my conclusions. There is, however, a major omission. Bridenthall does not mention at all that my book disproves the influential interpretation that attributes to the Third Reich a modernizing or even revolutionary impact on German society. Even more questionable is Bridenthall's dealing with what I have written about the role of Hitler.

My critic maintains that I come "dangerously close to apologia" with my conclusion that Hitler's decision concerning the mobilization of women was based mainly upon irrational ideological reasons and that his prohibition of a draft law for women in wartime was not a sign of political opportunism. Bridenthall denies the possibility that Hitler believed in his own ideology. She describes Hitler and the other party leaders as mere opportunists. Bridenthall seems to subscribe to the vulgar Marxist assumption that Hitler and the Nazis were nothing else than agents of capitalism and considered their ideology exclusively as an instrument destined to dominate the masses and to disguise their real purposes—namely, the defense of bourgeois interests. If this were true, how could we explain the utmost atrocities of the Nazis such as the killing of six million Jews?

Those who deny the destructive potential of irrational forces will always be inclined to push them aside as "dysfunctional" or "theoretically unimportant." The inevitable result of this type of interpretation is that a regime such as the Third Reich looks much less cruel than it was in reality.

DÖRTE WINKLER
Stegen-Eschbach,
West Germany

PROFESSOR BRIDENTHALL REPLIES:

I can best respond to Dörte Winkler's letter, point by point, by referring back to my original review.

First, I would like to repeat from it that Dörte Winkler's research is thorough and her scholarship strong, though I take issue with some of her con-

clusions. Second, I acknowledged that Winkler showed the Nazis' stress on the traditional sector of the economy *retarded* modernization, but surely we both agree that they did not cause capitalist development either to stagnate or regress but allowed it to continue. Third, the taint of apologia for Hitler appears to have been very unsettling, so I would like to point out that in the review I added the phrase "surely not intended by Winkler."

Fourth, I did not dismiss Hitler as a "mere" opportunist or, even more mechanically, as an "agent" of capitalism. Rather, I described him as "the 'little man' incarnate, of the bourgeoisie, especially the petit bourgeoisie." That means he was not simply a tool or a puppet, but a true representative, literally, of his class. The sincerity, however real, of his and of its belief in their ideology matters less than the fact that it was self-serving—that is, political. Sincerity usually remains opaque, among the dead even more than among the living, and concerns the historian less than does the more telling question: "A cui bono?" In the case of women, the fact remains that bourgeois women evaded war work and accepted Nazi "protection" of their "womanly role." Working-class women were not spared the rigors of patriarchy.

Fifth, mentioning the atrocities seems a non-sequitur in this context, except as an anti-Marxist aside, so I will address it as such. Marxists do not dismiss irrationality but consider it as being at the core of capitalist dynamics. Thus, irrationality is not dysfunctional; it is one of the meanings of the term "contradiction" in a dialectical movement. Scapegoating—of Jews, blacks, women, and the like—is certainly not theoretically unimportant and is usually quite functional as a diversion from social division by class. An aside of my own: the automatic gluing of the word "vulgar" to the word "Marxist" is itself vulgar and a time-worn cliché. By now, even an *ad femina* insult would be preferable.

RENATE BRIDENTHAL
Brooklyn College and
The Institute for Research in History

TO THE EDITOR:

Alexander Orbach's review of my book, *The Jewish Intelligentsia and Russian Marxism* (AHR, 84 [1979]: 504–05), notes two problems with the work. These appear to be sufficiently serious to warrant a reply.

The stated purpose of the book is to begin constructing a structural theory of intellectual radicalism and ideological divergence through the use of qualitative and quantitative data on over two hundred Jewish members of the Bolshevik, Menshevik, Bundist, and Poalei-Zionist parties in turn-of-the-century Russia. In the course of my research I came

to the conclusion that a substantial portion of the variance in the ideological views of the persons in my sample can be explained by examining the paths of social mobility that they traversed in the course of their careers: they tended to learn culture patterns associated with the different positions that they occupied in society at various times, and this learning predisposed them to join one party rather than another.

Professor Orbach questions the usefulness of my theoretical approach on the grounds that it is "highly restricted in its application": my explanation may work well for intellectuals in the four parties I have chosen to examine, but not, apparently, for anarchists, populists, and the like. One problem with this criticism is that it may or may not be valid. We do not know since no comparable data are as yet available on anarchists. More important, perhaps, it suggests a quite peculiar attitude to the role of theory in sociohistorical research: rather than trying to reformulate theories that work better than others, so that workable theories can accommodate a still broader range of data, Orbach suggests that we reject entirely the workable theories if any apparently contradictory evidence can be presented. On this advice, we could hardly expect any progress at all in the realm of sociological theory construction.

Professor Orbach also states that my theory is marred by an internal inconsistency: if, as I sought to demonstrate, the social origins of Poalei-Zionists differed very substantially from the social origins of Bolsheviks, how can one explain the fact that the two groups of intellectuals came closest to each other on certain doctrinal issues? The answer has to do with certain similarities in what I refer to as their "social destination"—that is, the classes and strata of Russian society to which they were, because of their social circumstances, more or less compelled to attach themselves when they entered adult political life. I discussed this point at considerable length in the book, and I cannot understand how it managed to escape Orbach's attention.

ROBERT J. BRYM
University of Toronto

TO THE EDITOR:

I would like to comment on John Burnham's review of Nathan Reingold's *Papers of Joseph Henry*, vol. 2 (AHR, 84 [1979]: 547–48), because it has raised important issues for those of us (like myself) who are currently engaged in preparing selected editions of the papers of scientists or physicians.

Professor Burnham makes three essential points in his review. First, he maintains that a selected edition of papers, such as the Henry Papers, does not satisfy the needs of the "true scholar," who needs all

of the papers of a given collection before him. Second, he argues that many of the documents selected by Dr. Reingold and his associates are trivial. And, third, he claims Dr. Reingold's annotations are in large measure exercises in antiquarianism. Burnham's assertions raise all sorts of questions. What should the criteria be for selecting and editing documents? Should the annotations of such documents be detailed? And, finally, what is the ultimate usefulness of such selected editions?

I believe that projects like the Henry Papers are not only important and useful; they are, in fact, necessary. The history of science and medicine in this country are relatively young disciplines. Many of the manuscript collections that inform these disciplines are still in the process of being collected and organized. In recent years, many historians who work in related fields of intellectual, social, and urban history have increasingly found materials from the history of science and medicine useful in reinterpreting our history. They, as well as those historians who are currently being trained in the history of medicine and science, need well-edited, selected documentary collections in the history of medicine and science, easily at hand, to add to their store of information on which to sharpen their thought and understanding. As a historian of medicine, I would give my eye teeth to have the entire corpus of Benjamin Rush papers before me. That wish, however, does not impair the usefulness or value of Lyman Butterfield's selected edition of the Rush papers. I have used that edition for well over a quarter of a century to introduce my students to problems in the history of medicine. I cannot begin to detail the wealth of information and ideas that edition has given me.

Professor Burnham claims that many of the documents Dr. Reingold and his associates chose for volume 2 are trivial. Trivial in relation to what—the other documents in the Henry collection or innately trivial? Are not most documents trivial until they are given meaning by the imagination and understanding of the historian? There is no faulting the historical acumen and imagination of Reingold and his associates. The documents they have chosen not only illustrate the process of Henry's research, they inform as well the professionalization of science in the United States and family, political, and intellectual history.

Professor Burnham complains that many of Dr. Reingold's annotations are obvious and tell him more than he really wants to know, or will ever need to know, "except in the most bizarre circumstances." Burnham's complaint is more revealing of Burnham than of Reingold. I found the reverse to be true. For me, the annotations were worth the price of admission to the volume. For example, while it is true that Reingold's annotation of the

document, *Henry's Galvanic Experiments on the Murderer Le Blanc*, were longer than the document itself, it illustrated in the most extraordinary fashion the impact of the work of Dr. P. M. Roget (of Roget's *Thesaurus*) on the subsequent development of Henry's thought and research. As a historian of medicine, I further appreciate the rich biographical detail that Reingold and his associates supplied about physicians who lived in the Albany and New York areas and, in particular, about their relationships to the rest of the contemporary intellectual community in the United States.

I would like to add here that Dr. Reingold's annotations have one other important virtue: they stand as guides both to the literature of science in Joseph Henry's day and to the important secondary literature in the history of science in our own time. Professor Burnham believes otherwise. He complains that Reingold and his associates, for all of their scholarship, have overlooked George Daniels's pioneer study, *American Science in the Age of Jackson*. While Burnham does not say it outright, he suggests that Daniels's study was overlooked for some malign personal reason. It is true that Daniels's study is not cited. Still, the study in question was cited several times in volume 1. In addition, Daniels's edited volume, *Nineteenth Century American Science: A Reappraisal*, is cited in volume 2. One can only assume that Daniels's earlier study was not cited in volume 2 because it did not serve to explain the documents that were chosen.

Dr. Reingold's edition of the Henry papers might well serve as a model in preparing selected editions of the papers of scientists and physicians. No matter what the model, we need to increase well-edited, selected editions of such manuscripts if we are to have truly new historical interpretations of our history.

SAUL BENISON

University of Cincinnati

PROFESSOR BURNHAM REPLIES:

The allocation of resources and the use of the scholarly community to facilitate a researcher's communicating his findings are matters about which there may be honest differences of opinion. I am grateful for the opportunity to try to summarize one point of view.

The *Papers of Joseph Henry* provide an excellent example of painstaking scholarship that is not channeled so as to achieve optimum benefit. Much of the effort that went into selection and annotation could have produced instead some excellent articles and notes that would have been available in refereed and indexed journals. As it is, little gems and some important essays are buried in a relatively inaccessible place and cannot be located by any index or similar bibliographic aid (and this is particularly

hard on researchers from neighboring subjects or colleagues dependent on small libraries). The very erudite among us may be able to remember each such discrete item or take notes with elaborate cross references. But most of us will miss benefiting from some of the editors' work because it is not indexed or even placed in a volume or article with a title that describes clearly the major subjects discussed therein.

Sam Benison's elaborate defense of the omission of references to Daniels's standard monograph bears out my point. Had any of the important essays embodied in the footnotes of this volume been submitted to a good journal, the journal editor and his referees would have made sure that the author related the essay to the important literature in the field, thus doing the author the favor of increasing the significance of his work and the reader the favor of showing how scholarship is cumulative.

The question of trivial and antiquarian material presents the temptation to win debater's points by using ridicule—that is, citing amusing and outrageous examples from the volume in question. I would prefer simply to say that once again editors and colleagues can provide reasonable compromise concerning such matters. As scholars we continually discriminate and make judgments. I am glad that we do, and I wish that the published Henry papers showed more evidence of the editor's sense of proportion (and that of his advisors and sponsors) and less of assiduous accumulation.

I believe that money spent on such projects as the Henry papers could be better spent on microfilm editions of collections and on research support for articles, monographs, and books of broad synthesis and interpretation. Comments by some reviewers of similar volumes of great men's papers suggest that I am not alone in these sentiments.

JOHN C. BURNHAM
Ohio State University

TO THE EDITOR:

Alexander De Grand's review of my *Fascism in the Contemporary World* (AHR, 84 [1979]: 714–15) contains an unusually large number of incorrect and misleading statements. For brevity's sake, I will rejoin only to five.

First, Professor De Grand believes the "curious" thesis that many authoritarian regimes in late-modernizing societies possess marked similarities to fascism to be my own invention. This is very flattering, but he will find this thesis advanced by A. J. Gregor, A. F. K. Organski, and John Weiss, all cited in my bibliography and acknowledged in the preface. De Grand could also have found Gregor's version of the thesis in the foreword. Second, De Grand seems scandalized at my characterization of Mussolini's regime as developmental. But the same scandalous

characterization appears in the works of A. J. Gregor, Ernst Nolte, Roland Sarti, Domenico Settembrini, Eugen Weber, and many others. Third, De Grand finds my view of the Italian Socialists' responsibility for Mussolini's rise to power the result of a "simplistic and tendentious rendition of Italian history." He might have informed the reader that my position is shared by Denis Mack Smith, H. Stuart Hughes, Maurice Neufeld, Angelo Tasca, and others (see pp. 38–39). Fourth, De Grand says that Vargas was not "brought into this conceptual framework" of Third World fascism, but in fact Vargas's nationalism, corporatism and authoritarianism, and his resemblances to Perón are noted on page 185. Fifth, De Grand states that I did not use "non-English sources in anything but translation." Now how does he presume to know what sources I used? All he knows is that my bibliography is *explicitly* limited to English-language books, with advice to readers of other languages about where to find non-English works on fascism.

Having shown himself hardly more familiar with my book than with the literature on which it is based, De Grand then reveals his notion of the bounds of legitimate scholarly controversy by actually stating that my book "verges on fascist apologetics." I am not sure what good purpose is served by having a book reviewed in a professional journal in the style of the popular pamphleteer.

ANTHONY JAMES JOES
Saint Joseph's College,
Philadelphia

PROFESSOR DE GRAND REPLIES:

Anthony James Joes feels that I have misrepresented his thesis and underestimated his research. My review, however, was directed at the simplistic and mechanical application of the thesis and, more particularly, at his attempt to establish a connection between Italian fascism and a grab bag of societies in different historical contexts and at vastly different levels of development.

I could hardly be unaware of the origins of Professor Joes's ideas, but the sweeping application of the "fascism as modernization" thesis makes it lose all subtlety and much of its validity. The only connection between Joes's interpretation of Mussolini (point two) and the work of Nolte, Sarti, *et alia* is that a rotten and a ripe apple are both apples.

It is true that the Socialist party has been singled out for special criticism by several of the authors mentioned in point three. Again, what I objected to was Professor Joes's tendency to heap so much of the responsibility on the left that it, rather than fascism, becomes the villain. Underlying his whole section on Italy was a justification of fascism as a positive response to the Italian crisis of 1919–20. Largely ignored were the tragic dimensions of class cleavage

in Italy and the destructive nature of the fascist response. I feel that Joes's underestimation of the negative side of fascism until he discusses events after 1936 stems from the abstraction of the model that so universalizes the concept in fuzzy notions of modernization that any sense of its real impact on Italy is lost.

Finally, I am amazed that he should ask how I presume to know what sources he used. Any reviewer must use the notes and bibliography. Professor Joes cleverly hides his non-English-language sources.

As for who is a professional pamphleteer, I leave it to readers of the book to determine whether its violent anticommunism does not fall into that category.

ALEXANDER DE GRAND
Roosevelt University

TO THE EDITOR:

Sally Marks gives the impression of "hardly knowing where to begin" in recounting the failings of *France's Rhineland Diplomacy* (AHR, 84 [1979]: 770-71). I scarcely know where to begin a rebuttal to so undisciplined an assault. No long book handling many sources and a complex subject is without minor errors; what makes the Marks "review" so malicious is that detection of alleged errors was apparently her sole purpose. And, for someone so intolerant of imperfections, she is remarkably imprecise. She has removed statements from context, ignored or distorted nuances, and employed exaggeration and outright falsification in what can only be termed a "hatchet job."

Many of Professor Marks's alleged "errors" are brazen inventions. (1) Never do I bluntly assert that Belgian minister Jaspar "favored Rhenish separatism." Rather, I stress the great nervousness of the Belgian government about illegal separatism, and I show the Theunis-Jaspar cabinet's actions in 1923 to be not "support for separatism" but wily use of a putsch in the Belgian zone to abort French separatist intrigues (pp. 196, 237-38, 256-57, 268, 311-16). (2) Never do I assert that Lloyd George preferred Poincaré to Briand. This is a false allegation based on my accurate statement that at *one* point the British cabinet aired the notion that, "if M. Poincaré returned to power, it might be possible to come to some more satisfactory settlement with him than with M. Briand" (p. 179). Earlier, Lloyd George was anxious to preserve Briand in power, as I show (p. 147). (3) In my book, according to Marks, "In 1923 Foster Dulles becomes an official American spokesman." My text, however, describes Dulles as "an unofficial envoy" (p. 288).

Other charges are so vague as to be unanswerable. (4) My interpretation, which is never described, is said to be "at variance with historical

data." Never is this demonstrated. (5) I am accused of "a multitude of mistakes." In fact, even Professor Marks's hand-picked few are dubious, wrong, or too trivial to support her grave charges. (6) It is claimed that "more reference to conference minutes" would correct my account of reparations, and (7) that more work in London would have "reduced the level of error regarding reparations, etc." Apparently, Marks is a devotee of the "what one clerk said to another" school of diplomatic history, for many of her criticisms stem from this bias. I, too, believe that diplomatic exchanges are often central to the course of international relations. But where I deem "conference minutes" *not* of primary import, I believe it my duty to spare the reader. My book was not designed to be another reparations chronicle. (8) Had Marks deigned to show how I "misrepresented" the December 1921 and September 1923 talks, perhaps I could respond to her objections.

(9) Professor Marks's most serious charge—that "evidence is distorted or suppressed"—is also a flimsy contrivance. In the Nothomb affair, Marks is apparently referring to a telegram cited on page 197 (here, as always, it is left to us to guess). Rechecking the document I see that I did inadvertently broaden a report of Jaspar's reluctance to say whether Belgian authorities would support the separatist Smeets in a lawsuit, to reluctance to take a position on Nothomb-style separatism in general. This error is seriously advanced by Marks as "suppression of evidence," even though the issue—and all Belgian matters of such keen interest to Marks—are peripheral to my main arguments. Furthermore, the datum she accuses me of "suppressing"—a repudiation of Nothomb's goals—actually supports my *true* characterization of Belgian policy as hostile to illegal separatism and any Rhenish schemes unsupported by Britain. (10) Marks's other instance of "distortion" is "an August 1923 Poincaré telegram." Assuming she means the one cited on page 300, her objection escapes me—the quotation in the text speaks for itself. In any case, such potshots are abuses of the reviewer's advantage over the reader. How are we supposed to know just what she is disputing and whether her complaints are justified? That such vagaries are meant to support accusations of extreme gravity seems most unprofessional.

The blanket charge that I "contradict and disprove myself" also reflects Professor Marks's overzealous search for ammunition. (11) A daft nonentity like Dorten *was* "suited to lead" a motley movement of nonentities. I describe his antics at length (pp. 46-47, 70-72, 122-28, 158-61, 195-99, 280-86, and most of chap. 9)—only the most inattentive reader could think that I sought to praise Dorten's leadership. (12) The charge of Briand's being "for and against" a Ruhr occupation is unsupported and ignores the fact that most French

politicians *were* ambiguous about such a grave, uncertain act. (13) French Rhenish policies *did* involve many competing ends and means that varied over time. French confusion and hesitancy is one of my main themes (pp. 8–14, 244–49, 259–69, 360–66). These are not instances of “contradictions” but of distortion or misunderstanding on the part of the reviewer. With more space, the list would continue.

A book review is supposed to be informative, especially if it is negative. Yet the reader knows no more about my book at the end of Professor Marks’s piece than at the beginning. She ignores the book’s interpretations and conclusions, many of which one would have expected her to endorse. Her “review” is not an attempt to dissuade readers from views she finds untenable; it is an attempt to prevent readers from discovering my views at all! One wonders why. Surely as a serious scholar working in her field, in which there is presumably room for others, I deserve the same courtesy and reflection that Marks would demand for herself.

WALTER A. MCDUGALL
University of California,
Berkeley

TO THE EDITOR:

Sally Marks’s review of Walter A. McDougall’s *France’s Rhineland Diplomacy, 1919–1924*, which appeared in the *AHR* last June, accused McDougall of gross incompetence as an historian, if not outright dishonesty (“evidence is distorted or suppressed”), and dismissed the book as a whole as virtually worthless.

When I read the review I was dismayed. I know the book fairly well, since the period it covers is my own field of specialization. I personally did not agree with McDougall’s entire interpretation. But it was obvious from the book that he is an honest, serious, and highly professional scholar. It was abundantly clear, moreover, that an interpretation at least does exist. But this the reviewer apparently refused to admit. Certainly the reader could get no sense from the review as to what the overall argument of the book in fact is.

McDougall, incidentally, is not a personal friend of mine. Nor do I bear any grudge against Marks. And I do not particularly want to comment here on Marks’s specific allegations in any detail. Of the few I was able to track down, some were in fact correct (the Alsatian bishops); some were clearly false (Dulles’s status); and some apparently were gross distortions of the author’s meaning (the Little Entente). But the accuracy of the reviewer’s specific criticisms is really secondary. The important issue is that the general claims about the worthlessness of the book and the author’s lack of scholarly skill and integrity are grossly unwarranted. I feel it is important not to let so unfair an attack go completely un-

answered—or answered only by its victim, whose comments many readers may attribute to hurt feelings and dismiss in advance as self-serving.

It is not that anything I write buried here in the “Communications” section months after the review was published can possibly undo the damage the review has caused. But a certain standard of elementary fairness and decency needs to be upheld by the profession; and if no one speaks out such a standard would not exist even in theory.

MARC TRACHTENBERG
University of Pennsylvania

TO THE EDITOR:

We, along with other younger colleagues who have expressed their dismay to us, must report our distress at the harsh tone of Sally Marks’s review of Walter A. McDougall’s book, *France’s Rhineland Diplomacy, 1914–1924*, in the June 1979 issue of the *American Historical Review*.

Certainly a reviewer should point out where more documentation might alter conclusions, and he or she may contest the thesis of a book. Indeed, one of us has already debated aspects of McDougall’s conceptualization of post-1918 French diplomacy. Nonetheless, the assurance conveyed by the review that when archival material was not cited it was not known and when cited it was used only cursorily and, likewise, the reviewer’s recurrent examples from Belgian diplomacy to contest McDougall’s arguments about French policy—arguments moreover, that are never summarized for the reader—seem a less than fruitful critical approach. Without arguing that all books deserve mercy because they take work, we would suggest that reviewers keep a tone of criticism that recognizes that a common scholarly effort unites them with the author.

GERALD FELDMAN
University of California,
Berkeley

CHARLES S. MAIER
Duke University

PROFESSOR MARKS REPLIES:

In response to Walter A. McDougall’s letter, I acknowledge that additional substantiation of my contentions would indeed have been desirable, but unfortunately I could not move the *AHR* beyond seven hundred and fifty words, and thus I was unable to pack more evidence into my review. The same problem arises with this reply. Of course it is true that all books contain occasional errors, but McDougall’s massive inaccuracy far exceeds acceptable professional limits and destroys his credibility. This is particularly unfortunate because the topic of France’s Rhineland diplomacy is important, but

McDougall's book is much too flawed to contribute to our understanding of it. In addressing that situation, I focused primarily upon fundamental errors that seriously undermine the structure of the work. McDougall's rebuttal largely ignores these substantive matters.

My review's two sentences on Belgium seem reasonable, given the vital importance of Belgian policy both in their zone and on the High Commission to the fate of French-supported separatism. McDougall declares that Belgian policy initially was "military protection for Deckers' north Rhenish state, the key to Belgian strategy. The Belgian military, cabinet, and private intrigues all hoped to fashion a federalized autonomous Rhineland in which British and Belgian influence would balance the French" (p. 311). While two cabinet ministers and some junior officers supported Nothomb's intrigues—which did not contemplate British involvement—government policy was hostile from the outset. As Professor McDougall cites file 240, Jaspar Papers, he must have seen Theunis's sarcastic reaction, Nothomb's complaints of government hostility, and Jaspar's immediate declaration, "Les sottises dont nous avons tant pâti au moment de l'armistice vont recommencer." Jaspar added that he knew nothing yet about the separatist outbreak, that Nothomb's group had no mandate and had not consulted him, and that the Belgian government did not support its views. The resultant prompt collapse of the first 1923 separatist outbreak in the Belgian zone largely destroyed the credibility of subsequent French-backed separatism, but McDougall ignores this circumstance. Similarly, his depiction of the 1919 negotiation of the Rhineland Agreement as "refreshingly free of interallied conflict" (p. 83) deviates from both the documents and Keith Nelson's careful enunciation of them in *Victors Divided* (1975), which McDougall cites elsewhere in his work. Such disregard of data occurs frequently, although space does not permit additional elaboration of distortions previously noted on pages 197, 300, and elsewhere.

On reparations, Briand's statements to Lloyd George hardly constitute "what one clerk said to another." I complained, moreover, not of failure to recite minutes but of fundamental confusions, which I enumerated. Further, Briand did not accept the August financial division in December 1921 (p. 177); Professor McDougall confuses both the December scheme and its significance, which perhaps is why he ignores the confrontations at Cannes. Baldwin's brief meeting with Poincaré in September 1923 while passing through Paris and the exchange of vague speeches represented neither "a political event of the first magnitude" nor British concessions (p. 295).

I acknowledge with apology that brevity caused

imprecision concerning Dulles, but surely terming him an "unofficial envoy of the United States" and citing state department policy (pp. 288–89) implies inaccurately that he represented more than himself. Similarly, in the midst of two paragraphs (pp. 178–79) otherwise devoted to Lloyd George's attitudes at Cannes, Professor McDougall says not what he quotes above but rather that "the British cabinet expected the fall of " Briand. He adds, "Poincaré . . . might be easier to deal with." In context, this suggests Lloyd George's views, not poor paragraph construction.

I would gladly have discussed McDougall's thesis had I discerned one. As I indicated by example and quotation, there is so much muddle and contradiction in his book that one cannot discover where he stands. I did not lightly undertake the responsibility of writing a critical review but concluded that, if I did not, McDougall's fluent misinformation might be accepted by nonspecialists as "historical truth." Finally, I regret that my effort to warn readers of massive confusion strikes McDougall as unprofessional.

In further reply to Gerald Feldman, Charles S. Maier, and Marc Trachtenberg, I would remark that, if in fact Professor McDougall has read files that he neither lists nor cites, his suppression of evidence would exceed what I have suggested. Moreover, in exercising my scholarly judgment, fulfilling my obligation to readers of this journal, and writing the review that—alas—clearly had to be written, I certainly had no desire to become embroiled in the politics of the profession: that my rather euphemistic description of McDougall's scholarly deficiencies should generate such responses is indeed depressing.

Perhaps those who know Professor McDougall well have been able to ascertain his views through discussion with him. If so, it is a pity that none of them chose on this occasion to inform readers of his argument and its contribution. I, however, could only be guided by McDougall's book, which, as I indicated, sheds little light on his views because he frequently contradicts himself. Even if any consistent argument could be discerned, a further question would arise concerning the validity of an interpretation based upon such numerous inaccuracies. Certainly, greater fidelity to the documents and much more careful handling of evidence would have been required before this book could, in my judgment, qualify as a professional work of history. After all, a commitment to seeking the truth and investment of the necessary time and care in its pursuit are fundamental to scholarship. Without these, the historical fraternity could hardly claim to possess the standards requisite to a profession.

SALLY MARKS
Rhode Island College

Recent Deaths

At his retirement dinner in 1972 there gathered a wide assortment of people to congratulate RUSSELL LEON CALDWELL for his twenty-seven years of service to the University of Southern California and the Los Angeles community that surrounds the university. No one then realized that he would teach the history of the press in the School of Journalism for an additional five years and die on May 23, 1979, still an active member of the teaching faculty. Russell had the reputation through many years of being an inspiring, energetic, and witty teacher, and many of those who honored him at the retirement dinner were doubtlessly former students of the 1950s and 1960s who remembered his lectures in Bovard Auditorium before audiences of fifteen hundred people.

Russell Caldwell published amazingly little for a man who liked manuscripts, books, and literature. His only piece of writing, *American Freedom: A History*, was published in 1962, but earlier, before he began his professional career as a historian, he had published some articles on oratory and debating in the *Quarterly Journal of Speech*. Russell gave his energy instead to dissertations and theses, to lectures and commission reports, to university service in projects involving faculty government, AAUP services, and grievance committees, and to plans for the employment of retired faculty to improve the college.

Russell was born in the steel town of Farrell, Pennsylvania in 1904, attended high school there, and went to Hiram College in Ohio where he learned the art of debating and oratory, studied theology, and became interested in history. His early career was devoted to teaching in high schools in Pennsylvania and Ohio, where he won a reputation as a gifted speech coach who had regional and national champions. In 1933 he completed a masters degree from the University of Southern California in medieval history, but he was attracted to American history and returned in 1945 to the university to study U.S. constitutional history under Frank H. Garver. Before he completed the doctorate in 1948, he had been hired by the university to chair its Department of General Studies in Ameri-

can Civilization and awarded the rank of assistant professor. His facility in handling large student numbers brought advancement to associate professor in 1951 and professor in 1963. In those years, he had also taught as a summer faculty member at the Massachusetts Institute of Technology, the University of Michigan, UCLA, and Ohio State University.

A man of such energy is often difficult to characterize, but a sample of activities will illustrate his vitality. He wrote argumentative pieces for the *Daily Trojan*, often on teaching, sometimes on university policy, and many times on national issues; he sponsored petitions to the legislature against capital punishment; he sat on slum clearance boards and revealed the politics of the real estate lobbies; he spoke frequently in the university senate on tenure and salary matters.

His reputation is marked well by the careers of twenty or more successful doctoral candidates, fifty or more masters students, and the thousands of former students in the professions and businesses who benefited by his letters of recommendation. In 1972, one vice president of the university, in trying to capture Russell Caldwell's spirit, praised him as the most important member of the faculty in shaping the lives of undergraduates in the university. The audience rose, clapping, and toasted him for his devotion and loyalty to his students and the profession.

JOHN A. SCHUTZ
University of Southern California

JOHN F. GLASER, Professor of History at Ripon College, was killed in an automobile accident September 29, 1979. Born in Hamilton, Ohio, October 27, 1920, he attended Washington and Jefferson College, receiving his A.B. degree summa cum laude in 1941. He did his graduate work at Harvard University, earning an M.A. in history in 1942 and, after four years of military service (1942-46), a Ph.D. in 1949. He wrote his dissertation under the direction of David Owen. He taught at Washington Square

College of New York University from 1948 to 1954 but was on leave during 1952-53 to study in England on an American Council of Learned Societies fellowship. In 1954 he joined the faculty of Ripon College, rose quickly to the rank of full professor, and served as chairman of the history department from 1962 to 1969 and again in 1975-76.

In 1958 his article "English Nonconformity and the Decline of Liberalism" appeared in the *American Historical Review*; this same article was subsequently republished in two "problem books" dealing with English liberalism. A second article on "Parnell's Fall and the Nonconformist Conscience" appeared in *Irish Historical Studies* in 1960. He contributed more than fifty book reviews to the leading journals in his field.

Glaser was a charter member of the Conference on British Studies, was a member of its Executive Committee, and served as president of the Midwest Conference on British Studies from 1972 to 1974. He was a member of Phi Beta Kappa. Although his principal research interests were focused on Victorian England, he considered himself a humanist in the broadest sense of the word. His courses in European intellectual and cultural history received more and more of his attention in later years. Ideally suited to the academic environment of a small liberal arts college, he is remembered by two generations of students as a brilliant and witty lecturer and a stimulating if demanding instructor. His many friends and colleagues on both sides of the Atlantic will remember his warm hospitality and his absolute intellectual integrity.

GEORGE H. MILLER
Ripon College

MARGARET HASTINGS, Professor Emeritus of History at Douglass College, Rutgers University, died in an automobile accident on October 20, 1979, while on a visit to England. This is very sad news for many reasons but mostly because she had recently come through a period of poor health, and she and all of her legion of friends were looking forward to her enjoying a long, happy, and productive retirement.

Margaret Hastings was born in Springfield, Massachusetts, on May 23, 1910. She graduated from Mount Holyoke College in 1931, received an M.A. in English medieval history there in 1932 and a Ph.D. degree from Bryn Mawr in 1939, and supported herself in the meantime by teaching school. She continued to do this after 1939. Then, after a brief stint as a research analyst with the army late in the war, she joined the Douglass faculty as a lecturer in history in 1946, rising to the rank of full professor in 1959. She retired in 1975. She taught all sorts of courses during her years here, and all of them with the kind of perceptiveness and under-

stated wit that were the hallmarks of her conversation. As a teacher she was very good; as a scholar she was great. Her field was late medieval English law, a tangled thicket into which very few have the courage to venture. The records are voluminous, contradictory, and physically very awkward to handle; the language is often imprecise, and the handwriting ranges from difficult to unreadable. She ventured in and emerged with a book that was published in 1947 and became an instant classic: *The Court of Common Pleas in 15th Century England*, described by a British reviewer thirty years later, in 1977, as "one of the best books ever written on medieval England." She was not a rapid worker, and her bibliography is not a long one; but all her published work is first rate. Not all of it is narrowly specialized; her last book was an interpretive essay, *Medieval European Society, 1000-1450*, which, since it appeared in 1971, has been in regular use in Western civilization courses both here at Douglass and at many other colleges and universities. Her reputation among her professional colleagues was immense. English medievalists are a notably crotchety and contentious lot; but there was no dispute among them as to the quality of her scholarship. At various times in her career she was a Guggenheim fellow and a Fulbright fellow, and when the Helen Maud Cam visiting fellowship was established at Girton College, Cambridge, in honor of the great British medievalist, Margaret was its first recipient, a remarkable distinction for a colonial. It is pleasant to report that she was not without honor in her own backyard: she was the first Douglass College faculty member to receive a Lindback award for distinguished research, and in 1976 her alma mater, Mount Holyoke, gave her an honorary degree.

Margaret Hastings was a great scholar, with an enormously keen and perceptive mind—and, because she was modest, she never really took in how awfully good she really was. But she was much more than a great scholar. She was a wonderful colleague—who else would intervene in a tense personnel meeting with the suggestion that the rather erratic husband of one of her junior colleagues should be arrested so that that colleague could get on with her work? And she was a wonderful human being—warm and witty and wise, serious and funny by turns, and always worth hearing, whether the subject was hiking or Dickens, canoeing or Picasso, or medieval English law. The world, the profession, and all of her friends are the poorer for her passing.

MAURICE LEE, JR.
Douglass College,
Rutgers University

ALBERT HYMA, Emeritus Professor of History at the University of Michigan, died September 22, 1978,

in Marion, Ohio, at the age of 85. Throughout most of his life, he had been in vigorous health, until weakened after having abdominal surgeries during the Christmas holiday season in 1977 and in the spring of 1978. His death was caused by heart failure during surgery to replace a shattered hip.

Professor Hyma was born in Groningen in the northern Netherlands on March 18, 1893, and the years of his early childhood and youth were spent in Leeuwarden, to which his parents moved in 1896. When Albert was seventeen years of age, the family immigrated to America, where he attended Calvin College in Grand Rapids, Michigan, in 1913-14, and then the University of Michigan, at which he earned the B.A. in 1915, M.A. in German in 1916, and Ph.D. in history in 1922. During the school year 1916-17 he taught German at Knox College, in Galesburg, Illinois.

His career in history teaching began at the University of North Dakota, from 1922 to 24. In 1924 he returned to the University of Michigan, and there served until his retirement in 1962. During his tenure at Michigan, he also served as a guest professor at the University of Redlands during the school year 1944-45 and taught summer courses at various institutions, including the University of Chicago (1928), Ohio State University (1930), and the University of Colorado (1941).

Hyma's scholarly activity took him back to Europe as early as 1919-21, sponsored by the Algemeen Nederlandsch Verbond; and in 1928-29 he was in Europe again, as a Guggenheim Fellow. On one of several further trips abroad, he was knighted in 1936 by Queen Wilhelmina of the Netherlands for his scholarly work done in the field of Dutch history. His other scholarly honors and distinctions include election to fellowship in the Royal Historical Society in 1926, winning of the Henry Russell award at the University of Michigan in 1927, and election to fellowship in the Maatschappij der Nederlandsche Letterkunde te Leiden in 1957. He was also one of the founding editors of the *Journal of Modern History* in 1929, and he served as editor for the medieval and Reformation history areas in the *Twentieth Century Encyclopedia of Religious Knowledge*, published in 1955.

Near the beginning of his scholarly career Hyma gained almost immediate recognition as a Renaissance-Reformation specialist by his classic work, *The Christian Renaissance: A History of the "Devotio Moderna"* (1924), and he enhanced his reputation in this field by numerous further publications, including the textbook *Renaissance to Reformation* (1951, 1955) and various biographical studies on Erasmus, Luther, and Calvin. His more than forty published books and several hundred articles and book reviews reveal, however, interests and expertise beyond the Renaissance-Reformation field. With

Arthur E. R. Boak and Preston Slosson he co-authored the standard history textbook *The Growth of European Civilization*, two volumes, first published in 1936 and appearing in later editions in revised form as *The Growth of Western Civilization*; and he authored or co-authored further general history textbooks in later years. As an expert in the history of the Dutch in the Far East, he produced *A History of the Dutch in the Far East* (1953), as well as *An Outline of the Growth of Far Eastern Civilizations* (1946). His interest in the Dutch in America and in early Michigan history led him to produce his *Albertus C. Van Raalte and His Dutch Settlements in the United States* (1947) and to co-author with Frank B. Woodford the book *Gabrie! Richard: Frontier Ambassador* (1958). His breadth of knowledge and expertise was also evidenced in the numerous book reviews that he provided for both American and European publications, treating a wide array of works with titles in seven different languages (some fifty reviews have appeared in the *American Historical Review*).

At the time of Hyma's retirement from the University of Michigan, he was honored not only by the university but also by some fifteen of his doctoral graduates and other friends who presented him with a festschrift volume entitled *The Dawn of Modern Civilization: Studies in Renaissance, Reformation and Other Topics Presented to Honor Albert Hyma* (1962, 2d ed., 1964). His formal retirement did not, however, mean inactivity for him. In addition to further writing and general lecturing, he accepted semester- or year-long teaching appointments at such institutions as Aquinas College in Grand Rapids, Michigan; Western Kentucky University in Bowling Green, Kentucky; and Christian Heritage College in San Diego, California.

The year 1927 had been a most important year in Hyma's life, for in that year he married Vera Alberta Nodine, a marriage that resulted in a happy home with three children and in a companionship of mutual interest in his historical studies. The year 1927 was indeed doubly significant for him, for it was also in that year that he was naturalized a United States citizen. Shortly after his retirement in 1962, the Hymas moved to Holland, Michigan, where they made their home until 1976. Since then, they resided near their children, first in Brigham City, Utah; then in Schenectady, New York; and lastly in Marion, Ohio. After Professor Hyma's death, Mrs. Hyma returned to Holland, Michigan, in November 1978.

Hyma achieved recognition as both a church historian and a secular historian, a dual role in which he displayed a remarkable capacity for combining divergent elements—the sympathetic understanding of one who lives genuinely and experientially within the framework of the church and the scholarly technique of one who is absolutely and thor-

oughly devoted to the strictest standards of the historical discipline. A church member reared in the Reformed tradition, he was committed to the Christian way of life. A disciplined historian, he was dedicated to the search for historical truth. The two facets of his experience never clashed. Rather, he combined them in beautiful harmony—a harmony that united the warmth of one who feels with the authority of one who knows.

But Professor Hyma's true greatness reached beyond the realm of scholarship. His personal magnetism, his genuineness of character, his sincere and friendly demeanor—these were among the noble qualities that endeared him to those of us who had the high privilege of personal association with him. He was a great scholar and a great teacher, but he was also a great Christian gentleman and a great friend.

KENNETH A. STRAND
Andrews University

BERNARD MAYO, Emeritus Professor of History at the University of Virginia, died on August 20, 1979, in Charlottesville, Virginia. Born at Lewiston, Maine, on February 13, 1902, Mr. Mayo attended the University of Maine, received his A.B. degree in 1924 and A.M. degree in 1925 from George Washington University, and earned his Ph.D. at Johns Hopkins University in 1931. He taught at the National University from 1926 to 1937 (it merged with George Washington University in 1954) and at neighboring Georgetown University from 1937 to 1940. In the fall of 1940 he moved to the University of Virginia where he taught until his retirement in 1972. At that time his appreciative former students gathered and presented him a festschrift, *America: The Middle Period: Essays in Honor of Bernard Mayo* (1973). Mr. Mayo served as visiting professor at Harvard in 1946–47 and taught summer school at Columbia (1946) and the University of Missouri (1961). In 1958 he gave the inaugural Eugenia Dorothy Blount Lamar Memorial Lectures at Mercer College, published as the delightful *Myths & Men: Patrick Henry, George Washington, Thomas Jefferson* (1959). More than twenty years before, Mr. Mayo had begun his scholarly career with his distinguished *Henry Clay: Spokesman of the New West* (1937, reprinted 1966). He edited for the American Historical Association *Instructions to the British Ministers to the United States, 1791–1812* (1941) and followed that with *Jefferson Himself: The Personal Narrative of a Many-Sided American* (1942). Like Jefferson himself, Mr. Mayo's writing demonstrated "a particular felicity of expression." Active in his profession, Mr. Mayo was a member of the American Historical Association, the Organization of American Historians, the Society of

American Historians, the Southern Historical Association (member of the executive council, 1942–44, and of the editorial board, 1944–47), the Virginia Social Science Association (executive council, 1954–57, vice-president, 1959–60, president, 1960–61), and the Albermarle County Historical Society.

For two generations of students Mr. Mayo's superb teaching was his most memorable achievement. With unfailing charm and wit he taught them how to think more clearly and write more gracefully. He had that special ability to inspire students to do better than their best. One of his most characteristic accomplishments was the founding of the History Club at the University of Virginia in 1948. For many years the home of Professor and Mrs. Mayo was the center of things at the university for townspeople, visiting dignitaries, faculty members, and, most of all, for hundreds of students. Mr. Mayo was a man of liberal sentiments and cultivated tastes. He loved his music, his books, and his garden. He could light up a room with his presence. It was altogether appropriate that at the University of Virginia's fall 1976 convocation he gave the feature bicentennial address, "Another Peppercorn for Mr. Jefferson" (published in 1977). Again like Jefferson, Mr. Mayo was a man for all seasons who enriched immeasurably the University of Virginia and all whose lives he touched.

Mr. Mayo is survived by his wife, Margaret McClure Macleod, two children, Dr. Malcolm Macleod of Sacramento and Margaret Macleod Kern of Chicago, and several grandchildren.

W. W. ABBOT
University of Virginia
JOHN B. BOLES
Tulane University

After a long fight with cancer, PHILIP P. POIRIER, Professor of History at the Ohio State University, died at Columbus on February 28, 1979, at the comparatively early age of fifty-eight. He was one of a brilliant group of Harvard graduate students who worked in the late 1940s and early 1950s with David Owen and have done so much to reshape the study of Britain in the United States since the Second World War, helping to give it dramatic new dimensions. Poirier belonged to a new generation of academics who were less traditionally minded and less traditional in background than many of those who had entered academic life previously, particularly in the area of British studies. He was born and grew up in Gloucester, Massachusetts. He graduated from Boston University in 1942 (and at the university's Centennial Celebration he was cited as one of its 100 most distinguished alumni). Then he went off to war, served in the army in both France

and Germany, and was awarded the Bronze Star. At Harvard he received his M.A. in 1948 and his Ph.D. in 1953.

Poirier made his special area of concern the growth of the Labour Party: the way it attempted to transform British society at the same time that it drew deeply from lasting British traditions, so that it is the despair of the more doctrinaire (and at times of the more adventuresome) but also the hope of those who believe that the left can have its roots and traditions. In private conversations and in papers and comments given in London, New York, Washington, and elsewhere at various conferences and meetings, Poirier was tough-minded and thorough, had a total command of the sources, most particularly those to be found in private papers, and would not tolerate sloppy thinking in himself or in others. He was an example of how scholarship should be conducted—he was unfailingly polite and unfailingly insistent on getting things right.

His major work, *The Advent of the British Labour Party* (1958), showed all his skills at their most impressive. Two British scholars and he were the first to take advantage of newly opened papers that revealed the arrangements made by the Labour Party with the Liberal Party not to fight one another in constituencies in so far as that was possible. It was a plan that allowed the Labour Party to return a significant number of M.P.s in the General Election of 1906. Poirier's study, dealing with the six crucial years from 1900 to 1906, is extraordinary in its range of research and is written with elegance and force. It is a model monograph. Dealing with an important subject, it changed the course of thinking about a crucial period. Poirier created a book that has lasted and will continue to be read by all students of the Labour Party and Britain. Its position was recognized in 1961 when it received the first triennial prize of the Conference on British Studies. He continued, through numerous reviews and talks, to elucidate aspects of modern Britain and provide directions, suggestions, and insights for his fellow scholars in both the United States and Britain. In 1974 he wrote an important introduction for a reissue of L. T. Hobhouse's *The Labour Movement*, and at the time of his death he had finished the research and was about to embark on the writing of a study of "The Impact of Conscription in Great Britain during World War I." It is a great loss that we are deprived of this study, which would have combined his acute sense of the politics of the period with an awareness of the social dimensions of conscription. (There are hopes that another scholar will be able to bring his work to completion.) Poirier was a considerable figure in the international world of scholarship and his pungent and powerful contributions will be sorely missed.

Even as his absence will be much regretted in the world of British studies, and by his many friends in

the profession, so too will he be missed at Ohio State, where he spent his entire teaching career, other than a brief experience as an instructor at Williams College in 1952. That same year he came to Ohio State as an instructor. He moved up the ranks, becoming a full professor in 1964. There too he met his wife, Carole Rogel Poirier. He taught a full range of courses, primarily on modern Britain at the graduate and undergraduate levels, but in fact the last course he taught, in the autumn of 1978, was on nineteenth-century European intellectual history, one of his regular offerings, suggesting the breadth of his mind and interests. He was deeply involved with graduate education, and while he was on various committees for his department and at the university, probably his most notable service was in the reform of the history graduate program. As a colleague has written, "His own Seminar for M.A. and Ph.D. students was a model of its kind—well organized, exacting, demanding, yet not excessively severe. He was renowned for strengthening the graduate program and for upholding and setting standards." He himself firmly believed in the need to study manuscript collections in England, and during his career received support from the American Council of Learned Societies, the American Philosophical Society, and the Guggenheim Foundation in order to do so. (His position was also recognized in his election as a Fellow of the Royal Historical Society.) It is particularly appropriate that the memorial to him should be a fund designed to enable graduate students to do research in England. (Contributions to it should be sent to Professor Gary Reichard, Chairman, Department of History, 230 West 17th Avenue, The Ohio State University, Columbus, Ohio 43210. Checks should be made out to the Ohio State Development Fund and marked the Philip Poirier Memorial Fund.)

British studies, most particularly the investigation of the period of the early part of this century, when so much of what we now recognize as the modern world came into being, have suffered a great loss in the death of Philip Poirier.

PETER STANSKY
Stanford University

MORRIS FENTON TAYLOR, Professor of History and Head of the Social Studies Division at Trinidad State Junior College in Colorado, died of cancer on January 29, 1979. He was sixty-three years old at the time of his passing.

Born in Mount Morris, New York on October 21, 1915, Morris Taylor spent his youth in Warsaw, New York. In 1939, he received his B.A. from the University of Colorado, and in 1940 he obtained his M.A. from Cornell University. In 1941, he did advanced graduate work at Cornell, and in September

of 1941, he joined the faculty at Trinidad State Junior College. Except for serving in the United States Marine Corps from 1942 to 1943, Morris Taylor remained a member of the faculty at Trinidad State for nearly forty years. In 1969, the University of Colorado awarded an honorary Doctorate of Humane Letters to him.

Elected to a three-year term in 1974 to the Executive Council of the Western History Association, Morris Taylor's other officerships included President of the Colorado Federation of Teachers, President of the Colorado Archaeological Society, President and then life-time Honorary President of the Trinidad Historical Society, President of the Colorado History Group, President of the Community Concert Association, Vice-President of the Colorado Conference of the American Association of University Professors, and Regional Vice-President of the State Historical Society of Colorado. His professional affiliations included membership in the Organization of American Historians, the Western History Association, the State Historical Society of Colorado, the New Mexico Historical Society, the Great Plains Historical Association, and the Trinidad Historical Society.

Morris Taylor won numerous awards and honors, both while living and posthumously. He participated in the Fulbright Teacher Exchange and taught in Kent, England in 1960 and received a fellowship to study at the Huntington Library in 1976. The American Association for State and Local History presented him an award in 1967 for his book entitled *Trinidad, Colorado Territory*, and five years later the same body awarded him a commendation for his research and publications on topics concerning the Southwest. He received the Historical Society of New Mexico Certificate in recognition of his writings on New Mexico history in 1971, and he won the LeRoy R. Hafen Award from the State Historical Society of Colorado for the best article published in *The Colorado Magazine* in 1974. Memorial awards included the establishment of the Morris F. Taylor Memorial Scholarship to Trinidad State Junior College, the dedication of two issues of *The Colorado Magazine* to his memory, and the establishment of a Morris F. Taylor Library by the Trinidad Historical Society in the Trinidad Museum Complex.

As both a teacher and a scholar Morris Taylor excelled. Although teaching four or five courses per term, during his career he still published six books, two of which being *First Mail West*, published by the University of New Mexico Press in 1971, and *O. P. McMains and the Maxwell Land Grant Conflict*, published by the University of Arizona Press in 1979. His numerous articles appeared in several historical journals.

Professor Morris F. Taylor will certainly be

missed. Fortunately, his writings and the several memorial awards he received will remind us of his contributions to the field of history.

RAYMOND WILSON
Fort Hays State University

T. HARRY WILLIAMS, Boyd Professor of History at Louisiana State University, died on July 6, 1979, in Baton Rouge, Louisiana. He was admired and held in deep affection by a multitude of students, former students, colleagues, and other acquaintances. This year was his retirement year; he was honored in appropriate ceremonies at his university only about two months before his death.

Professor Williams was born May 19, 1909, in Vinegar Hill, Illinois. He grew up and received his education in Wisconsin, where he gained a bachelor's degree from Platteville State Teachers College in 1931 and a master's degree from the University of Wisconsin in 1932 and the doctorate in 1937. He taught in the extension division of the University of Wisconsin from 1936 to 1938 and at the University of Omaha from 1938 to 1941, the year he came to Louisiana State University. He became a Boyd Professor in 1953. He was a visiting professor at various universities; he was the Harmsworth Professor of American History at Oxford University during 1966-67. He was a member of the American Historical Association throughout most of his career. He was also a member of the Southern Historical Association, serving as president in 1958-59, and of the Organization of American Historians, serving as president in 1972-73. He received honorary degrees from five colleges and universities.

Professor Williams was a highly productive and distinguished scholar. He wrote or edited some 20 books, and he wrote dozens of articles and book reviews. He directed the work of 36 students who earned the Ph.D. degree and 57 who earned the M.A. degree. The subject that engaged him over the longest period of his professional life was the American Civil War; his most impressive books in this field were *Lincoln and His Generals* (1952), which was a Book-of-the-Month Club selection, and *P. G. T. Beauregard: Napoleon in Gray* (1955), which some reviewers believed was worthy of a Pulitzer Prize. At the prime of his career he turned his attention to the twentieth century. In 1969 he brought out *Huey Long*, which won both the Pulitzer Prize and the National Book Award. At the time of his death he was at work on a biography of Lyndon Baines Johnson.

Professor Williams was as outstanding in the classroom as at his desk. He was essentially an old-fashioned teacher. He dealt brilliantly with political, economic, and social causes and effects, but his deepest interest was in people as individuals. He delivered sharp analyses and concise syntheses of ma-

for historical developments, but his favorite method of instruction was by narration and anecdote. With an encyclopedic command of the facts, an unexcelled vividness of expression, and an almost matchless sense of both pathos and humor, he made his courses into high theater.

He was no mere chronicler of events. His writing and teaching were an elaboration of his reflections upon the lessons of the past. He was fascinated by strength of personality and by the exercise of power in the affairs of society. He believed, up to a point, in the Great Man theory of history: that social movements do not simply occur but are the result of personal inspiration and leadership. This conviction pervades his works on Lincoln and Long, both of whom he admired in spite of the obvious great differences between them, and the belief shows clearly throughout his other writings, as it did in his lectures. He was captivated also by the colorful personality, whether effectual or ineffectual. His biography of Confederate General Beauregard, a resourceful but visionary and erratic Louisiana Creole for whom he had a genuine fondness, is a study in the exotic and the romantic. Beauregard, he wrote, "had more glamor and drama in his Gallic-American personality than any three of his Anglo Saxon colleagues in gray rolled into one." In a more casual and more pungent idiom Professor Williams said that Beauregard in the midst of the Lees, Johnstons, and Jacksons of the Confederacy was like a serving of shrimp jambalaya surrounded by a mess of turnip greens.

Professor Williams was as interesting outside the classroom or study as he was inside them. After moving to Louisiana he quickly adopted the style of the bayou state. He married an accomplished and gracious Louisiana lady, Estelle Skolfield, and he became an inseparable part of the local scene. Physically spare, he more than compensated for his lack of robustness with the force of his intellect and the readiness of his wit. Activated by an inexhaustible nervous energy, he lived at the height of mental and emotional tension. A master of the sharp comment and the instant retort, he enjoyed nothing more than a verbal tilt with a colleague or graduate student. Yet he was a warm and generous man. His home was the center of frequent social gatherings where faculty and graduate students mingled freely and exchanged opinions and gibes with startling candor. His death put out one of the brightest lights of the academic community.

The subject of Professor Williams's most celebrated book, Huey P. Long, once said of himself that he was *sui generis*—the only one of his kind. This is perhaps a fitting characterization of T. Harry Williams.

CHARLES P. ROLAND
University of Kentucky

EDWARD E. YOUNGER, Alumni Professor of History at the University of Virginia, counselor and friend to hundreds of graduate students, died in Charlottesville, June 23, 1979. He had retired on May 31 after more than thirty years of dedicated service to the Corcoran Department of History, the university, and the profession.

A native of Arkansas, Edward Younger began a distinguished teaching career in a one-room school at the age of sixteen. While teaching full time, he earned a bachelor's degree from Arkansas State College. Leaving a high school principalship for the uncertainties of graduate study in the depression years, he took the M.A. in history from Oklahoma A & M College in 1938 and the Ph.D. from George Washington University in 1942. After completing a tour of duty in the navy in World War II, he was appointed assistant professor of history at the University of Virginia.

His contributions to the university over the next three decades were manifold. He developed courses in American diplomatic history, the Gilded Age, and twentieth-century United States, and, despite the increasing specialization of the profession, continued to teach them for more than twenty years. As chairman of the Corcoran Department of History from 1962 to 1966, he presided over a program of expansion that nearly doubled the size of the faculty and attracted a number of distinguished historians to the university. From 1966 to 1969, he served as dean of the Graduate School.

In an era when scholarship flourished as never before and publication was the standard means of professional recognition and advancement, Edward Younger's scholarly achievements seem modest. His biography of John A. Kasson, Iowa politician and diplomat of the Gilded Age, nevertheless remains a model study of a second-level political figure, and his many essays and reviews are distinguished by precision of analysis and clarity of style.

It was characteristic of the man that he sacrificed his own scholarly work to the service of others. Like many of his generation, he was deeply sensitive to the new role being assumed by the United States in the world, and he sought to employ his academic expertise to further international cooperation. As Fulbright lecturer at Allahabad University in India in 1957-58, he worked diligently and patiently to explain American history and foreign policy to frequently skeptical audiences. He returned to the United States with a keen interest in Indian history and culture and a determination to increase American knowledge of and friendship for the world's largest democracy. He lectured extensively on India in Virginia and across the nation and was instrumental in establishing a South Asian studies program at the University of Virginia.

While encouraging the study of different cultures,

he also maintained a special scholarly interest in his adopted state. Throughout his career, particularly in the last ten years, he enthusiastically promoted the study of recent Virginia history, an area largely neglected by a people steeped in the antebellum past. He urged graduate students to work on post-Civil War Virginia and played a major part in procuring the manuscripts that served as their raw materials. After completing his term as graduate dean in 1969, he launched, in collaboration with his students, two ambitious projects now nearing completion, a collection of biographical essays on the post-1865 governors of Virginia and a history of Virginia after the Civil War. For his pioneering work in this field, he was named an Honorary Fellow of the Virginia Historical Society in 1973.

Edward Younger will be most affectionately remembered as a graduate teacher. By the time of his retirement, he had directed 65 dissertations and had supervised the work of 140 M.A. candidates. The numbers are staggering, but there was never the slightest resemblance to a "Ph.D. mill." A demanding taskmaster, he oversaw each project with painstaking care and insisted on the highest standards of scholarship. He gave generously of himself to all of his students. Even during the time he was department chairman and graduate dean, he was available day and night for assistance, counsel, and, when necessary, ego-stroking. Considering the rigors

of his early career, it must have been difficult for him to sympathize with those of us who were at least slightly spoiled by an age of unparalleled prosperity and unique academic opportunity. Yet he somehow understood. He kept countless people in school when they were inclined to drop out. He drove through to completion many dissertations that might have languished. His interest continued after students departed from the university. If they were dissatisfied with positions, he helped them find new ones. When they encountered professional frustration he listened patiently and with that knowing twinkle in his eye put things in perspective. He took enormous pride in their accomplishments, always in a completely selfless manner. Those who returned for visits found the door open, as before, and the same warm hospitality and rapt attention.

In the words of colleague Martin Havran, Ed Younger, above everything else, was an "intensely humane man." Quiet and unassuming, always reserved and dignified, he radiated warmth and kindness and possessed a sly sense of humor that he employed with a light and unerring touch. He was a master at getting the best from those who worked for and with him, seemingly without effort. He will be deeply missed and long remembered by those who knew and loved him.

GEORGE C. HERRING

University of Kentucky

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book-length study of Irish landlordism from the 1840s to the 1920s. His other interests include popular culture in Victorian England.

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“A Place on the Ballot”: Fusion Politics and Antifusion Laws

PETER H. ARGERSINGER

ONLY IN RECENT YEARS have historians seriously investigated the institutional framework of the American electoral system and begun to examine the political effects of ballot forms, voting systems, and suffrage requirements. In particular, some scholars have sought to explain the dramatic changes in political behavior that occurred around the turn of this century as “unintended consequences” of reforms in the structural properties of the electoral system rather than as a reflection of any larger development. While illuminating the political results of such institutional changes, these scholars have largely ignored the political context within which the changes evolved. Thus, they have regarded those structural modifications as essentially apolitical or nonpartisan and have sharply rejected any view that change stemmed from an “antidemocratic conspiracy” to control the political system.¹ Yet, at least one little-known development in the

A preliminary version of this article was presented at the annual meeting of the American Historical Association in Dallas, December 28–30, 1977. I wish to thank the principal commentator at that time, Howard W. Allen, for his helpful criticism. I also wish to express my appreciation to Jo Ann E. Argersinger for advice and assistance.

¹ For present purposes, this “legal-institutionalist” school is best approached in terms of the reaction to Walter Dean Burnham’s “The Changing Shape of the American Political Universe,” *American Political Science Review*, 59 (1965): 7–28. Burnham argued that sharp declines in turnout and increases in split-ticket voting and other indexes of partisan volatility and voter marginality reflected the establishment of corporate political hegemony in the realignment of 1896 and a consequent breakdown in party organization and competition coupled with a rise in voter alienation. For major rejoinders, see Jerrold G. Rusk, “The Effect of the Australian Ballot Reform on Split Ticket Voting, 1876–1908,” *ibid.*, 64 (1970): 1220–38; and Philip E. Converse, “Change in the American Electorate,” in Angus Campbell and Philip E. Converse, eds., *The Human Meaning of Social Change* (New York, 1972), 263–337. For the continuing controversy, see Burnham, *Critical Elections and the Main-springs of American Politics* (New York, 1970); Burnham and Rusk, letters in *American Political Science Review*, 65 (1971): 1149–57; Burnham, “Theory and Voting Research: Some Reflections on Converse’s ‘Change in the American Electorate,’” *ibid.*, 68 (1974): 1002–23; Converse, “Comment on Burnham’s ‘Theory and Voting Research,’” *ibid.*, 1024–27; Rusk, “Comment: The American Electoral Universe: Speculation and Evidence,” *ibid.*, 1028–49; and Burnham, “Rejoinder to ‘Comments’ by Philip Converse and Jerrold Rusk,” *ibid.*, 1050–57. John J. Stucker, like Rusk a former student of Converse at the University of Michigan, has joined Rusk in two further contributions to the legal-institutional theory of electoral change: “The Effect of the Southern System of Election Laws on Voting Participation: A Reply to V. O. Key, Jr.,” in Joel H. Silbey et al., eds., *The History of American Electoral Behavior* (Princeton, 1978), 198–250; and “Legal-Institutional Factors in American Voting,” in Walter Dean Burnham et al., eds., *A Behavioral Guide to the Study of American Electoral History* (Cambridge, Mass., forthcoming). For Burnham’s most recent and developed statement, see his “The System of 1896: An Analysis,” in Paul Kleppner et al., eds., *The Evolution of American Electoral Systems* (Westport, Conn., forthcoming). Finally, for an evaluation of the behavioral and legal-institutional positions in light of the decline in voter turnout in the early twentieth century, see Paul Kleppner and Stephen C. Baker, “The Impact of Registration Requirements on Electoral Turnout, 1900–1916: Multiple Tests of Competing Theories,” paper delivered at

electoral reform of the 1890s involved a conscious effort to shape the political arena by disrupting opposition parties, revising traditional campaign and voting practices, and ensuring Republican hegemony—all under the mild cover of procedural reform. This development was the adoption of so-called antifusion laws, which also altered the political behavior characteristic of the Gilded Age, with varying effects on the role of third parties, modes of political participation, and the electoral process itself.

FUSION, OR THE ELECTORAL SUPPORT OF A SINGLE SET of candidates by two or more parties, constituted a significant feature of late nineteenth-century politics, particularly in the Midwest and West, where full or partial fusion occurred in nearly every election. Such fusions customarily involved a temporary alliance between third parties and the weaker of the two major parties, usually the Democrats in the Midwest and West. In the 1878 congressional elections, however, Indiana Greenbackers fused in one district with Democrats and in another with Republicans. That fusion often seemed to have a life of its own was evident in the Greenback effort of 1884 to arrange fusions in each state with whatever party was in the minority.² Fusion plans were generally undertaken, nevertheless, to promote the needs of the major party and were generally initiated or avoided according to the calculations of its politicians rather than those of the leaders of the evanescent third parties. Thus, the Republicans sometimes arranged fusions in the South but retreated whenever their participation in such a campaign might work against the Democratic divisiveness they sought to exploit. Similarly, in the West, Democrats repeatedly fused on third party tickets even over the bitter opposition of independents—that is, any third party followers in the nineteenth-century usage—who feared absorption by the major party or accusations of ideological betrayal. But, if fusion sometimes helped destroy individual third parties, it helped maintain a significant third party tradition by guaranteeing that dissenters' votes could be more than symbolic protest, that

the annual meeting of the American Political Science Association, Washington, D.C., August 1979. Converse has noted that Burnham's "conspiratorial interpretation" prompted his own work; "Comment on Burnham's 'Theory and Voting Research,'" 1024. Much of Rusk's work seems similarly motivated; see his "Comment: The American Electoral Universe," 1045–46. In 1974 Burnham backed away from suggestions of a conspiracy; "Theory and Voting Research," 1022. In one prominent exception to the "nonpartisan" thesis of the legal-institutionalist school, however, J. Morgan Kousser has argued that "the cross-fertilization and coordination" between Democratic movements to quash political opposition legally in the South "amounted to a public conspiracy"; see Kousser, *The Shaping of Southern Politics: Suffrage Restriction and the Establishment of the One-Party South, 1880–1910* (New Haven, 1974), 39. Rusk was prepared to "find a conspiracy which used legal means to control the system" within the Democratic South but strongly denied that one existed among Republicans in the North. He quite rightly recognized that the "paramount" issue in determining the nature of electoral change is that of "legislative intent"—"who urged the passage of these laws and why?" See his "Comment: The American Electoral Universe," 1045–46. The present essay will concentrate on those two questions in explaining one particular Northern electoral development.

² *Appleton's Annual Cyclopaedia*, 1878 (New York, 1879), 443; *Chicago Daily Tribune*, September 4, 1884; and Fred E. Haynes, *Third Party Movements since the Civil War* (1916; reprint ed., New York, 1966). An extreme example of the complexity of fusion politics came in North Dakota in 1890 when the Independents fused with the Prohibitionists to nominate candidates for governor and auditor, after which this coalition fused on Republican nominees for lieutenant governor and congressman and Democratic nominees for secretary of state and attorney general. *Appleton's Annual Cyclopaedia*, 1890 (New York, 1891), 629.

their leaders could gain office, and that their demands might be heard. Most of the election victories normally attributed to the Grangers, Independents, or Greenbackers in the 1870s and 1880s were a result of fusion between those third party groups and Democrats. That some politicians regarded fusion as a mechanism for proportional representation is not surprising.³

Fusion was a particularly appropriate tactic given the period's political culture. Voter turnout was at a historic high, rigid party allegiance was standard, and straight-ticket voting was the norm. Partisanship was intense, rooted not only in shared values but in hatreds engendered by cultural and sectional conflict. Changes in party control resulted less from voter conversion than from differential rates of partisan turnout or from the effect of third parties. Although the Republicans continued to win most elections, moreover, the era of Republican dominance had ended in the older Northwest by 1874 and had been considerably eroded in the states farther west by the 1880s, so that elections were bitterly contested campaigns in which neither major party consistently attracted a majority of the voters.⁴ Minor parties regularly captured a significant share of the popular vote and received at least 20 percent in one or more elections from 1874 to 1892 in more than half of the non-Southern states. Even where their share was smaller, it represented a critically important proportion of that electorate. Between 1878 and 1892 minor parties held the balance of power at least once in every state but Vermont, and from the mid-1880s they held that power in a majority of states in nearly every election, culminating in 1892 when neither major party secured a majority of the electorate in nearly three-quarters of the states.⁵ By offering additional votes in a closely divided electorate, fusion became a continuing objective not only of third party leaders seeking personal advancement or limited, tangible goals but also of Democratic politicians inter-

³ *New York Herald*, March 12, August 13, 1892; *Chicago Daily Tribune*, September 2, 1884; and Haynes, *Third Party Movements*. Also see Lee A. Dew, "Populist Fusion Movements as an Instrument of Political Reform, 1890-1900" (M.A. thesis, Kansas State Teachers College, Pittsburg, 1957). Fusion was not, of course, always successful, but it did offer the best chance of overcoming the Republicans. As one South Dakota Republican observed, "No fusion means Republican victory"; *Brookings County (S.D.) Press*, September 29, 1892. Even when defeated the policy of fusion caused a great deal of uncertainty within Republican ranks. In 1884, for instance, Republican Senator William B. Allison of Iowa warned party "managers in the East that this fusion of the Democrats with [Benjamin F.] Butler's forces in the West would require some attention and that we could not afford to rest on our oars with the field combined against us"; *Chicago Daily Tribune*, September 7, 1884.

⁴ For general discussions of the period's political culture, see Barnham, "Changing Shape of the American Political Universe"; Paul Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850-1900* (New York, 1970) and *The Third Electoral System, 1853-1892: Parties, Voters, and Political Cultures* (Chapel Hill, 1979); and Melvyn Hammarberg, *The Indiana Voter: The Historical Dynamics of Party Allegiance during the 1870s* (Chicago, 1977).

⁵ These conclusions as to the political importance of minor parties are derived from data that Paul T. David recorded for gubernatorial and presidential elections in the thirty non-Southern states; see his *Party Strength in the United States, 1872-1970* (Charlottesville, Va., 1972), 102-2E6. Even these statements underestimate the role of minor parties, because David systematically adjusted his data to discount the minor parties precisely when they engaged in fusion. I have made allowances for this adjustment only in a few obvious instances, as in the 1892 presidential returns for North Dakota or Wyoming. I have focused on state elections here, because electoral laws were a function of individual state legislatures. Paul Kleppner emphasized the same point from a wider perspective when he wrote, "The mean vote cast for minor parties in both the 1876-88 and 1876-92 sequences of biennial elections exceeded the major-party mean partisan lead in the Midatlantic, the East North Central, the West North Central, and the Western regions of the country, as well as in the United States as a whole"; *The Third Electoral System*, 239.

ested in immediate partisan advantage. The tactic of fusion enabled Democrats to secure the votes of independents or disaffected Republicans who never considered voting directly for the Democracy they hated; it permitted such voters to register their discontent effectively without directly supporting a party that represented negative reference groups and rarely offered acceptable policy alternatives.

The use of separate party ballots constituted another feature of the political culture of the Gilded Age that facilitated fusion. Each party printed and distributed its own ballot, without the necessary involvement of either state officials or the candidates themselves. The ballots, or tickets, were strips of paper on which only the names of the candidates of that party appeared. The individual voter could remain ignorant of the nominees of other parties; he merely had to deposit his party ticket in the ballot box, without studying or, in some states, even marking it. This election system allowed partisans of fusing parties to cast their votes without explicitly acknowledging their shared behavior or its significance, and it enabled a party to pursue fusion with an unwilling partner.

Given their vulnerability to fusion politics, Republicans continually sought to prevent cooperation among their opponents. Repeatedly, they pointed out the contradictions in the platforms of the different groups contemplating fusion and urged members of each to adhere to their own principles rather than to fuse with groups holding obviously different aims. Although the Republican motive was transparent, the argument held considerable force, particularly for conservative Democrats, for those Democrats and third party followers who believed in the representative nature of parties and nominations (not uncommon among minority political groups), and for those third party supporters who were interested in the development of their party and realized their reform goals required more than just immediate and perhaps counterproductive electoral victories.⁶ At times Republicans tried to encourage these antifusion elements among their opponents by going beyond such attempts to incite partisan prejudice and actually subsidizing their activities and party newspapers. In 1878 Indiana Republicans even underwrote a separate campaign by the Greenbackers, hoping thereby to draw votes from the Democrats. One final, if perhaps unofficial, tactic to sabotage fusion was demonstrated in Michigan's legislative elections of 1884, when Republicans distributed "bogus tickets calculated to deceive the greenbackers and democrats" by substituting the names of the Republican nominees for the fusion candidates on what otherwise appeared as a regular, fusion-party ballot.⁷

The effectiveness of this type of ballot trickery in disrupting fusion was easily surpassed by the possibilities inherent in the Australian ballot system. The presidential election of 1888, with its widespread incidents of bribery, intimidation,

⁶ For examples of such Republican appeals, see the *Portland Morning Oregonian*, October 25, 26, 27, 28, 1892; and the *Minneapolis Tribune*, October 12, 20, 21, 1892. For an examination of the issue of representativeness in parties, see Austin Ranney, *Curing the Mischief of Faction: Party Reform in America* (Berkeley and Los Angeles, 1975).

⁷ *Detroit Evening News*, November 1, 1884. Morton Keller, *Affairs of State: Public Life in Late Nineteenth-Century America* (Cambridge, Mass., 1977), 282; and *Des Moines Farmers Tribune*, August 18, 1897.

and fraudulent voting, provoked a reaction against the partisan excesses possible in the party-ballot system of voting and helped spur most states toward adopting the Australian ballot, long advocated by a number of disparate groups. This system did more than merely ensure secrecy for the voter. It also provided for an official ballot printed at public expense and distributed only by public election officers at the polling place. The system featured a blanket ballot, moreover, which contained the names of all of the candidates legally nominated by any party. The candidates' names were arranged on the ballot in one of two general patterns, the office-bloc or the party-column format. On the office-bloc ballot, candidates were grouped under the name of the office sought and their partisan affiliations were shown. The voter made his choice for each office by marking a square corresponding to the appropriate candidate. On the party-column ballot, candidates were grouped by party and listed in parallel columns. In some states the ballot laws even placed emblems or vignettes at the head of the columns to enable the voter to distinguish more easily the separate parties. Finally, lawmakers frequently added to the ballot a device to facilitate straight-ticket voting, a party circle, which, when marked, constituted a vote for the entire party ticket. These developments represented legislative efforts to retain some of the familiar, partisan features of the old ballot system while providing the secret and official characteristics of the new.⁸

By providing for public rather than partisan control over the ballots and by featuring a blanket ballot, the Australian system opened to Republicans, given their dominance in state governments, the opportunity to use the power of the state to eliminate fusion politics and thereby alter political behavior.⁹ The Republicans' modifications of the Australian ballot were designed to take advantage of the attitudes and prejudices of their opponents and were based on a simple prohibition against listing a candidate's name more than once on the official ballot. This stipulation, Republicans believed, would either split the potential fusion vote by causing each party to nominate separate candidates or undermine the efficacy of any fusion that did occur, for in this time of intense partisanship many Democrats would refuse to vote for a fusion candidate design-

⁸ The scholarly literature on the development of the Australian ballot is surprisingly thin and analytically unsophisticated. But see L. E. Fredman, *The Australian Ballot: The Story of an American Reform* (East Lansing, Mich., 1968); and Eldon C. Evans, *A History of the Australian Ballot System in the United States* (Chicago, 1917).

⁹ It is not asserted here that Republicans enacted the Australian ballot in the first place for such partisan purposes, and Rusk's attempt to deny the partisan effect of the Australian ballot by noting that both Democrats and Republicans voted for the initial reform in state legislatures is unsatisfactory. Rusk, "Comment: The American Electoral Universe," 1045. The law itself and its basic provisions for a secret, public ballot did not become the object of contention (except in rare cases as in New York) so much as the modifications of the Australian ballot system and the use that could be made of them did. As one opponent of subsequent Republican ballot changes in South Dakota said, "The real trouble is the change from the law as it originally stood." Another Dakota correspondent noted that each legislature after the one that had enacted the Australian ballot "has been tinkering at the law, and . . . wrapped the ballot in technicalities." After a Populist governor urged "that the old safe-guards which have been one-by-one repealed since the passage of the original law be reinstated," a Populist legislature adopted a law providing "for a return to the method when the Australian system was first adopted." Sioux Falls (S.D.) *Argus-Leader*, January 11, 1895; *Chicago Daily Tribune*, January 4, 1897; *South Dakota Senate Journal* (Pierre, 1897), 43-44; and Yankton (S.D.) *Press & Dakotan*, February 11, 1897. For opposition in New York to the Australian ballot itself on practical, ideological, and partisan grounds, see the discussion in Herbert J. Bass, "I Am a Democrat": *The Political Career of David Bennett Hill* (Syracuse, 1961), 96-101, 128-30, 133-35, 147-48, 151-53.

nated "Populist" and many Populists would feel equally reluctant to vote for a "Democrat."¹⁰ Related regulations could restrict straight-ticket voting by fusionists or even eliminate one of the fusing parties, antagonizing its partisans and causing them either to oppose the fusion arrangements or to drop out of the electorate altogether. Given the closely balanced elections of the late nineteenth century, the elimination of even a small faction of their political opponents because of ideology, partisanship, or social prejudice would help guarantee Republican ascendancy. Although other ballot adjustments increased its effectiveness, this simple prohibition against double listing became the basic feature of what the Nebraska supreme court described as a Republican effort to use the Australian ballot as a "scheme to put the voters in a straight jacket."¹¹

Publicly, Republicans defended this prohibition as necessary for achieving equal treatment, efficiency, and an end to political corruption, and they insisted that technically it did not interfere with nominations or voting. But, given what one Wisconsin judge called "the strength of party ties" and the reality that "political rights are universally exercised through party organizations," the logic of the law lay in his conclusion that "its only purpose is to prevent fusion." The law, he continued, "will prevent no illegal vote from being cast, nor will it stop any corrupt practice, nor in any way preserve the purity of the ballot." It was designed, instead, to interfere with "the freedom of action of the party . . . [and] of the citizens who compose that party." The Republican judicial rejoinder, of course, was that "mere party fealty and party sentiment, which influences men to desire to be known as members of a particular [political] organization, are not the subjects of constitutional care."¹² The law, then, was intended to promote the dissolution of party ties while giving Republicans the residual benefits of them.

THE POSSIBILITIES OF ADJUSTING THE BALLOT SYSTEM in this direction became evident during the 1892 presidential campaign, the first held under the original Australian ballot system. That campaign marked as well the initial national appearance of the most important third party of the late nineteenth century, the People's Party, which had its greatest appeal to economically distressed farmers in the Western states, traditionally controlled by Republicans. In an effort to increase the electoral chances of its presidential candidate, Grover Cleveland, the Democratic National Committee urged party officials in several Western states to withdraw their nominees for the electoral college and fuse on the Populist nominees, thereby denying Republicans the electoral votes that Cleveland would be unable to capture for himself.

¹⁰ It was "well known," one newspaper observed, that many voters would not vote for a candidate unless he were listed on their ticket. "This may be a prejudice, but it is not an unworthy one in a community where party government is recognized." *Detroit Free Press*, March 15, 1895. An Ohio Greenbacker had made the same point earlier and more graphically: "Men would as soon cut off their right hands almost as vote a Democratic ticket." *Cincinnati Enquirer*, August 22, 1877, as quoted in R. C. McGrane, "Ohio and the Greenback Movement," *Mississippi Valley Historical Review*, 11 (1925): 535.

¹¹ *State v. Stein* (Neb.), 53 N.W. Rep. 999.

¹² *State v. Anderson* (Wisc.), 76 N.W. Rep. 482.

The responses in Oregon and Minnesota proved most significant. Both states were controlled by Republicans although the GOP represented only a minority of voters in each. Hoping to arrange a successful fusion, Democratic officials withdrew one of their four nominees in Oregon and four of their nine nominees in Minnesota, replacing them with candidates nominated by the Populists. In both states many Populists denounced the Democratic maneuver, worrying that, as one Minnesota Populist elector said, the tactic "will hurt the People's Party rather than help it, as a great many in that party were formerly Republicans, and . . . will have a tendency to drive them back to the old party." Many Democrats also complained of the arrangement, but gradually most concluded that, although fusion with the Populists was distasteful, "in the present case, the end justifies the means," as the Oregon state chairman observed.¹³ The initial Republican reaction to these fusion arrangements also followed the customary pattern. To their own partisans Republican leaders stressed two contradictory conclusions: fusion was a confession of Democratic weakness, but Republicans would have to turn out in greater numbers to vote it down. To Democratic and Populist voters, the Republican leaders appealed separately, insisting that fusion required their party to subordinate its own sacred principles and able candidates to those of the other party.¹⁴

These fusion campaigns differed from previous ones, however, because of the Republicans' partisan implementation of the Australian ballot law, which both states had enacted in 1891. In adopting the office-bloc form of ballot, the Oregon legislature had also prohibited the name of any candidate from appearing more than once on the ballot. Perhaps this provision had seemed a logical corollary to the ballot type, for it excited no comment at the time. When in 1892 Democratic officials recognized the implications of that clause for their fusion plans, they argued that another provision, which permitted the names of electoral college candidates to be grouped by parties, allowed the fusionist elector, Nathan Pierce, to be listed with Democrats as well as with Populists on the ballot. Republicans countered that Pierce's name could be listed only once and identified as a "Populist" or at most "Populist-Democrat," expecting that the word "Democrat" would be a signal to Republican-Populists to scratch the name and that the Populist designation would alienate some Democratic voters: "a very pretty jungle," in the words of one Republican editor.¹⁵

The question of ballot form appeared so late in the campaign that there was no time to secure a legal decision, and county clerks turned to party leaders for guidance in printing the official ballots. The ballot devised by the Democratic state committee and subsequently copied by Democratic county clerks listed Pierce's name in both the Democratic and Populist groupings, while those county clerks who followed the instructions of the Republican state chairman

¹³ *Minneapolis Tribune*, October 13, 19, 24, 1892; and *Portland Morning Oregonian*, October 28, November 2, 1892.

¹⁴ *Portland Morning Oregonian*, October 25, 26, 27, 28, November 2, 1892; and *Minneapolis Tribune*, October 12, 20, 21, 1892.

¹⁵ *Portland Morning Oregonian*, October 27, 28, 30, 1892.

listed Pierce only among the Populist nominees, though designating him with both party affiliations.¹⁶

Because of this singular ballot situation, Oregon's election results revealed both the value of fusion and the effect of ballot format in shaping electoral outcomes and disrupting fusion coalitions. The Republicans won three of the four electoral votes, averaging 35,000 votes for their candidates. The straight Populist candidates averaged 27,000 and the straight Democrats 14,000. Had Pierce received the full vote of both parties he would have been an easy victor with approximately 41,000 votes, but he squeaked through with only 35,811. Regression analysis indicates that in those counties in which his name was listed under both Democratic and Populist groupings virtually all Populists voted for their fellow partisan, while 92 percent of the Democrats also supported Pierce, an indication of some hostility to fusion but also of a general willingness to vote the Democratic ticket and all who were designated on it. But, in those counties in which Pierce's name was listed on the ballot only once (under the Populist group), 9 percent of the Populist voters refused to support a Populist who was also labeled a Democrat, although he was identified as a Populist and listed with the other Populist electors. And 29 percent of the Democrats refused to vote for a Democratic candidate who was also listed as a Populist.¹⁷ Republican expectations as to voter behavior had proved accurate.

While Oregon provides the most revealing evidence of the effect of ballot format on voting behavior and fusion politics, events in Minnesota proved more immediately influential for electoral reform. Like that in Oregon, the ballot law in Minnesota also established the office-bloc format, but without the restriction on listing candidates' names more than once. In preparing the official ballot for 1892, however, the Republican secretary of state simply proceeded as though that were a legal requirement, grouping the five straight Democratic electors separately and scattering the four endorsed Populists among the five other Populists, though designating them as both Populist and Democratic. Democratic officials charged that the Republican ballot design was constructed to "render it more difficult for the voter to cast his vote according to his preference" and sought a court order to compel the double listing of fusion electors. Democratic lawyers argued that, as drawn up, the official ballot would disfranchise twenty thousand voters. But, since the ballots were already printed, the court was confronted with a Republican *fait accompli*, the reversal of which would have required a postponement of the election itself, and accordingly the court judiciously ruled that it had no jurisdiction in the matter.¹⁸

Ignoring their own structural revolution, Republicans crowed that "the court

¹⁶ *Ibid.*, October 28, 30, 1892.

¹⁷ *Appleton's Annual Cyclopaedia, 1892* (New York, 1893), 615; and *Portland Morning Oregonian*, November 11, 1892, January 6, 1893. Estimates of voter behavior were derived from ecological regressions calculated for those twenty-seven (of thirty-two total) counties for which firm evidence exists as to the ballot format employed. For the best introduction to this technique, see J. Morgan Kousser, "Ecological Regression and the Analysis of Past Politics," *Journal of Interdisciplinary History*, 4 (1973): 237-62; and W. P. Shively, "'Ecological Inference': The Use of Aggregate Data to Study Individuals," *American Political Science Review*, 63 (1969): 1183-96.

¹⁸ *Minneapolis Tribune*, October 16, 18, 19, 1892.

and secretary of state do not propose to become the cat's paws of the fusion schemers and turn the ballot upside down to suit their political ends." Democrats found consolation in the publicity their court case had engendered: it called voter attention to the structure of the ballot and indicated how Democrats would have to vote. Indeed, even Republicans argued that repeated instruction in "the science and art of casting a ballot under the Australian system" would be more valuable than "profound dissertations on the tariff and the currency." The election results validated that estimate of the importance of the ballot. The straight Democratic electors averaged 101,000 votes and the straight Populists 29,000; their combined total would have easily defeated the Republicans' 113,000. Yet the four fusion electors received only 110,000 votes, the drop of 20,000 that Democratic officials had predicted, which allowed the minority Republicans to sweep to complete victory.¹⁹

The massive vote differentials in these states were largely a function of institutional change in the voting system, but they also involved a behavioral component, for the ballot arrangements were advertised and explained extensively, and voters could have selected the fusion candidates if they had been willing to vote with a different party. That some voters were obviously unwilling to ally themselves even symbolically with another party testifies again to the nature and strength of partisan affiliation in the political culture of the time. In evaluating the decline in the fusionists' votes, one Minnesota election judge observed, "It matters not whether this was the result of sharp practice or not, the fact remains . . . they were cheated out of their votes" by the "system of voting."²⁰ But, significantly, the decline was not an "unintended consequence" of ballot change but rather resulted from "sharp practice." The institutional change had been purposely designed to exploit the observed behavioral patterns in the political culture and did not represent some abstract or disinterested impulse toward "reform."

This basic reality became increasingly obvious from the reactions in other states to the Minnesota experience. Neighboring Wisconsin, traditionally Republican, had gone Democratic in 1892 because of local circumstances. Fusion had occurred at several levels and, as one Republican editor complained, "the labor party, or people's party, or Farmers' Alliance, assisted to place in power" the Democrats, and "without those voters the democratic party is in a minority in the state." To protect these voters and their own new position, Democratic legislators amended the election law in 1893 specifically to provide for dual ballot listings in the event of fusion nominations.²¹

By 1893, Michigan had perhaps experienced more consistent fusion politics than any other state, and the new, Republican legislature decided to revise the Australian ballot law that had been enacted by its Democratic predecessor. Although there was considerable discussion about the need "to purify elections

¹⁹ *Ibid.*, October 10, 19, 1892. John D. Hicks, "The People's Party in Minnesota," *Minnesota History Bulletin*, 5 (1924): 545.

²⁰ *Minneapolis Tribune*, November 17, October 19, 1892.

²¹ Madison *Wisconsin State Journal*, January 3, 1893; and *The Registry and Election Laws of the State of Wisconsin* (Madison, Wisc., 1894), 26.

and prevent fraud thereat," the GOP's objective clearly was, as one Democrat observed, to "purify elections according to the Republican idea of purity, and prevent frauds by all other parties." One Republican legislator, at least, was candid: "We don't propose to allow the Democrats to make allies of the Populists, Prohibitionists, or any other party, and get up combination tickets against us. We can whip them single-handed, but don't intend to fight all creation."²² The Republicans' solution was ballot manipulation, a tactic they first applied retroactively by unseating non-GOP legislators whose names had been listed upon more than one ticket. The presiding officer refused even to entertain Democratic protests against this "revolutionary action," but the state supreme court partially restrained the Republican majority by upholding the legality of ballots with dual listings. The Republicans then countered by moving to amend the state's election law by prohibiting double listing of candidates' names on the ballot. This effort also failed, but by only three votes. All forty-eight votes in favor of the bill were Republican; all Democrats and Populists voting opposed the measure.²³

This Republican attempt to unseat legislators reveals an important aspect of the movement for the legal disruption of fusion politics. Fusion occurred most often in local and state-legislative contests, where the candidates' personal popularity was more likely to displace partisan issues in determining voters' preferences. In Michigan, for example, fusion did not materialize in the presidential race of 1892, and the Populists made a negligible showing. But Populist strength was regional and often proved decisive in local contests. In the legislature of 1893, which first debated the issue, twenty-one of the thirty-one Democratic and Populist state representatives had been elected through fusion, while twenty-six Republican representatives had been elected over fusion opponents. At least twenty more Republicans were elected only by plurality votes and would have been defeated if their opponents had successfully fused. Thus, a major object of antifusion legislation was at times local, not national, politics. When Michigan did, in fact, later enact an antifusion law, its passage owed some of its immediate support to a pending special congressional election in southwestern Michigan, where Populists were strongest and where the Republican candidate, the presiding officer of the state senate, was opposed by the fusion nominee of Populists, Prohibitionists, Silverites, and Democrats. This local nature of fusion was what prompted interest in antifusion legislation in those states where, at the aggregate level, it did not seem necessary or important. But a focus on the small total number of Populists in large industrial states like Michigan or Wisconsin is misleading in other ways as well. While Michigan Populists polled only 4.3 percent of the total state vote in 1892, that proportion gave them the balance of power in the closely contested electorate; when delivered to Democratic can-

²² *Detroit Free Press*, February 1, January 5, 1893.

²³ *Ibid.*, January 4, 19, February 15, 16, 25, 1893; *Journal of the House of the State of Michigan, 1893* (Lansing, 1893), 697, 1031; and *Official Directory and Legislative Manual of the State of Michigan, 1893-4* (Lansing, 1893), 706-11.

didates through fusion, as in the contest for attorney general, it sufficed to bring about the only Republican losses on the state ticket.²⁴

South Dakota succeeded where Michigan failed in 1893 in passing the first explicitly antifusion law.²⁵ Constituting only a minority of the voters, the state's Republicans had apprehensively watched developments in Minnesota and devoted their own 1892 campaign "almost exclusively to the business of preventing a fusion" between Populists and Democrats. In 1893 they carried this objective into the legislature and enacted a number of changes in election laws. The most important simply provided that "the name of no candidate shall appear more than once on the ballot for the same office." Related changes similarly designed to frustrate fusion included a prohibition against the withdrawal of candidates shortly before elections, called "the Minnesota plan" of fusion; a provision to treat fusing parties as a single party when appointing election judges; and the replacement of the office-bloc with the party-column format, containing a party circle provision for straight-ticket voting.²⁶ This last modification made more effective the prohibition against double listing, for by requiring party columns a candidate could be identified with only one party affiliation, unlike candidates on the Oregon and Minnesota ballots, and the second party to nominate a candidate would appear on the ballot as having no nominee for that office at all. Those wishing to fuse would thus lose the symbolic protection of voting for their own party and be required to vote as members of another party. When fusion did not involve whole tickets, which was the usual case, fusion voters would also lose the advantage of the party circle and have to check each individual name—a provision that was certain to complicate voting and lead to the invalidation of ballots through improper marking.²⁷

The effects of these new ballot provisions were felt in the state's elections in 1893 and 1894, when, as Republicans observed, they served as a "stumbling block" to their opponents. Populists and Democrats named separate state tickets in order to maintain their parties' organization and independence, though each party conceded that such separation would lead to a Republican victory. The weaker Democrats, in particular, feared that under the new law cooperation with Populists would be "not fusion but absorption." Although fusionist leaders made some local efforts at fusion, they predicted that at least 20 percent of the

²⁴ Richard Harvey Barton, "The Agrarian Revolt in Michigan, 1865-1900" (Ph.D. dissertation, Michigan State University, 1958), 125-51; *Kalamazoo Weekly Telegraph*, October 19, 1892, March 20, 1895; *Official Directory and Legislative Manual of Michigan, 1893-4*, 593-625; and *Appleton's Annual Cyclopaedia*, 1892, 467.

²⁵ The Oregon law of 1891 cannot be so considered for it was passed without apparent recognition of its significance, was still open to contrasting interpretations, and preceded a formal antifusion law enacted in 1895. Both Kentucky and Indiana had early laws that contained provisions resembling those characteristic of antifusion laws but that were really designed to deal with the possibility of nonpartisan nominations by petition rather than with party action. In practice, moreover, their election laws were not interpreted in a fashion to prevent fusion. For a discussion of the local political context surrounding the development of antifusion legislation in South Dakota, consult my "Confusion to Democracy: Ballot Laws and Politics, 1890-1902," paper delivered at the thirteenth annual Northern Great Plains History Conference, Fargo, N.D., October 27, 1978, pp. 2-6.

²⁶ *New York Times*, October 21, 1892; *South Dakota House Journal* (Pierre, 1893), 862; *South Dakota Senate Journal* (Pierre, 1893), 58, 283-86, 1006; and *Yankton (S.D.) Press & Dakotan*, March 9, 23, 1893.

²⁷ See *Sioux Falls (S.D.) Argus-Leader*, November 4, 1893; *Yankton (S.D.) Press & Dakotan*, November 24, December 8, 1892; and *DeSmet Independent*, as quoted in *Brookings County (S.D.) Press*, November 2, 1893.

Democrats would refuse to vote outside their party name, a fall-off that spelled defeat in a close election. After the expected Republican victory, one Democratic party official observed, "Under the present system of voting as arranged by the Republican party, fusion results in confusion to Democracy."²⁸

Nationally, the Republican success in 1894 led to the passage of antifusion laws by other states in 1895. Oregon Republicans, who had captured a majority in the legislature with only a minority of the popular vote, formally enacted an antifusion statute.²⁹ In neighboring Washington, after successfully campaigning against "fusion schemes," the Republicans applied the force of the one-listing provision in the party-column format to the office-bloc ballot by stipulating that only one party affiliation could be designated for any candidate.³⁰ Michigan Republicans, now in complete control of the legislature, reintroduced their anti-fusion bill of the previous session and pushed it into law. Although some judges described it as "unconstitutional" and "revolutionary," the state supreme court upheld the measure in the same partisan spirit in which it had been enacted—four Republican judges in the affirmative, one Democrat in dissent.³¹ The Ohio legislature, meeting in 1896, concluded this first legislative flurry with the so-called Dana law, an elaborate measure based upon the customary antifusion ballot requirement. In Ohio, the local focus of antifusion legislation seemed particularly evident, at least initially. In the recent Cincinnati mayoral election, the Republican machine of "Boss" George Cox and Joseph B. Foraker had been challenged by a fusion coalition of Populists, Socialists, laborites, and dissident Republicans that had nearly received the Democratic endorsement as well. The regular Republicans had reacted "as if civilization were at stake." Some legislative observers regarded the subsequent Dana bill, prepared by a Foraker Republican, as primarily designed to prevent just such unified popular revolts against machine rule in municipal elections. Indeed, the Republican legislative majority, so large as to be "dangerous," according to one editor, voted down a proposed amendment to exclude municipal elections from the antifusion provisions.³²

THE LARGER POLITICAL IMPORTANCE of these new antifusion laws was promptly demonstrated in the presidential election of 1896, the pre-eminent fusion cam-

²⁸ *Brookings County (S.D.) Press*, October 19, 1893; and *Sioux Falls (S.D.) Argus-Leader*, August 3, 29, September 5, 6, November 13, 1894.

²⁹ *Appleton's Annual Cyclopaedia*, 1894 (New York, 1895), 636; *Appleton's Annual Cyclopaedia*, 1895 (New York, 1896), 632; *Oregon House Journal* (Salem, Oreg., 1895), 1007-08; and *Oregon Senate Journal* (Salem, Oreg., 1895), 631, 640.

³⁰ *Spokane Spokesman-Review*, November 1, 10, 1894; *House Journal of the State of Washington*, 1895 (Olympia, Wash., 1895), 667-72; and *Senate Journal of the State of Washington*, 1895 (Olympia, Wash., 1895), 709. Republican legislators backed this "reform" by a vote of 68 to 1, while Populists opposed it by a margin of 17 to 3.

³¹ *Journal of the Senate of the State of Michigan*, 1895 (Lansing, 1895), 112, 373-74, 457, 775-78; *Journal of the House of the State of Michigan*, 1895 (Lansing, 1895), 961; *Kalamazoo Weekly Telegraph*, March 20, 1895; *Detroit Free Press*, March 26, 1895; and *Todd v. Election Commissioners*, 104 Mich. 474, 486 (1895).

³² Zane L. Miller, *Boss Cox's Cincinnati: Urban Politics in the Progressive Era* (New York, 1968), 89; *Cincinnati Enquirer*, November 7, 1895; *Cleveland Plain-Dealer*, April 9, 1896; *The Journal of the Senate of the State of Ohio*, 1896 (Norwalk, Ohio, 1896), 399-400; and *The Journal of the House of the State of Ohio*, 1896 (Norwalk, Ohio, 1896), 689.

paing of the late nineteenth century, when Democrats, Populists, and Silver Republicans fused on the candidacy of William Jennings Bryan. In those antifusion states, like Ohio, in which Democrats constituted by far the major portion of Bryan's supporters, the Populists were sacrificed in a way that not even middle-of-the-road Southerners anticipated when they opposed the Populist endorsement of Bryan.³³ Ohio state election officials announced that the Dana law would eliminate a Populist national ticket from the ballot and, through its party-column and marking procedures, might invalidate ballots that tried to combine support for Bryan with a state or local Populist ticket. To avoid that outcome, Democratic and Populist leaders reluctantly also agreed to fuse on complete state and local tickets. The Democratic state committee withdrew the Democratic nominees for several offices and substituted Populist nominees in exchange for Populist acceptance of the remaining Democratic candidates. All candidates were listed on the ballot only under the Democratic heading. Many Populists, however, were loath to become even nominal Democrats and objected to these arrangements in which their party "was left without a place on the ballot." Defiance County Populists even advocated rescinding Bryan's nomination in order to protect their own party, while other Populists refused to withdraw their nominations. Ultimately, in addition to the designated Democratic ticket, composed of both Democrats and Populists, the official ballot *did* list a severely truncated Populist ticket after all, so that a Populist could not easily vote a straight ticket.³⁶

A second antifusion provision of the Dana law threatened even the Democrats' ability to cast a straight ticket by prohibiting the entry of nominees on one party's ticket when they were certified as members of another party. Accordingly, the secretary of state prepared to split the arduously constructed composite ticket into separate columns after all. Thus, Populist nominees had to declare themselves as Democrats, causing still more disaffection, for the Populists announced that they had "already gone much further than they had wished in order to effect the fusion agreement" and were reluctant "to declare that they are Democrats and thereby destroy absolutely the individuality of the party organization they have been striving to represent." Finally, the Populist state chairman ignominiously had to assert that the Populists had not really made any nominations and to withdraw their certificates; election officials then placed the fusion slate on the ballot as the Democratic ticket.³⁵ Though the fusionists' troubled campaign ended in defeat, the straight Populists fared even worse with the election law. Their officially disavowed ticket failed to attract enough votes

³³ The following discussion involves only those developments that stemmed from antifusion legislation and does not cover any of the quite different difficulties with respect to fusion that Robert Durden has already described well; see his *The Climax of Populism: The Election of 1896* (Lexington, Ky., 1966). The phrase "middle-road," or "middle-of-the-road," referred to those Populists who opposed fusion or cooperation with either major party, which they regarded as being in the gutters of the political system—on each side of those who kept clean and pure in the middle of the road.

³⁴ *Bowling Green (Ohio) Daily Sentinel*, July 17, 28, August 28, September 16, 23, 1896; *Cincinnati Commercial Tribune*, August 8, 12, October 21, 1896; and *Columbus Ohio State Journal*, October 20, 24, 1896.

³⁵ *Cincinnati Commercial Tribune*, October 3, 4, 6, 7, 8, 1896.

to entitle the party to be on the Australian ballot in the future, except by petition. As one Populist disconsolately wrote his national chairman, "We have now in Ohio no People's party, but simply scattered organizations here and there."³⁶

In antifusion states where Populists constituted the majority of Bryan's supporters, roles were reversed but the same difficulties developed. In Oregon, Democrats had to withdraw their electors and accept the Populist ticket as their own. The Populist state chairman explained the result: "Under our statute they surrendered their legal autonomy by this act, so that we have but two parties in this state." The further consequence was that, whereas the Populists and Democrats running separate tickets had together captured 53 percent of the votes in the June state election on much the same issues, Democratic fall-off permitted them to attract only 48 percent of the vote in November under this rankling arrangement.³⁷ In Washington as well, after a committee of lawyers examined the "cunningly devised" election law, the Democrats felt forced to accept the Populist name for the fusion ticket. The outcry against sacrificing the party name was so great, however, that, to the end of the negotiations, it seemed likely that the

³⁶ Hugo Preyer to Marion Butler, March 19, 1897, Marion Butler Papers, Southern Historical Collection, University of North Carolina Library; and *Bowling Green (Ohio) Daily Sentinel*, November 10, 1896. This dissolution of Ohio's Populist party demonstrates the destructive effect on smaller parties of the interaction between the antifusion law and another standard provision of the Australian ballot system. The adoption of the Australian ballot meant of course that disgruntled citizens could no longer simply organize themselves spontaneously and enter the political arena independently by issuing their own party ticket. The use of an official, blanket ballot required the state to establish procedures to regulate the appearance of parties and their candidates on the ballot. This regulation usually involved, *inter alia*, defining a party that could appear on the ballot in terms of its percentage of the total vote in the preceding election. Manipulation of the minimum required percentage often reduced the number of minor parties by directly limiting their ability to present themselves for voter consideration. Petitioning provided an alternative method for gaining party access to the ballot. But, again, some states required unreasonably large numbers of signatures—or even specified a particular geographical distribution of the petitioners—so that, in practice, the candidates would be confined to the larger parties. Once parties were stricken from the ballot (through their candidates) by the operation of the antifusion law, they legally ceased to exist until their partisans successfully petitioned to secure ballot consideration again. Even the Democratic Party had no standing in those states where it fell victim to antifusion regulations; see pages 300–01, below. But small and poor or loosely organized parties faced particular difficulty in regaining an opportunity to appear on the ballot. Members of all political parties were extremely sensitive to this possible consequence of the interaction of the antifusion and other ballot provisions of the electoral law. Unfortunately, not even some of the Populists themselves could resist this legal opportunity to obstruct possible opponents by keeping them off the ballot in the first place. In Kansas, for instance, the regular Populists, after being troubled by a radical (middle-of-the-road) Populist separate ticket in 1896, amended the state's Australian ballot law in the 1897 legislature to quintuple the number of signatures required to gain a ballot position through petition and thereby keep "small bodies of reformers out of politics." Only rarely, of course, were Populists in a position to manipulate the legal parameters of politics, a point perhaps underlined by this same legislature's simultaneous ability to defeat an antifusion bill—on a strict party vote, all Republicans in favor, all Populists and Democrats opposed. *Dubuque (Iowa) Herald*, February 17, 1897; and *Senate Journal: Proceedings of the Senate of the State of Kansas* (Topeka, 1897), 787, 884–85, 1111, 1201. For examples of the more typical major party effort to obstruct new parties through the requirement of an extraordinary number of petition signatures, see Erik Falk Petersen, "The Struggle for the Australian Ballot in California," *California Historical Quarterly*, 51 (1972): 239; and Charles Chauncey Binney, "Merits and Defects of the Pennsylvania Ballot Law of 1891," *Annals of the American Academy of Political and Social Science*, 2 (1892): 751–71, esp. 757n, 758n. Beyond mandating procedures for securing a position on the ballot, the "infamous" Missouri election law required a party to receive one-third of the total votes cast or be "disbarred from all privileges and representation" in the appointment of election judges and clerks. *Kalamazoo Weekly Telegraph*, April 15, 1891. For a striking example of the local exclusion of small parties through legal regulations, see *Bowling Green (Ohio) Daily Sentinel*, March 27, 1896.

³⁷ John C. Young to Marion Butler, March 22, 1897, Butler Papers; and *Appleton's Annual Cyclopaedia*, 1896 (New York, 1897), 628.

Democrats would repudiate the plan, thereby defeating Bryan in a state where victory would have been easy under previous electoral rules.³⁸

Similarly in South Dakota, Democrats recognized that under the ballot law they had to sacrifice their party's organization to secure the state's electoral votes for their party's nominee. Accordingly, they canceled their state convention and adopted the ticket and the name of the People's Party. At the county level, however, Democratic opposition to voting under the Populist name frequently led to a compromise name of "Free Silver" for the local fusion ticket. But this ran afoul of the party-column and party-circle provisions of the ballot law and made it difficult for fusionists to cast a straight ticket for both state and local offices. The likely result under the ballot law, one newspaper predicted dryly, was "the loss of numerous votes for one ticket or the other or . . . loss of both tickets."³⁹

Michigan, with its fusion tradition and moderately strong third parties, furnished the final experience among antifusion states in 1896. Both Populists and Democrats objected to accepting the others' name and, unable to fuse in the customary fashion, therefore dropped all old party names to adopt a new collective one for the purpose of the ballot: the Democratic-People's-Silver Union. But even this stratagem had a weakness, for the state supreme court ruled that by entering the DPSU the regular Democratic organization had abandoned the name "Democrat." The court therefore awarded that designation on the ballot to a ticket named by bolting anti-Bryan gold Democrats, a decision that the Democratic state chairman understandably denounced as "an attempt to mislead the people."⁴⁰

The Michigan and Ohio experiences were not lost on Republicans in Indiana, the state in which Mark Hanna feared fusion most.⁴¹ Hoosier Republicans made two unsuccessful efforts during the campaign to secure the effects of antifusion legislation without actually having such a law. When the Populists and Democrats agreed on a common electoral ticket to appear on the ballot under both the Democratic rooster and the Populist plow and hammer vignettes, the Republicans sought to enjoin the fusionists from filing dual nomination papers. Populists were alarmed that, if this tactic succeeded, "a large proportion of our voters will be practically disfranchised." One Populist, who wrote his national chairman seeking legal assistance, expressed his fear that they would "have trouble in Ind. & perhaps all other states that have accepted the Australian system of ballots to get our fusion tickets on the official ballot." The Republican state chairman also instructed Republican election judges to separate fusion votes into Democratic and Populist totals, as though the parties had different candidates. Though both of these maneuvers failed, after the election the Republicans made immediately clear, as the *Chicago Tribune* reported, their in-

³⁸ *Spokane Spokesman-Review*, August 15, 18, 22, 1896; and Winston B. Thorson, "Washington State Nominating Conventions," *Pacific Northwest Quarterly*, 35 (1944): 104-05.

³⁹ *Sioux City (Iowa) Journal*, October 17, 1896; and *Appleton's Annual Cyclopaedia*, 1896, 707-08.

⁴⁰ *Detroit Free Press*, November 1, 1896; *Kalamazoo Weekly Telegraph*, August 26, September 2, 1896; and *Baker v. Board of Election Commissioners* (Mich.), 68 N.W. Rep. 752.

⁴¹ James S. Clarkson to H. G. McMillan, October 5, 1896, James S. Clarkson Papers, Library of Congress.



Figure 1: The "Popogriff," a hostile depiction of the combination of political parties and factions fusing on the Bryan candidacy of 1896: the Democratic donkey with its "regular head," Populist whiskers, middle-of-the-road (Populist) boots, Silver Party wing, Tammany tiger body, and anarchistic tail. "Pedigree of the Popocratic Mule: Sire—Democracy, Dam—Populism, Out of Free Silver by Anarchy"; *Judge*, October 24, 1896.

tention "to amend the election law, so as to prevent fusion like that perpetrated in the last campaign."⁴² The bill, involving the customary prohibition against double listing, was drawn up by the Republican state committee and then passed in the legislature by a vote of 83 to 58, all Republicans in favor, all Democrats and Populists opposed.⁴³

The lessons learned in, and the opportunity presented by, the sweeping 1896 Republican victory led Republican-dominated legislatures in many more states to enact antifusion laws quickly. Republican legislatures passed antifusion laws in 1897 in Illinois, Iowa, North Dakota, Pennsylvania, Wisconsin, and Wyoming as well as in Indiana. As Republicans gained sufficient legislative control elsewhere, the law spread still further: California and Nebraska in 1899; Kansas, Minnesota, and South Dakota in 1901; Idaho in 1903; and Montana in 1907.⁴⁴

⁴² Lew W. Hubbell to Marion Butler, September 13, 1896, Butler Papers; *Chicago Daily Tribune*, January 4, 27, 1897; and *Cincinnati Commercial Tribune*, October 28, 29, 1896.

⁴³ *Indianapolis Journal*, January 15, 1897; *Journal of the Indiana State Senate, 1897* (Indianapolis, 1897), 592; and *Journal of the Indiana House of Representatives, 1897* (Indianapolis, 1897), 967–68.

⁴⁴ *Appleton's Annual Cyclopaedia, 1897* (New York, 1898), 395, 419, 574, 664; New York State Library Bulletin, *Summary of Legislation* (Albany, 1897), 519; *Appleton's Annual Cyclopaedia, 1901* (New York, 1902), 702; and Arthur C. Luddington, *American Ballot Laws* (Albany, 1911), 15, 39, 43, 78. South Dakota's law of 1901 followed the Populists' repeal in 1897 of the original antifusion legislation of 1893. Republican legislators had passed an antifusion bill in 1899, only to have it vetoed by the Populist governor. This pattern of ballot legislation suggests the partisan motivation involved and indicates the common conviction of the law's political effects. See

ENDING THE EFFECTIVE COOPERATION of Democrats and third party groups was both the primary goal and the major result of these efforts. As the attorney general of one state noted, the antifusion law should have been renamed "an act to keep the populists in the middle of the road." Any cooperation that did take place under the new electoral rules involved a sacrifice of voters that rendered the whole less than the sum of its parts. If forced to vote for fusion as Democrats, many Populists declared, they would prefer to return to the GOP or simply not vote at all.⁴⁵ Analysis of the Kansas election returns of 1902 confirms this. Under the 1901 antifusion law, the fusion vote declined drastically from that of 1900. Those Populists who had voted for a fusion ticket under their own heading in 1900 proved little more likely to vote for a fusion ticket under a Democratic heading in 1902 (45 percent) than actually to vote Republican (40 percent), and, when confronted with that choice, a sizable minority either voted for a symbolic third party or dropped out of the electorate altogether.⁴⁶ This last course proved even more agreeable to South Dakota Populists. The proportion of original Populists willing to support fusion fell by two-thirds between 1900, when they could vote under their own heading, and 1902, when they were required to vote as Democrats following the Republican enactment of the antifusion law in 1901. And three-fourths of that shift was accounted for by a huge increase in those who simply refused to vote at all.⁴⁷

By preventing effective fusion, antifusion laws also brought an end to another major characteristic of late nineteenth-century politics—the importance and even existence of significant third parties.⁴⁸ Whether such legislation split the

Argersinger, "Confusion to Democracy," 11. In addition, the Democratic legislatures of three Southern states also enacted antifusion legislation in the early 1900s, and controversy over the law actually provoked a riot in the Kentucky legislature. Thus, while the focus here has been on Northern Republicans, the law was obviously regarded as serving the interests of the dominant party wherever it was enacted. Antifusion legislation was of minor importance in the South because the passage of more blatantly partisan electoral legislation obviated the need for subtler controls; see Kousser, *The Shaping of Southern Politics*. Antifusion laws were more appropriate to the more closely balanced North, where slight alterations in the electorate were sufficient to guarantee partisan control. Some Northern Republicans, however, likened their antifusion legislation to the South's repressive legislation. See, for example, Des Moines *Iowa State Register*, February 19, 1897.

⁴⁵ *Detroit Evening News*, March 16, 1895; *New York Times*, April 6, 1900; and Des Moines *Farmers Tribune*, August 3, 1897.

⁴⁶ Based on an ecological regression calculated over those sixty-nine counties for which 1900 fusion votes were separately returned according to their Populist and Democratic components. This 1902 election, moreover, finally marked an approximate return to the state's pre-Populist political alignments: the Republican vote correlated significantly with the Republican vote of the 1880s for the first time in more than a decade and the Democratic vote correlated significantly with the Democratic vote of the 1880s (as the fusion votes of 1896, 1898, and 1900 had not).

⁴⁷ Based on ecological regression involving 1890, 1900, and 1902 South Dakota voting results; also see Sioux Falls (S.D.) *Argus-Leader*, November 6, 7, 8, 1902. Original Populists are here defined as those who voted the Independent ticket in 1890. Clearly, this is an incomplete measurement, for it provides no information concerning those who joined the People's Party in, say, 1894. This is only part of the difficulty in trying to measure the effect of antifusion legislation. The general question is the counterfactual one: how would things have been different if they had not been as they were? One major consequence of antifusion legislation of course could be what did *not* happen, as in those instances in which fusion was avoided. The common failure of election boards to report disaggregated partisan votes for fusion candidates, except for the partial Kansas case analyzed above, prevents a careful calculation of effects when fusion did take place. Ideally, that determination also requires consecutive fusion elections with low issue-salience, a condition that did not obtain in the 1890s. The 1892 Oregon contest thus assumes great significance in establishing the political importance of an antifusion ballot.

⁴⁸ Third parties did, of course, appear in subsequent years, but with the exception of the Socialists they were

GOP's opponents or encouraged attenuated new combinations, the same result obtained: the non-viability of third parties. A Populist explained the dynamics involved with these words: the law "practically disfranchises every citizen who does not happen to be a member of the party in power. . . . They are thus compelled to either lose their vote (as that expression is usually understood), or else unite in one organization. It would mean that there could be only two parties at one time."⁴⁹ Political realities, moreover, dictated that those two parties would be the existing major ones. Because the adoption of a new composite name left the Democratic name, with all of its appeal and tradition, to be used by minority factions as in Michigan in 1896, because of even more grotesque ballot complications under the laws of some states,⁵⁰ or merely because their greater national strength gave them an advantage in all electoral contests, the Democrats were ultimately able to insist successfully that the name "Democrat" be adopted by all fusionists. In Michigan, for example, the charade of maintaining three separate conventions ended in 1899, and in 1901 the DPSU became simply the Democratic Party. Similarly, in Washington the fusionists joined in a union convention in 1900 and agreed to the Democratic name. A Detroit newspaper alluded to this logical tendency of the antifusion law when it renamed the legislation "the law providing for the extinction and effacement of all parties but the Democratic and Republican."⁵¹

Antifusion legislation also undermined the People's Party by exacerbating the existing fratricidal split within the party between the middle-of-the-roads and fusionists. As one Indiana Populist immediately recognized of the antifusion law, "an element of discord has been introduced by the dominant party which is expected to rend the populists' ranks and remove all doubts from future con-

generally expressive rather than instrumental. Those with any great support were short-lived and often based on the appeal of a dominant personality, like the Roosevelt Progressives of 1912 or the La Follette Progressives of 1924. Certainly, such parties rarely had, over time, the characteristics of late nineteenth-century third parties: local organization, voter identification, mass support in some areas and generalized regional strength, and especially tangible electoral success.

⁴⁹ *Kalamazoo Weekly Telegraph*, March 20, 1895.

⁵⁰ Following the passage of North Dakota's antifusion law in 1897, Independents (Populists) and Democrats, each opposing incorporation under the other's banner, combined into a new organization and adopted the title Independent-Democratic Party "as a party name . . . under which both Democrats and Populists can fight." But the imaginative Republican secretary of state interfered with this new-style fusion by ruling that the candidates of the Independent-Democratic Party could not be permitted on the Australian ballot at all, for such a party had not received the legal minimum of 5 percent of the vote in the preceding election—when, of course, it had not yet existed. Furthermore, he ruled, since the separate Independent and Democratic Parties had formed a new party, they had ceased to exist themselves and therefore could not *regain* a ballot position, leaving the Republicans the only party on the ballot. See *Winterset (Iowa) Review*, March 31, 1897; *Bismarck (N.D.) Daily Tribune*, October 28, 31, November 2, 1898; and *State v. Falley (N.D.)*, 76 N.W. Rep. 996. This action seemed to answer an earlier Populist who wondered, after the passage of an antifusion law, "why [the legislature] did not go on a little further and say there shall be but one ticket allowed on the ballot, and that must be the Republican ticket." Des Moines *Farmers Tribune*, March 17, 1897. For similar comments, see *Kalamazoo Weekly Telegraph*, March 20, 1895; and *Spokane Spokesman-Review*, August 18, 1896. Wyoming simply prevented the creation of any new-style fusions such as the DPSU or the Independent-Democratic Party by adding to its antifusion ballot amendment a requirement that the names of political parties not exceed *one* word. New York State Library Bulletin, *Summary of Legislation*, 519.

⁵¹ *Detroit Evening News*, March 20, 1895; Arthur Millsbaugh, *Party Organization and Machinery in Michigan since 1890* (Baltimore, 1917), 19, 55; *Spokane Spokesman-Review*, August 30, 1900; and Des Moines *Iowa State Register*, May 13, June 24, 1897.

tests."⁵² The usual mid-road arguments against fusion, based on the necessity of maintaining the party's identity and organization, acquired new and intense meaning in a legal situation that, as one judge phrased it, "says to the party, and through the party to the electors composing it: 'You shall not endorse candidates of any other party, except on condition that you surrender your existence as a party and lose your right of representation upon the official ballot in the future.'"⁵³ Antifusion legislation thus required those Populists interested in preserving their party's integrity to attack fusion ever more vigorously. As one Minnesota mid-roader noted, such laws would otherwise eliminate the Populists in every state where they did not outnumber the Democrats and thereby end any semblance of a national People's Party. But fusionist Populists countered that the mid-road position, in combination with the Republican "ballot law plot," would itself "divide and disfranchise populists and aid the monopoly and gold standard power." They argued that third parties had had their practical importance primarily as members of fusion coalitions and that "fusion, in the manner it has been had before on the official ballot, is no longer a possibility."⁵⁴ The logic of their position, then, required fusionists to merge the People's Party into the Democratic ranks. Limited to a ballot choice between Democrats and Republicans, some Populists voted Republican while others dropped out. The mid-roaders, though legally a bolting minority, issued their own Populist ticket, which after lengthy court battles between the two Populist factions invariably failed to attract enough support to guarantee the party a position on the ballot in the future.⁵⁵ In either case the People's Party ceased to exist.

IN SOME MEASURE, THEN, the People's Party died not only from prosperity and psychic collapse but also from ballot restrictions deliberately imposed by partisan legislatures in a movement that cannot accurately be said to have "scrupulously" preserved "all the forms of political democracy."⁵⁶ Some Populists vigorously attempted to amend the ballot laws, but others, recognizing that the Australian ballot itself had opened politics to "the dictation of state authority," argued for its repeal. "If 'amendment' is insisted upon," wrote one Iowa Populist, "let it be in the style of the farmer who amended his worthless dog's tail by letting the cleaver fall just behind the cur's ears."⁵⁷ Other Populists in antifusion

⁵² Quoted in *Des Moines Farmers Tribune*, March 17, 1897.

⁵³ *State v. Anderson* (Wisc.), 76 N.W. Rep. 482.

⁵⁴ *Des Moines Farmers Tribune*, March 17, June 23, 1897. Also see O. D. Jones to Marion Butler, April 21, 1897, Butler Papers.

⁵⁵ In particular, see the Iowa experience in the *Winterset (Iowa) Review*, July 28, September 9, 1897; *Des Moines Farmers Tribune*, June 30, September 15, November 10, 1897; *Des Moines Iowa State Register*, August 19, 20, September 4, 8, 1897; and *Dubuque (Iowa) Herald*, October 17, 22, 28, 1897.

⁵⁶ Also see Burnham's larger statement that it "is difficult to avoid the impression that while all the forms of political democracy were more or less scrupulously preserved, the functional result of the 'system of 1896' was the conversion of a fairly democratic regime into a rather broadly based oligarchy." Burnham, "Changing Shape of the American Political Universe," 23.

⁵⁷ *Sioux City (Iowa) Journal*, May 26, 1897; and *Des Moines Farmers Tribune*, February 17, 1897, January 5, 1898.

states began to push for electoral change to protect the existence of third parties: proportional representation.⁵⁸ That those efforts failed is hardly surprising.

Nor did the effects of antifusion legislation end with the destruction of the People's Party. Obviously, these laws contributed to the widely observed decline in party competition in the "system of 1896." It is reasonable to assume, moreover, that demoralized former Populists, whether they were forced into the Democratic or Republican Party, became more "peripheral" than "core" supporters of their new parties and were less likely to vote and, when they did, were more likely to engage in the split-ticket, drop-off, and roll-off tendencies characteristic of the electorate after the crisis of the 1890s.

Certainly, antifusion laws were not solely or even primarily responsible for those tendencies, which appeared throughout the political system. But the time has surely come to discard the notion that political effects were "unintended consequences" of nonpartisan institutional reforms. Such alterations in electoral law must be viewed within a larger political context and not treated as "uncaused causes" of the transformation of voting behavior.⁵⁹ These laws were enacted by politicians who deliberately sought to protect or advance their own interests by manipulating the rules of the game. That their interests corresponded in some respects to decreased political participation, particularly by the more democratic elements of the population, and a consequent circumscription of public policy only adds to the poignancy of the process. Obviously, it is not true that electoral "reform," as one political scientist has claimed, "ended the earlier party practice of using the institutional framework for its own benefit."⁶⁰ Indeed, antifusion laws, as one dissident observed in 1895, were "a step toward making the Australian ballot system a means for the repression instead of the expression of the will of the people."⁶¹ As for whether there was a "conspiracy," the Populists, who have often been charged with paranoia and conspiracy-mindedness, might have appreciated today's graffiti—"even paranoids have real enemies."

⁵⁸ William E. Lyons, "Populism in Pennsylvania, 1892-1901," *Pennsylvania History*, 32 (1965): 55; and *Bowling Green (Ohio) Daily Sentinel*, August 12, 1897.

⁵⁹ Burnham, "Rejoinder to 'Comments' by Converse and Rusk," 1054.

⁶⁰ Rusk, "Comment: The American Electoral Universe," 1049.

⁶¹ *Kalamazoo Weekly Telegraph*, March 20, 1895.

Cicero and Tacitus in Sixteenth-Century France

J. H. M. SALMON

IN EARLY SIXTEENTH-CENTURY FRANCE Cicero seemed a perfect model to humanists who challenged scholasticism through a rhetoric embodying both philosophy and history. Admiration for Ciceronian style was accompanied by a moralizing civic humanism and a respect for Cicero, the philosopher, as the purveyor of Greek wisdom. At the end of the century Tacitus had become a more important linguistic influence, while the ideal of the active citizen and virtuous orator had been replaced by one of Stoic fortitude and withdrawal. Tacitus, the historian of the corruption of liberty, emerged as the exemplar of private and public prudence, and a reinterpreted Cicero was relegated to the role of a minor precursor in prudential morality. This parallel shift in linguistic structures and moral ideologies did not simply result from the stresses of the religious wars in the last third of the century. It was also, in part, a long-term consequence of the earlier importation from Italy of a debate about the extent to which Cicero should be imitated—a debate that attained a new dimension in the context of the Reformation. Other elements that contributed to the demise of Ciceronian humanism were not only the relativist implications in the new historical approach to law but also the logic of Peter Ramus, who, while pleading for the union of eloquence and philosophy, effectively disjoined rhetoric from its component parts. As the neo-Stoic movement developed in reaction to rival religious enthusiasms, the literary models of Tacitus and Seneca invaded Latinity and left their mark upon the vernacular.¹ In the new climate of absolutist politics Cicero, *pater eloquentiae*, yielded place to Tacitus, *pater prudentiae*.

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¹ There is no easy way to find logical connections between the esthetics of style and the shifting ideological assumptions of the French Renaissance. The Constance school of *Rezeptionsgeschichte und Rezeptionsästhetik* denies synchronic value and meaning to a literary work and proceeds relativistically to place it in dialectical relationship with the chain of works that preceded and followed it as well as with the interpretations of successive generations of readers. The extent to which a work alters the threshold of the reader's expectation provides the criterion for esthetic appraisal; see Hans Robert Jauss, *Kleine Apologie der ästhetischen Erfahrung* (Constance, 1972), and *Literaturgeschichte als Provokation* (Frankfurt-am-Main, 1974). As its practitioners have admitted, there are problems in applying this approach to a period when the literature of classical antiquity was regarded as an absolute standard for imitation. Nevertheless, the very intensity with which a particular classical style is defended can provoke reaction when the image of its exemplar is altered to fit the changed moral attitudes of a new age. Therefore, I will use a complementary method that traces tensions between rhetoric and

This exercise in counterpoint is not intended to explain more than a limited number of elements in an exceedingly complex movement of modes and ideas.² Yet in these terms the juxtaposition of Cicero and Tacitus seems more suggestive than the many separate studies that have related Cicero to French humanism in the first two-thirds of the century and have traced connections between Tacitism and *raison d'état* after the religious wars.³ In particular, the use that is made of both Latin authors in the interval between these two periods is important for an understanding of the shift in emphasis from one to the other. It may also be that preoccupation with particular disciplines has impeded a grasp of the broader significance of the changing Renaissance view of the classical world. Concern with the role of Cicero in humanist scholarship has been more obvious in studies of Renaissance rhetoric and philosophy than it has been in surveys of attitudes to history and politics. The reverse has been true of modern works on the influence of Tacitus, where his relationship to various strands of Machiavellism has seemed of primary importance. Moreover, it is easy to find in Livy a historical counterpart to Ciceronianism and in Seneca a philosophical counterpart to Tacitism. The apparent connections between the stylistic revolution and neo-Stoicism have been tentatively explored,⁴ and several scholars have examined the relative popularity of Livy, Polybius, Tacitus, and other classical historians during the Renaissance.⁵ What is attempted here is, on the one hand, more ambitious in its scope and, on the other, more eclectic in its exemplification.

MISCONCEPTIONS BORN FROM THE DESIRE TO SIMPLIFY readily invade discussion of literary mode. Cicero's style is by no means as monolithic as the *Ciceronianus* debate might at times suggest. The easy discursiveness of the *Epistolae ad familiares* contrasts markedly with the balanced rhythms of *De oratore*. Within the

moral philosophy and links changes in linguistic structure to shifting moral priorities. Unconscious and emotive assumptions preceding rhetorical utterance can be seen in such circumstances to affect form and content in history and political thought. Generic changes are likely to occur both in periods of political crisis and in their aftermath, when analytic thought achieves new insights and rhetoric becomes formalized. These generalizations apply to Greece in the ages of Pericles, Thucydides, and Demosthenes and to Rome in the last throes of republicanism. They have been shown to be particularly relevant to Florentine humanism in the fifteenth century; see Nancy S. Struever, *The Language of History in the Renaissance: Rhetoric and Historical Consciousness in Florentine Humanism* (Princeton, 1970). Here I have applied them to sixteenth-century France.

² Attempts to capture this complexity in full may be self-defeating; see, for example, Jean Jehasse, *La Renaissance de la critique: L'Essor de l'humanisme érudit de 1560 à 1614* (Lyon, 1976).

³ André Stegmann, "Un thème majeur du second humanisme français, 1540-1570: L'Orateur et le citoyen," in Peter Sharratt, ed., *French Renaissance Studies, 1540-1570: Humanism and the Encyclopedia* (Edinburgh, 1976), 213-33. On Tacitism, see Else-Lilly Etter, *Tacitus in der Geistesgeschichte des 16. und 17. Jahrhunderts* (Basel, 1966); Jürgen von Stackelberg, *Tacitus in der Romania* (Tübingen, 1960); André Stegmann, "Le Tacitisme," in his *Machiavellismo e Antimachiavellismo nel Cinquecento* (Florence, 1969), 117-30; Etienne Thuau, *Raison d'état et pensée politique à l'époque de Richelieu* (Paris, 1966), 32-54; and Giuseppe Toffanin, *Machiavelli e il 'Tacitismo'* (Padua, 1921). For a recent survey, wider in period than its title suggests, see Kenneth C. Schellhase, *Tacitus in Renaissance Political Thought* (Chicago, 1976).

⁴ Morris W. Croll, *Style, Rhetoric, and Rhythm*, ed. J. Max Patrick and Robert O. Evans (Princeton, 1966).

⁵ Peter Burke, "A Survey of the Popularity of Ancient Historians, 1450-1700," *History and Theory*, 2 (1966): 135-52; Arnaldo Momigliano, "Polybius' Reappearance in Western Europe," in Olivier Reverdin, ed., *Polybe* (Geneva, 1974), 347-72; and J. H. Whitfield, "Livy > Tacitus," in R. R. Bolgar, ed., *Classical Influences on European Culture, A.D. 1500-1700* (Cambridge, 1976), 281-93.

speeches themselves one should distinguish between the rich Asianism of *Pro Milone*, the middle style of Attic antithesis in *Pro Caelio*, and the "lean and energetic" vigor of the *Philippics*.⁶ Like Cicero, Tacitus knew that the style he adopted for history was quite unsuited to rhetoric. In the sixteenth century some Tacitean scholars refused to accept Tacitus as the author of *Dialogus de oratoribus* because it seemed so out of character with the Tacitus they knew.⁷ Trained as an orator in his youth, Tacitus was just as concerned about the decay of eloquence since Cicero's age as he was about the decline of history in the sycophantic climate of imperial authority. Yet Tacitus, in his view of both oratory and history, alternated between indignation against the process of corruption and decay and acceptance of change for its own sake: Aper, one of the characters in the *Dialogus*, criticizes Cicero and argues also that all things are relative and each generation has its own fashions.⁸

Cicero's opinions on history were even better known than those of Tacitus on rhetoric. In *De oratore* he called upon the historian to tell the truth, the whole truth, and nothing but the truth. No one was better qualified than the orator to light the lamp of the past as a guide through present darkness, and yet the style of history was not that of rhetoric.⁹ In *Brutus*, his history of Roman oratory, Cicero praised the simplicity of Caesar's *Commentaries*: "They are bare, simple and straightforward, and devoid of all ornament. But, while Caesar wished to preserve the materials from which others might write history, he did a service for those clumsy writers who burn holes in them by trying to twist them into new shapes with curling tongs. So great is his achievement that it deterred men of good sense from writing, for there is nothing in history more agreeable than pure and shining brevity."¹⁰ Although Jean Bodin and Montaigne cited this passage, they took less account of Cicero's description of how other kinds of history should be written, notably the dramatic account of a single event like Salust's *Conspiracy of Catiline* and the high-flown Asianism of later annalist technique, a form that Livy perfected. Together with Lucian's *De scribenda historia* and the appraisal of Thucydides by Dionysius of Halicarnassus, Cicero's remarks on history in his three mature works on rhetoric formed the *loci classici* for Renaissance scholars who sought the views of the ancients on history and its style.¹¹ Cicero wrote no history, but few were ignorant of the privileged place he ascribed to it. Without history, men would remain forever children.

Three paradoxes within the Ciceronian corpus were transmitted unrealised to the humanism of the Renaissance. First, although prudence was itself a traditional virtue, tempering the ideal to the practical demands of circumstances and

⁶ R. G. M. Nisbet, "The Speeches," in T. A. Dorey, ed., *Cicero* (London, 1965), 47-49.

⁷ Tacitus, *Dialogue des orateurs: Dialogus de oratoribus*, ed. Henri Goelzer (Paris, 1947), 4.

⁸ *Ibid.*, xvii.

⁹ Cicero, *De oratore libri tres*, ed. Augustus S. Wilkins (Amsterdam, 1962), 2: 36 (p. 245), 62 (p. 258). Also See Pierre Boyance, *Études sur l'humanisme cicéronien* (Brussels, 1970), 136.

¹⁰ Cicero, *M. Tulli Ciceronis Brutus*, ed. A. E. Douglas (Oxford, 1966), 262, 75.

¹¹ Michel Montaigne, *Essais*, ed. Maurice Rat, 2 vols., 1 (Paris, 1962): 458; and Bodin, *Method for the Easy Comprehension of History*, trans. Beatrice Reynolds (New York, 1969), 56; and Claude-Gilbert Dubois, *La Conception de l'histoire en France au XVI^e siècle* (Paris, 1977), 72.

advocating the expedient, as Cicero did in the second book of *De officiis*,¹² weakened the morality of the common good. Second, behind the active role that Cicero for the most part commended to the citizen lay the alternative concept of the contemplative life, which pervaded his philosophical works at the end of his career and also appeared in the Stoic elements of *De officiis*, written in the same period. Third, history could not simply teach virtue if it also maintained the standards of objective truth that Cicero recommended. To later historians it was apparent from his letters to Atticus that Cicero had himself at times allowed ambition to bend the path of duty. The methods of the legal humanists involved historical relativism, and they valued Cicero's speeches and letters as sources for the science of philology and the understanding of Roman law in the particular society in which it had reached maturity. Even so, it seldom occurred to them that this society had not always lived up to Cicero's ideals.

There was a fourth area in which vision was restricted. Although, as philologists, the legal humanists were sensitive to neologisms and changes in grammatical structure, they often judged them by fixed standards of eloquence. Thus Lorenzo Valla, at once the progenitor of legal humanist method and the severest critic of Latinity, was more indignant at the barbarisms in the language of Bartolus than he was at the great commentator's lack of historicity. Yet Valla, no slavish admirer of Cicero, preferred Quintilian in matters of style, although Quintilian was himself generous in Cicero's praise. The wide popularity in France of Valla's *Elegantiae linguae latinae* contributed to the controversy aroused by the *Ciceronianus* of Erasmus.¹³ Valla felt no attraction to the style of Tacitus, who in any event was little known in the fifteenth century.

The first legal humanist to refer favorably to Tacitus in this respect was Andrea Alciato, and even he once described the historian's prose as "a thicket of thorns." Alciato published his edition of Tacitus's works in 1517, declaring him to be the best of Roman historians. Moreover, he saw in Tacitus a guide to conduct in the face of evil princes, advocating a private prudence that complemented the concept of prudence Guillaume Budé found in Cicero. For all this, it is doubtful whether Alciato, when he began to teach at Bourges in 1529, brought with him any enthusiasm for Tacitus except as an important source for the history of Roman law, and even there he made far greater use of Cicero. Alciato's mentor, Budé, who embodied all the paradoxes of civic humanism in its French guise, had no time at all for Tacitus, calling him "omnium scriptorum perditissimus" in the light of his denunciation by the Church Fathers, Orosius and Tertullian.¹⁴ Budé admired Cicero without imitating his style and

¹² In the third book of *De officiis* Cicero attempted to reconcile *utile* with *honestum*. Most modern commentators have assumed that Cicero subordinated *utile* to the common good, which is in itself a moral concept. A recent interpretation, however, reverses these priorities. Whether or not this was Cicero's intention, it was certainly seen this way by some Renaissance writers. See Maria L. Colish, "Cicero's *De Officiis* and Machiavelli's *Prince*," *Sixteenth-Century Journal*, 9 (1978): 81-93; and pages 323-24, below.

¹³ Donald R. Kelley, *Foundations of Modern Historical Scholarship* (New York, 1970), 26. Many of the Paris editions of *Elegantiae* were produced by Budé's publisher, Josse Bade. On the role of Valla's textbook in the *Ciceronianus* debate, see Augustin Renaudet, *Erasme et l'Italie* (Geneva, 1954), 203.

¹⁴ Peter Burke, "Tacitism," in T. A. Dorey, ed., *Tacitus* (London, 1969), 149; and Schellhase, *Tacitus in Renaissance Political Thought*, 87.

felt that, in introducing Greek philosophy to the Romans, Cicero had anticipated his own vocation in respect of the French. Budé filled his *Commentarii linguae graecae* with Ciceronian citations.

In his *Institution du Prince*, addressed to Francis I, Budé used Cicero to associate rhetoric and history, observing that "the father of Latin eloquence" had described history as the "witness of time, light of truth, mistress of human life, and messenger of antiquity." Budé recommended that the king make history his "great mistress."¹⁵ It was a mirror in which the present reflected the past:

By its consideration men can greatly acquire prudence and better deal with affairs at hand in the counsels of princes, as we see happening every day. There is nothing more important for men of discretion than to understand the state of the world and condition of human nature and to foresee what may come to pass and how to provide for it or forestall it. This is what historians teach. Prudence is acquired in this way by the man of good judgment in reading and reflecting about the past and present government of the world and how all kingdoms and great monarchies have met their end and by what shortcomings they have fallen into difficulty or by what means they have long been preserved and maintained in power and prosperity. . . . Prudence comes for the most part from experience and from observing those examples in the past for which history serves as register.¹⁶

In the preceding passage Budé had explained how rhetoric required knowledge of history, together with "a style graceful through nature and ready invention, and the discretion and prudence to adapt what is said to the audience and the circumstances of the occasion."¹⁷ Prudence in rhetoric was the ability to take account of the particular, and prudence in history was to understand the particularity of events and to apply to them general rules of human behavior in the interest of the public weal. It was this aspect of Cicero that was seized upon and distorted later in the century, when Ciceronian style went out of fashion and the Tacitean mode gained ascendancy. Budé represented the type of early French humanism that accepted authority in the prince and preached virtue in the citizen. Rhetoric, philosophy, and history were associated means to this end, and the works of Cicero seemed its perfect instrument. Tacitus, on the other hand, was distrusted and little known in France at this time.

THE PUBLICATION of Erasmus's satirical dialogue, *Ciceronianus*, in 1528 exposed the tensions within the French version of civic humanism. It was not, of course, the initiation of a new controversy. Following Valla's reservations about Cicero in fifteenth-century Italy and his exchanges on that subject with Poggio Bracciolini, Poliziano had advised the reader to choose from many models, whereas his critic, Cortesi, insisted upon the stylistic pre-eminence of Cicero. Then Giovanni Pico della Mirandola had taken a stand similar to Poliziano's, while Barbaro had followed Cortesi. In the next generation the younger Pico, Gianfran-

¹⁵ Budé, *Institution du Prince* (1524), ed. Claude Bontems, *Le Prince dans la France des XVI^e et XVII^e siècles* (Paris, 1965), 87. This passage is a direct translation of Cicero; see his *De oratore*, 2: 36 (p. 245)—see note 9, above.

¹⁶ Budé, *Institution du Prince*, 90–91.

¹⁷ *Ibid.*, 89.

cesco, supported his uncle, and Bembo earned a reputation as the archetypal Ciceronian.¹⁸ In these debates, form and content received different priorities, and rhetoric drew apart from philosophy. The *Ciceronianus* transferred the quarrel to France at a time of religious crisis. Perhaps Erasmus, after his editing of Jerome, recalled the dream in which the saint found his Christianity at odds with his worship of Cicero.¹⁹ In his preface Erasmus turned a northern knife in an open wound. "What a loss to scholarship," he wrote, "if one began by saying that Cicero alone should be imitated. I suspect that under cover of his name another matter is at issue: I mean, to make us forget Christ and lead us back to paganism. But, for my part, no task is more necessary than to consecrate good letters to the glory of Christ, our Lord and Master." This was the same evangelism that had moved Erasmus to declare in his *Paraclesis* that the eloquence of Cicero was far less to be preferred than that of the gospel.²⁰

It seemed a gratuitous insult to couple this accusation in the preface with a slight upon the pious master of French humanism, Guillaume Budé, who twelve years earlier had extravagantly praised Erasmus's Greek New Testament to which the *Paraclesis* served as introduction. Erasmus's last letter to Budé in 1528 was a sad piece, denying any intended slur upon French learning. He wrote also to Louis de Berquin, the translator of Luther, to explain that, when the text of *Ciceronianus* had ranked the Parisian publisher, Josse Bade, before his patron Budé, the speaker had been the ridiculous arch-Ciceronian, Nosoponus, not Bulephorus, who represented Erasmus's own opinions in the dialogue.²¹ Circumstances destroyed the point of this apology. A few months later, in April 1529, the bigots of the *parlement* finally accomplished the execution of Berquin for heresy, and not long after that Budé issued his *Commentarii linguae graecae*, where Cicero, Greek philosophy, and Christian humanism were mingled without overt incongruity.

Erasmus, the editor of *De officiis* in 1507 and *Tusculanae disputationes* in 1523, did not intend to denigrate Cicero but merely to ridicule those fanatical worshippers who allowed no other standard of Latinity. While he was being denounced in France, Erasmus continued his amiable correspondence with the Italian Ciceronians, Bembo and Sadoletto, and dedicated his 1532 edition of St. Basil to the latter.²² The most vicious of his French critics was a then obscure Agenais physician named Jules-César Scaliger, whose two invectives against Erasmus, published in 1531 and 1537,²³ launched him upon his career as a scholar of international reputation. More important to the present theme was

¹⁸ Remigio Sabbadini, *Storia del Ciceronianismo* (Turin, 1885), 25–50; Eduard Norden, *Die antike Kunstprosa vom VI Jahrhundert V. Chr. bis in die Zeit der Renaissance*, 2 (Leipzig, 1898): 773–79; and Izora Scott, *Controversies over the Imitation of Cicero as a Model for Style* (New York, 1910), 10–22.

¹⁹ M. L. Clarke, "Non Hominis Nomen sed Eloquentiae," in Dorey, *Cicero*, 87.

²⁰ Erasmus, *De recta latini graecique sermonis pronuntiatione: Dialogus Ciceronianus* (Lyon, 1531), 150; and Emile V. Telle, *L'Erasmianus sive Ciceronianus d'Etienne Dolet* (Geneva, 1974), 32.

²¹ Erasmus, *La Correspondance d'Erasmus et de Guillaume Budé*, ed. Marie-Madeleine de la Garanderie (Paris, 1967), 263–64. For the links between Erasmus and Berquin, see Margaret Mann, *Erasmus et les débuts de la Réforme française* (Paris, 1934), 113–49.

²² Renaudet, *Erasmus et l'Italie*, 218.

²³ Scaliger, *Oratio pro M. Tullio Cicerone contra Erasmus Roterodamum* (Paris [1531]), and *Adversus Des. Erasmi . . . dialogum Ciceronianum oratio secunda* (Paris, 1537).

the attack of Etienne Dolet, whose burning ambition and nonconformist temperament involved him in one feud after another. Tainted with accusations of heresy, Dolet fled from Toulouse in 1534 after a brush with the judges of the *parlement*, whom he labeled indiscriminately as "robed vultures" and "superstitious Turks."²⁴ At Lyon he was welcomed under the sign of the phoenix and winged orb of the bookseller-printer Sébastien Gryphe, who published Dolet's two discourses against the magistrates. Gryphe had issued a new edition of the *Ciceronianus* corrected by Erasmus in 1531 and, in his anxiety to stir the profitable pot of controversy, had just printed *Cicero relegatus*, *Cicero revocatus*, two dialogues by Ortensio Landi, an Italian exile in France. Dolet had in his possession the manuscript of his *Commentarii linguae latinae*, a Ciceronian Latin counterpart to Budé's introduction to Greek letters. While Dolet went to Paris to obtain a privilege for his masterpiece (arriving there in the midst of the storm created by the posting of extremist Protestant placards), Gryphe began to set up the text of a new reply to Erasmus adapted from several entries in the *Commentarii*. It appeared in 1535 as *Dialogus de imitatione Ciceroniana adversus Desiderium Erasmum Roterodamum pro Christophoro Longolio*.²⁵

Longolius, or Longueil, was a Ciceronian protégé of Bembo much revered by Dolet. A new and posthumous edition of his works in 1526 may have provoked Erasmus to compose the *Ciceronianus*. His published letters had accused the sage of Rotterdam of Lutheranism and compared his scholarship unfavorably with that of Budé. Assuming Longueil to be the model for Nosoponus, Dolet inserted into his own dialogue Longueil's friend and Dolet's master at Padua, Simon de Neufville. Through the mouths of Villanovanus (Neufville) and his foil Morus (a deliberate travesty of Erasmus's friend, Sir Thomas More, then awaiting execution in the Tower of London), Dolet vilified the character and works of Erasmus. After the latter's death Dolet also responded to an Italian defender of the *Ciceronianus*, Floridus Sabinus, who, oddly enough, had been secretary to Alberto Pio, prince of Carpi, the anti-Erasmian. There was little but personal abuse in this polemic coupled with praise of Budé and Poliziano and a note of respect for the shade of Erasmus.²⁶

Dolet published his reply to Sabinus on the press he had recently established at Lyon, and in the following year, 1541, he issued his own edition of Valla's *Elegantiae*. It was almost as if Dolet, beset by foes among humanists of both camps as well as defenders of scholastic method, was trying to shelter beneath past authority. Scaliger pursued him with implacable hatred. Led by Jean Visagier and Gilbert Ducher, the circle of Latin poets also united against him, including Hubert Sussannée, the author of a Ciceronian dictionary, and Antonio de Gouvea, who later opposed the Ciceronianism of Ramus. Nor did Dolet find support from the legal humanists, with whom in his early years he had shown an affiliation. Jean de Boyssoné, the disciple of Budé and friend of Alciato who

²⁴ Marc Chassigne, *Etienne Dolet* (Paris, 1930), 56.

²⁵ Telle, *L'Erasmianus sive Ciceronianus d'Etienne Dolet*, 40-42.

²⁶ *Ibid.*, 35-36; and Dolet, *Stephani Doleti Galli Aurelli liber de imitatione Ciceroniana adversus Floridum Sabinum* (Lyon, 1540), 7, 54. Dolet apologized for calling Erasmus "delirans senex batavus."

had been expelled from his chair of Roman law because of accusations of heresy, was probably his protector at Toulouse. In his letters to Boyssoné, Dolet had denounced the Bartolists, and he had once written to Budé expressing the hope that he could study under Alciato.²⁷ But Alciato came to profess impatience with Valla's literary preoccupations, and Dolet in his last years sided with Valla, symbolizing the growing divorce between literary and legal humanism. While Alciato became more concerned with history than with philology and used literary texts purely to exemplify aspects of Roman society, Dolet turned into a grammarian who subordinated historical truth to literary excellence and Christian to pagan inspiration.

Dolet's device on the title page of the works he published as a printer in Lyon consisted of a hand holding an axe to trim the branches of a fallen tree. The punning motto read, "Scabra et impolita adamussim Dolo atque perpolio" ("accurately, do I hack and level rough and unpolished pieces"). His axe sought to lop off French humanism from its evangelical stem. Among the many classical works that issued from his press were Cicero's *Tusculans* and *Rhetorica*. As stories circulated about his unbelief, he put tongue in cheek and supported his vigorous denials by printing a book as bizarre as *Cato Christianus*, an attempt, dedicated to Sadoletto, to explain Christian doctrine with the aid of an archpagan moral reformer. Dolet also began to translate into French Cicero's *Epistolae ad familiares*, a task that, when completed by the historian Belleforest in 1565, was accompanied by the latter's warning to the reader that his predecessor had been "un imitateur trop sévère." Dolet's French edition of *Tusculans* contains a preface to Francis I pleading that his fellow printers in Lyon were falsely accusing him of heresy in order to eliminate a successful competitor. Nonetheless, Dolet's version of the text in which Cicero questioned the immortality of the soul in this same volume contains added ambiguity. Here, too, are remarks about Roman use of prudence in government and Greek association of prudence and rhetoric.²⁸ Dolet's attitude to Cicero suggests that Erasmus's reproach of paganism was not entirely wide of its mark. His involvement in the *Ciceronianus* debate reveals the tensions within the French humanism of the time. Erratic, perverse, and eternally enigmatic, Dolet met his cruel death in the Place Maubert in 1546, but it is not only for his martyrdom that he deserves to be remembered.

The year of Dolet's execution was a turning point in Ciceronian scholarship for another reason. In 1546 Pierre de la Ramée (Ramus) published *De studiis philosophiae et eloquentiae conjugendis*, his commentary on *Brutus* (*Brutinae quaestiones*), and *Dialecticae institutiones*, which bears the name on the title page of Omer Talon, Ramus's acolyte.²⁹ Ramus was in search of a single logical method that he hoped to apply to all of the disciplines of the liberal arts. Some of his

²⁷ Lucien Febvre, *Le Problème de l'incroyance au XVI^e siècle: La Religion de Rabelais* (1942; Paris, 1968), 51–61; and Chassigne, *Etienne Dolet*, 23–29.

²⁸ Cicero, *Les Epistres familières de Marc Tulle Cicéron père d'éloquence . . . traduites de Latin en François . . .*, trans. Dolet and F. de Belleforest (Paris, 1566), unpaginated advertisement au lecteur, and *Les Questions tusculanes de M. T. Cicéron: Oeuvre très utile et nécessaire pour résister à toute viciieuse passion d'esprit, et parvenir au mespris et contemnement de la mort* (Lyon, 1543), 2–20.

²⁹ Walter J. Ong, *Ramus—Method and the Decay of Dialogue: From the Art of Discourse to the Art of Reason* (Cambridge, Mass., 1958), 30.

inspiration was derived from Rodolphus Agricola's *De inventione dialectica*, a fifteenth-century treatise on the logic of disputation aimed at scholasticism. But, ultimately, Ramus's system probably stemmed from Cicero's remarks in *Brutus* on the traditional division of rhetoric into five parts: *inventio* (discovery of material or general arguments), *dispositio* (arrangement), *elocutio* (style), *pronuntiatio* or *actio* (delivery), and *memoria* (memory).³⁰

The new method consisted essentially in transferring the first two elements to logic and using them as a means of proceeding from the general to the specific by way of bifurcatory analysis. Only style and delivery remained to rhetoric, while recollection or narration dropped from consideration. Although Ramus might pretend to conjoin philosophy and rhetoric, and rhetoric appeared to be his starting point, he advanced to a position in which dialectical logic became the queen of the sciences and rhetoric was reduced to a matter of technique. Moving logic and dialectic back to philosophy thus recreated an essential element in the attitudes of the schoolmen. These changes reversed the priorities that the Ciceronian humanists held dear—for rhetoric was no longer the dominant discipline that could gather others beneath its roof—and Ramus accomplished it all in Cicero's name. In so doing, he changed the entire concept of the union of citizen and orator. As Ramus gradually refined his method, he altered his stance from edition to edition of his works until the dialectical system came to consist of four parts: invention, disposition, distribution, and definition (or reconstruction). So fixed in its final form did this structure become that in his lecture *De logicae definitione* Ramus declared that, whatever the part of the world in which men dwelt, whatever their customs and governments, and whatever the divisions of knowledge they chose to admit (*grammatici, rhetores, poetae, historici, arithmetici, geometrici, musici, astrologi, physici, ethici*), all were obliged to follow "the one law and reason postulated in the dialectic."³¹

Ramus's dramatic flair and arrogant single-mindedness provoked intense opposition from fellow Ciceronians and all of the followers of Aristotle, whom Ramus had denounced. The resistance within the Ciceronian movement to Dolet's attempt to isolate letters from other humane studies was carried over into the struggle to prevent Ramus from imposing upon them a method supposedly derived from Ciceronian premises. Antonio de Gouvea, one of Dolet's critics, played the major part in the commission that resulted in the royal edict of March 1544, which censured Ramus for "témérité, arrogance, et impudence." While officially suspended from public teaching in the years that followed, Ramus continued to compose works on Cicero and Quintilian that exemplified his system, including many commentaries on Cicero's speeches and philosophical treatises. After the advent of Henry II in 1547, the suspension was lifted, and Ramus, with strong support in the royal entourage, made new converts in the *parlement* and the university. His appointment as joint professor of eloquence and philosophy at the Collège Royal in 1551 enabled him to initiate institutional re-

³⁰ Ramus, *Brutinae quaestiones*, in his *Petri Rami praelectiones in Ciceronis orationes octo consulares*, ed. Freigius (Basel, 1575), 472. For a list of Cicero's references to the five divisions in *Brutus*, see Douglas, Introduction to *Brutus*, xxxi.

³¹ Ramus, *Scholae in liberales artes* (1569), ed. Walter J. Ong (Hildesheim, 1970), 30.

forms of the liberal arts and provoked new waves of opposition. Pierre Galland, the conservative rector of the university and an early critic of the *Brutinae quaestiones*, led the outcry, describing Ramus as "a harpy that fouls all that he touches" and "a viper vomiting forth floods of poison."³² Galland was followed by Ramus's most relentless enemy, Jacques Charpentier, who, together with his friend, Marc-Antoine Muret, criticized the *Dialecticae institutiones* and resisted the union of rhetoric and philosophy.

Unlike Dolet, Ramus was careful not to mix religious speculation with his commentaries on Cicero's philosophical works. When he lectured on the *Somnium Scipionis* and hinted, darkly enough, that the rest of Cicero's *Republic* was doubtless kept well hidden by superstitious men, he used the occasion not to discuss problems of the afterlife but to apply his practical method to physical science. The very nomenclature used in astronomy, he said, was the product of the scholastic mind and suited only for "altercationes scholasticae." But Ramus's colleague, Adrien Turnèbe, did not object to metaphysical speculation in his criticism of the logician's commentaries on Cicero's *De fato* and *De legibus* but rather to the distortions involved in the application of Ramus's interpretative system. In this context Turnèbe branded Ramus with the epithet *usuarius*, meaning that he had exploited Cicero to illustrate his method and had no proprietary claim to the Ciceronian canon. Ramus seemed close to asserting such a demand. After rereading the *Brutus* on famous orators, he published in 1557 a number of his lectures under the title *Ciceronianus*. Here he identified himself with the father of eloquence and was so contemptuous of earlier controversies and commentaries that he labeled them "patchwork quilts of rags" (*panniculorum centones*).³³

In the religious wars of the 1560s Ramus, a declared if irregular Calvinist, fled from Paris, leaving the field to his enemies. Meanwhile, Denis Lambin, successor to Turnèbe as professor of Greek at the Collège Royal in 1566, proceeded with his edition of Cicero's *Opera omnia* in unyielding opposition to Ramist influence. Henri Estienne, whose revised version of the Ciceronian lexicon composed by his father, Robert, was completed by Lambin's index in 1568, returned to the tradition of Poliziano and Erasmus and satirized Mario Nizzoli, the leader of the old-style Italian Ciceronians. Estienne also drew attention to the rising star of Justus Lipsius, who with Marc-Antoine Muret, Charpentier's friend, inaugurated the age of Tacitean scholarship. Even Ramus, for whom Estienne had no sympathy, anticipated Lipsius in some respects. His angular Latin style resembled that of the great antiquarian, and his view of the virtue of

³² Charles Waddington, *Ramus (Pierre de la Ramée): Sa vie, ses écrits, et ses opinions* (Paris, 1885), 50–52, 94. For a list of Ramus's commentaries, see Walter J. Ong, *Ramus and Talon Inventory* (Cambridge, Mass., 1958), 38. Several of these commentaries, together with *Brutinae quaestiones* and *De studiis philosophiae et eloquentiae coniugendis*, were published in 1575 in the posthumous collection edited by Freigius, Ramus's disciple at Fribourg; see note 30, above.

³³ Ramus, *Somnium Scipionis ex sexto libro de republica M. Tullii Ciceronis Petri Rami Veromandue praelectionibus explicatum* (Paris, 1546), 26v, *Ad Turnebi disputatio ad librum Ciceronis de fato adversus quemdam qui non solum logicus esse, verumetiam dialecticus haberi vult* (Paris, 1554), 27, and *P. Rami regii eloquentiae et philosophiae professoris Ciceronianus ad Carolum Lotharingum Cardinalem* (Paris, 1557), unpaginated dedication. For an account of the exchanges between Turnèbe and Ramus, see Ong, *Ramus and Talon Inventory*, 289–94.

prudence in the orator touched, as Budé had formerly done, upon a theme that Lipsius later expanded.³⁴ Ramus met his death in the massacre of St. Bartholomew's Day—legend has it by Charpentier's conniving—while Estienne continued a stylistic debate that became increasingly irrelevant. In the religious wars the burden of intellectual debate shifted from rhetoric and philosophy to politics and history, and Tacitus suddenly assumed a new dimension.

THE NEW TONE WAS EXPLICIT in Jean Bodin's *Methodus ad facilem cognitionem historiarum* in 1566: "It is true that on account of his unpolished manner of speaking Tacitus is usually repudiated by those who prefer the lighter trifles of grammarians to the more serious accounts of those who have spent the whole of their lives in public affairs." Bodin reproached Alciato for quibbling about the style of Tacitus and remarked that those who criticized Tacitus for plain writing were like Christians attacking Jerome for being a Ciceronian. In contrast to Ammianus Marcellinus, Tacitus had "maintained the dignity of Roman speech." He ranked with the great historians of antiquity—Polybius, Thucydides, Xenophon, and Caesar—for like them he revealed "the causes of things, the origins, the progress, the inclinations, all the plans of everyone, the sayings and the deeds at their just weight." Influenced by Ramus and disillusioned with legal humanist attitudes, Bodin set out in his *Methodus* to deduce the principles of government from a comparativist approach to history. If he differed from Budé in this respect, he repeated Budé's observation that for men of affairs history was the essential means of acquiring political prudence. Cicero, however, did not appeal to Bodin. He accused Cicero of being digressive, refuted his belief in a government of mixed forms, and decried the republican and democratic values he discerned in Cicero's political thinking. "Cicero's definition of the state," he wrote, "as a group of men associated for the sake of living well indicates the best objective indeed, but it does not indicate the power and nature of the institution." He also assailed another Ciceronian account of the commonwealth as "the union of several associations under an approved law for a common advantage." Bodin's preference for strong monarchy led him to distort Tacitus's acceptance of the principate as the necessary consequence of the decay of republican virtue, and to see the historian as approving undivided authority by preference: "It is not only salutary, as Tacitus wrote, but also necessary in the administration of great affairs that power should rest entirely with one man."³⁵

Another admirer of Tacitus, and one who described him as "a good author, and as serious and reliable as any,"³⁶ was Montaigne's friend Etienne de la Boétie. His *Discours de la servitude volontaire* was completed about 1550 on the model of a classical indictment of tyranny, but it was not published until after the St. Bartholomew's Day Massacre, when it was pressed into the service of

³⁴ Jehasse, *La Renaissance de la critique*, 189–90, 330; Dubois, *Conception de l'histoire en France*, 250; and Ong, *Ramus—Method and the Decay of Dialogue*, 246. Henri Estienne's *Pseudo-Cicero* appeared in 1577, and his *Nizoliodiscultus* in 1578; see Scott, *Controversies over the Imitation of Cicero*, 105.

³⁵ Bodin, *Method for the Easy Comprehension of History*, 70, 83, 73, 17, 158, 272.

³⁶ La Boétie, *Discours de la servitude volontaire* (1574), ed. Paul Bonnefon (Paris, 1922), 83.

Huguenot propaganda against the monarchy.³⁷ Although he esteemed Tacitus as much as Bodin, La Boétie interpreted him quite differently and cited him in condemnation of the many crimes of the Julio-Claudian dynasty. Interestingly enough, the passage where he particularly praised the merits of the historian was as scathing of the *plebs sordida* as it was of Nero, who had so corrupted them that they mourned his death. A more celebrated work that used Tacitus against monarchical absolutism was François Hotman's *Francogallia*, composed largely in the late 1560s and first published in 1573. Yet among the greatest achievements of Hotman's early career were his massive textual commentaries on the orations and letters of Cicero. His major collection of observations upon Cicero's forensic pleadings had appeared as early as 1554. The stature of his scholarship in this respect may be judged from the pride of place given his comments in the Cologne compendium of Cicero's speeches in 1685 and his remarks on the *Epistolae ad Brutum et ad Quintum fratrem* in the composite edition of Gronovius in 1725.³⁸ Consciously or unconsciously, the political demands of the civil wars bent the objectivity of scholars. Hotman made selective use of both Cicero and Tacitus as allies in his cause.

The French situation resembled that in Germany at the beginning of the century, when humanists such as Conrad Celtis and Ulrich von Hutten enlisted Tacitus's *Germania* in the cause of patriotism and resurrected Arminius from the darkness of the Teutoberger Wald. The Alsatian humanist Beatus Rhenanus had imposed a more scholarly view with his edition of Tacitus's works in 1532, which surpassed even Alciato's in accuracy.³⁹ Now the French began to examine Tacitus for the light he might shed upon the political practices of their ancestors, the Franks, and such relevance as they might have for the sixteenth-century constitution. Although Hotman had made his name as a historian of Roman law, he came to see the tribal customs of the Germanic Franks as more important to Frenchmen than Roman jurisprudence. When Bodin was writing his *Methodus* in the interval between the civil wars of the 1560s, Hotman was preparing both his *Anti-Tribonian* and, with the help of the Beatus edition of Tacitus, his early outline of the history of the Francogallic constitution. The *Germania* was used by other historians and polemicists writing in this period, such as Etienne Pasquier and Jean du Tillet, although their views of French government were very different from Hotman's.⁴⁰ Their arguments were complicated by a debate about whether the Franks had been Teutons or migrant Celts and about the relative merits of Gaulois and Frankish customs. When these controversies were initiated in the previous decade Ramus had been a leading Gallicist and Charles du Moulin an early advocate of Germanism.⁴¹ Bodin and Hotman, respectively, continued these opposing traditions.

³⁷ The *Discours* first appeared in the second of the *Dialogi ab Eusebio Philadelphio* (or *Reveille-Matin*) in 1574 and was reprinted in the various editions of Simon Goulart's *Mémoires de l'estat de France sous Charles Neufiesme* (1576–79).

³⁸ Hotman, *Francogallia* by François Hotman (1573), ed. Ralph E. Giesey and J. H. M. Salmon (Cambridge, 1972), 38–52; and Cicero, *Marci Tullii Ciceronis orationum commentaria selecta* (Cologne, 1685), and *M. Tullii Ciceronis epistolarum ad Quintum fratrem et ad Brutum* (The Hague, 1725).

³⁹ Schellhase, *Tacitus in Renaissance Political Thought*, 37–47, 61–65.

⁴⁰ Hotman, *Francogallia*, 22.

⁴¹ *Ibid.*, 13; and Ramus, *Liber de moribus veterum Gallorum* (Paris, 1559).

When the *Francogallia* appeared in 1573 and went through massive revision and expansion in subsequent editions, the scholarly intentions of the draft begun in the 1560s took second place to the political needs of the later period. Tacitus and Cicero were invoked in tandem: the first to reveal the primitive Franks as the authors of liberty who elected their kings *ex nobilitate* and chose their war chiefs *ex virtute*; the second to extoll the merits of mixed government and repeatedly to stress the doctrine *salus populi suprema lex esto*, taken by Cicero from the Twelve Tables and described in *De legibus*.⁴² For all his Ciceronian scholarship, Hotman overlooked a vital reference that would have helped his case, the mention in *Epistolae ad Atticum* of the German Francones. The antiquarian Claude Fauchet took pride in pointing this out. He agreed with Hotman on the possible identification of the Franks with the Sicambrians of Batavia, but, more sympathetic to monarchical authority, he held the references of Tacitus to Germanic tribal kingship to be less restrictive on royal power than Hotman had claimed.⁴³

Other Huguenot theorists of resistance made less use of Tacitus than did Hotman, but they frequently cited Cicero. In *Du droit des magistrats* Théodore de Bèze referred to a passage that Seneca had quoted from the missing books of *De republica* to the effect that even under the early Roman kings final appeals could be made to popular assemblies. Similar use of this example was made in *Francogallia*.⁴⁴ Bèze drew the attention of his readers to a section describing tyranny in Cicero's *De officiis*, where it was held that in the last resort it was the duty of a son to accuse a father who threatened the safety of the state through tyranny. The author of *Vindiciae contra tyrannos* cited *De officiis* on many occasions to demonstrate that those in authority had to protect true religion, respect the law, and act for the welfare of the people who had placed them in office. As in *Du droit des magistrats* Cicero was invoked to greatest effect in defining the nature of tyranny. Rulers who betrayed their trust or broke their contract with those who had created them to govern became enemies of the people, committed treason against the commonwealth, and ought to be punished.⁴⁵ The *Discours politiques des diverses puissances*, a Huguenot tract rather less known than *Francogallia*, *Du droit des magistrats*, or *Vindiciae contra tyrannos*, seems to have been inspired by Ciceronian concepts in its attempt to adapt principles of civic humanism to secular resistance theory. Once again, *De officiis* was the work most often cited in the text.⁴⁶

⁴² Hotman, *Francogallia*, 208, 294, 296, 300, 342, 414, 450.

⁴³ Fauchet, *Oeuvres*, 1 (Paris, 1590): 29v, 30r.

⁴⁴ Bèze, *Du droit des magistrats* (1574), ed. Robert M. Kingdon (Geneva, 1971), 25; and Hotman, *Francogallia*, 300.

⁴⁵ Bèze, *Du droit des magistrats*, 47–48; and Cicero, *De officiis*, ed. H. A. Holden (Cambridge, 1899), 3: 23, 90 (p. 127). And [Mornay] *Vindiciae contra tyrannos* . . . *Stephani Junio Bruto Celta Auctore* (n.p., 1580), 28–29, 75, 105, 109, 121–22, 156, 179, 212, 215–16. Only one Ciceronian allusion in the *Vindiciae contra tyrannos* was not drawn from *De officiis* but came from Cicero's *Paradoxa Stoicorum* instead; *ibid.*, 179.

⁴⁶ *Discours politiques des diverses puissances establies de Dieu au monde* (1574) in Goulart's *Mémoires de l'estat de France sous Charles Neufiesme*, 3 ("Meidelbourg," 1579): 147v, 213r. For some definitions of tyranny from Cicero often employed in resistance theory, see *ibid.*, 165r, 167r (references to *Second Philippics*), 168v (*De officiis*, 1: 8, 26 [p. 12]), 177v (the passage from *De officiis* that Bèze cited in *Du droit des magistrats*; see *De officiis*, 3: 23, 90 [p. 127]; and note 45, above). The author also referred to Tacitus. I am indebted to Sarah H. Madden for drawing my attention to the Ciceronian tenor of this important and neglected tract.

Apart from Hotman, Huguenot writers generally confined their use of Tacitus to historical exemplification.⁴⁷ Cicero seemed an appropriate source for the definition of tyranny and Tacitus for its illustration. Those of moderate persuasion, who supported both royal authority and religious toleration, were even more inclined to see Tacitus's *Annales* and *Historiae* as useful sourcebooks, especially when they found themselves opposing alleged flatterers and Machiavellians around the throne. Such was the case with the Protestant magistrate Innocent Gentillet, who, while he was no monarchomach, shared the xenophobic Huguenot view that the crown had been corrupted by Italian influence in general and Machiavelli in particular. Gentillet's *Discours contre Machiavel* made much of flattery, deceit, and treason under Tiberius from Tacitus's *Annales*. He quoted Tacitus's version of the speech of the jurist Gaius Ateius Capito that the emperor should not pardon the technical treason of Lucius Ennius because the "laws desire that in such crimes of treason the least suspicion and appearance suffice to convict the accused, and it is in the great interest and utility of the state that one who has attempted anything, however slight, against the ruler should be rigorously punished."⁴⁸ Capito's words came close to genuinely Machiavellian reasoning, but Gentillet, who was so accustomed to distorting Machiavelli's precepts that he could not recognize a true instance of *raison d'état* when he saw it, was concerned only to criticize a piece of hypocritical flattery. Gentillet never accused Tacitus of being a precursor of Machiavelli. The Roman historian could still be seen in La Boétie's image of the exposé of tyrants, not the exponent of tyranny.

Jean Bodin, as we have seen, was an admirer of Tacitus not merely as a historian of causes and motives but also as an advocate of royal authority. His *Six livres de la République*, published in the same year as the *Discours contre Machiavel*, was also critical of Machiavellians and was aimed, besides, at the Huguenot theorists of resistance who themselves condemned the Florentine. At the beginning of his new book Bodin defined the commonwealth as "a lawful government of several families and what they hold in common, together with sovereign power." This sounded reminiscent of the definitions cited from Cicero in the *Methodus*, but the addition of sovereignty made all the difference in the world. With sovereignty described as absolute, with perpetual and indivisible law-making power located in the French crown, Bodin went one better than the monarchomach collective sovereignty of the people, exercised negatively to restrain or discipline the king.⁴⁹ Like Gentillet, Bodin saw no resemblance between Tacitus and Machiavelli, and he, too, used passages from the *Annales* without recognizing an affiliation with *The Prince*. Tiberius was the subject of several critical references in *Six livres de la République*, while Bodin alluded to Sejanus as an example of the useful royal technique of diverting discontent toward a ty-

⁴⁷ For example, see [Mornay] *Vindiciae contra tyrannos*, 141; and *Discours politiques*, 165r, 182v.

⁴⁸ Gentillet, *Discours contre Machiavel* (1576), ed. A. d'Ancrea and P. D. Stewart (Florence, 1974), 77; and Tacitus, *Annales*, 3: 70. For other relevant allusions to Tacitus, see Gentillet, *Discours contre Machiavel*, 98, 101, 102, 163, 197.

⁴⁹ J. H. M. Salmon, "Bodin and the Monarchomachs," in Horst Denzer, ed., *Verhandlungen der internationalen Bodin Tagung* (Munich, 1973), 359-78.

rannical minister.⁵⁰ In this passage Bodin did not hesitate to call Tiberius himself a tyrant, but he went on to cite Cicero, first, on the futility of abolishing the good with the bad in attempting to erase a tyrant's memory after his death and, second, on the dilemma confronting a member of a tyrant's council during the consideration of some profitable law. Tacitus was also called in on many occasions in *Six livres de la République* to sustain erudite references to such miscellaneous issues as Roman paternal authority, the chastity of the ancient Germans, and the Parthian custom of swearing alliances in blood. Except for a new rebuttal of the theory of mixed forms of government, Cicero was also employed more as a source of historical illustration than of evaluative judgment. His letters and speeches, together with *De officiis* and *De finibus*, were cited over thirty times on matters such as the family, citizenship, usury, and the role of magistrates.⁵¹ Bodin was too conscious of his own originality to require authorities to buttress his theoretical viewpoint. The architect of *politique* absolutism used Cicero and Tacitus indiscriminately as a grab-bag of information.

Bodin chose to write *Six livres de la République* in French, although he turned it into Latin in 1586, and the issues of Latin eloquence and literary style were irrelevant to his treatise. As more emphasis in political dialogue was placed upon the vernacular, a number of French translations of Tacitus and Cicero began to appear. Mode could still be important, however, to a translator. In 1575 Blaise de Vigenère published a composite volume in French containing part of Tacitus's *Germania*, the sixth book of Caesar's *Commentaries*, and Cicero's *De oratore*.⁵² His declared purpose in placing these three works between one set of covers was to show how a diversity of Latin styles could be faithfully represented in French. Vigenère, who had studied under Turnèbe and Dorat, also translated Livy and the dialogues on friendship of Plato, Cicero, and Lucian. Claude Guillemot published a complete French version of *Germania* in 1580, but it had evidently been prepared at the beginning of the Gaulois-Germanist controversy, for its dedication was dated 1551. The *Agricola* was translated in 1574 by the Huguenot lawyer Ange Cappel, who also rendered several of Seneca's works into French.⁵³ In 1582 Fauchet, the antiquarian, issued a complete translation of the

⁵⁰ Bodin, *Les Six livres de la République* (Geneva, 1577), 2: 5 (p. 392). In another passage Bodin did not hesitate to follow Tacitus in observing that Tiberius took away from the people "l'ombre de la liberté"; *ibid.*, 1: 6 (p. 83).

⁵¹ For Tacitus, see Bodin, *Six livres de la République*, 1: 4 (p. 47), 8 (p. 174), 5: 1 (p. 776); and, for Cicero, see *ibid.*, 1: 4 (p. 47), 2: 1 (pp. 338-39), 5: 2 (p. 802), 3: 5 (pp. 522-27).

⁵² Vigenère, comp., *Le Traicté de Cicéron de la meilleure forme d'orateurs, le sixième livre des commentaires de Caesar . . . , et la Germanie de Cornelius Tacitus* (Paris, 1575). After serving the duke of Nevers, Vigenère was employed in the French embassy in Rome and then in the office of one of the secretaries of state. He was a prolific author, and among his more curious works are treatises on comets, alchemy, and writing in cipher. Also see his translations of Plato *et al.*, *Trois dialogues de l'amitié: Le Lysis de Platon—le Laelius de Cicéron—et le Toxaris de Lucian* (Paris, 1579), and of Livy, *Les Décades*, 2 vols. (Paris, 1583).

⁵³ Tacitus, *Discours excellent auquel est contenu l'assiete de toute l'Alemaigne, les moeurs, coustumes, et façon de faire les habitants en icelle*, trans. Claude Guillemot (Paris, 1580), and *La Vie de Jules Agricola*, trans. Ange Cappel (n.p., n.d. [dedication dated 1574]). Cappel's brother Guillaume wrote a poem for the *Agricola* in honor of Ange and addressed it to Theodore de Bèze. Guillaume, who joined the Catholic League, was also a translator of Machiavelli. Another brother, Louis, was a professor of theology at the new University of Leyden, where he was a colleague of Lipsius. A fourth brother, Jacques, published a work in defense of Bartolus against Valla. Ange Cappel was an ardent reformer of legal procedures and a confidant of Sully. Descended from an eminent magistrate in the *parlement* of Paris, this extraordinary family seems to represent all of the intellectual threads incorporated in this paper.

works of Tacitus, incorporating parts of the *Annales* that Etienne de la Planche had already turned into French.⁵⁴ Among the more remarkable translators of Cicero at this time was the strange figure of Guy le Fèvre de la Boderie, remembered for his Syriac New Testament, his Chaldean grammar, his refutation of Islam, and his translations of Ficino and Giovanni Pico. Not surprisingly, he chose to publish Cicero's *De natura deorum* in French in 1581.⁵⁵

WHILE TACITUS WAS INCREASING IN POPULARITY among French readers and the theorists of resistance, together with their critics, managed to set him beside Cicero in support of their contentions, the two greatest Taciteans of the century—Marc-Antoine Muret and Joost Lips, Muretus and Lipsius—were laying the foundations for the triumph of their idol. Both were concerned with elegance and accuracy in Latin style; both began as Ciceronians and, after changing the focus of their attention to Tacitus, returned to Cicero with new insight; and both found the times in which Tacitus had lived a better analogue for the troubles the monarchies of Europe were experiencing in their own day than was the age of Cicero. It was in part because of their joint perception that a new literary mode was to be accompanied by a shift in the mode of political discourse.

As a youth Muret was an admirer of Erasmus's critic Jules-César Scaliger. His erudition and the brilliance of his own Latin style secured him lectureships at several French universities, including Paris, where he was a member of Charpentier's anti-Ramist group, and Bordeaux, where he may have tutored Montaigne. Charges of heresy and sodomy caused him to take refuge in Italy. At Venice in 1557 he dedicated to the doge a commentary on Cicero's speeches against Catiline, and adopted so ultra-Ciceronian a stance that he addressed Mocenigo, "It is extraordinary to see how these men, who in our own and our father's memory wanted to be called Ciceronians, could come out with so many barbarous expressions and defective constructions taken from the corrupt texts they consulted."⁵⁶ Typical of Muret's academic showmanship was his laying a trap for his rivals when they attended his lectures in the hope he would utter a phrase not in Cicero's canon. He put careful stress on a number of words that he knew to be contained in Cicero's works but that had been inadvertently omitted from the great lexicon prepared by Nizzoli. Muret was a professor in Rome for twenty-one years, until shortly before his death in 1585. When he began his series of lectures on Tacitus in 1580 his enthusiasm for his subject was so great

⁵⁴ Tacitus, *Les Oeuvres de C. Cornelius Tacitus*, trans. Claude Fauchet (Paris, 1582). *Le Dialogue des orateurs* was added to the edition of 1584–85. A third edition was published in 1594. Tacitus's *Annales* also entered France through Guicciardini's *Storia d'Italia*, which was deeply influenced by them. French editions of Guicciardini's history were published in 1568 and 1577; see Vincenzo Luciani, *Francesco Guicciardini e la fortuna dell'opera sua* (Florence, 1949), 32, 398. Guicciardini's *Ricordi*, containing allusions to Tacitus, appeared in French in 1576; see Guicciardini, *Plusieurs avis et conseils de François Guicciardin tant pour les affaires d'estat que privées* (Paris, 1576). I am indebted to Lionel A. McKenzie for this point.

⁵⁵ Cicero, *De la nature des dieux*, trans. Guy le Fèvre de la Boderie (Paris, 1581). In his Oriental interests and his religious syncretism La Boderie was a follower of Guillaume Postel.

⁵⁶ Muret, *M. Antonii Mureti ad Leonardum Mocenicum Patricium Venetum, orationum Ciceronis in Catalinam explicatio* (Venice, 1557), iii.

that he declared history to be the queen of the humanities.⁵⁷ He had a utilitarian history in mind, and Tacitus was its high priest. The lessons that Tacitus taught applied to the current age. While Muret was cautious enough to declare that Europe had no contemporary tyrants to compare with Tiberius, Caligula, and Nero as described in the *Annales*, he believed "it profitable for us to know how good and prudent men managed their lives under them, how and how far they tolerated and dissimulated their vices. . . . Those who do not know how to connive at such things not only bring themselves into danger but often make the very princes worse."⁵⁸ When the lectures on Tacitus had been completed, Muret turned back to Cicero and used *Epistolae ad Atticum* as his text. The Cicero that now emerged from the confidences imparted to his closest friend was a devious statesman in whom ambition vied with prudence. The only use for eloquence in modern times, Muret concluded, was in the writing of letters.⁵⁹

There is no space here to follow in detail the academic peregrinations of Lipsius from Jena to Leyden to Louvain or his religious apostasies among Lutheranism, Calvinism, and Catholicism. He was first known not only as a Ciceronian scholar but also as a supreme Ciceronian stylist. In later years he recalled his youthful enthusiasm and continuing, if modified, affection for Cicero in a piece of Latin that is almost a parody of the Tacitean mode: "Ciceronem amo. Olim etiam imitatus sum: alius mihi sensus nunc viro. Asianae dapes non ad meum gustum, Atticae dapes."⁶⁰ But, if Lipsius altered his estimate of Cicero's style, he remained consistent in his belief that Cicero's principal merit in politics was the advocacy of prudence. This was the virtue he commended when dedicating his first lectures on Cicero to Cardinal Granvelle before setting out for Rome in 1567.⁶¹ In Rome he met Muret and began his career of Tacitean scholarship, in which the concept of prudence developed into a kind of short-hand for political necessity, guarded by the moral reservations of the individual.

The two aspects of prudence, the private and the public, and the two fields of its exemplification, history and rhetoric, had been left disjointed by Budé. Now public and private prudence were directly associated, and politics replaced rhetoric as the second area in which prudence could be learned and applied. The connections had already been implicit in Cicero and the Stoic context of the orator's thought. The intellectual climate of the religious wars in France and the Netherlands had at first encouraged the application of Ciceronian rhetoric to political debate, but as the wars intensified the Ciceronian emphasis upon the active participation of the citizen no longer seemed appropriate. The help-

⁵⁷ Muret, *M. Antonii Mureti opera omnia*, 1 (Leyden, 1789): iii. For Muret's showmanship, also see J. E. Sandys, *A History of Classical Scholarship* (Cambridge, 1938), 150.

⁵⁸ Croll, *Style, Rhetoric, and Rhythm*, 152.

⁵⁹ Muret, *Mureti opera omnia*, oratio xvi, 318-25. Despite his new view of Cicero and his contempt for ultra-Ciceronians, Muret paid tribute to Sacoletto, Bembo, and Longueil in this lecture.

⁶⁰ "I love Cicero. Formerly I even imitated him. Now as a man I have other tastes. Asian feasts are not to my liking. Attic ones are." Lipsius, as quoted in Clarke, "Non Hominis Nomen sed Eloquentiae," 95. For brief outlines of Lipsius's life, together with comments upon his ideas, see V. A. Nordman, "Justus Lipsius als Geschichtsforscher und Geschichtslehrer," *Suomalainen tiedeakatemia Toimituksia (Annales Academiæ Fennicae)*, ser. B, 28 (1932): 1-101; Jason Lewis Saunderson, *Justus Lipsius: The Philosophy of Renaissance Stoicism* (New York, 1955); and Knud Banning, *Justus Lipsius* (Copenhagen, 1975).

⁶¹ Jehasse, *La Renaissance de la critique*, 208. Lipsius cited Cicero and Tacitus together on prudence; see Lipsius, *Politiconum sive civilis doctrinae libri sex* (Leyden, 1589), 212.

lessness of the individual in face of forces he could not control suggested resignation or withdrawal, and the tenets of Senecan neo-Stoicism provided the moral fortitude that enabled him to preserve his dignity and integrity. In this way neo-Stoicism conjoined the two kinds of prudence, encouraged the politics of necessity, and elevated history as the record of pragmatic realism and the source of individual consolation. The transformed Cicero still had a role to play in this new structure, but among classical authors Tacitus now occupied the center of the stage. Lipsius personified this intellectual shift and popularized the change in literary mode through which it was expressed. In France he had his counterparts or disciples in Montaigne, Guillaume du Vair, and Pierre Charron.

Despite his desire for scholarly withdrawal, Lipsius could not ignore the blood bath in his own country and across the borders to the south. When he lectured on Tacitus at Jena in the year of the St. Bartholomew's Day Massacre, he compared the Duke of Alba with Tiberius.⁶² Three years later appeared the first version of his masterly edition of the works of Tacitus, which he refined and improved in subsequent editions until his death in 1606. Also published in 1575, Lipsius's *Antiquae lectiones* confirmed the revision of his view of Cicero, although here he turned to Plautus and Seneca, rather than to Tacitus, for literary models. In the next decade came his neo-Stoic *De constantia* (1584) and his political handbook based extensively upon Tacitus, *Politiconum libri sex* (1589). A statement of expediency in the latter work—that the toleration of two religions inside one state was inconceivable and that one of them must be exterminated *ense et igne*—caused indignation at Leyden but was evidently ignored by Calvinists at La Rochelle, who printed a French translation there in 1590.⁶³ Lipsius continued to publish studies of Roman antiquities and earned equal rank with Isaac Casaubon and Joseph Scaliger in the scholarly "triumvirate" of the age, though Casaubon and Scaliger did not see eye to eye with Lipsius in his role of *Sospitator Taciti*. Casaubon denounced Tacitus as a specialist in shame and slighted his commentator by saying that anyone who thought Tacitus a guide to practical politics was either accusing kings of being tyrants or teaching them how to become so. Scaliger, with an acerbity reminiscent of his father, wrote that "Lipsius is the cause that many have now little respect for Cicero, whose style he esteems about as much as I do his own." Yet in literary matters Lipsius became something of a relativist. One of his most readable pieces was a satire, published in 1581, in which he described a dream where he witnessed the Roman senate prosecuting a group of audacious philologists who sought to correct ancient texts by their own standards.⁶⁴

⁶² Arnaldo Momigliano, "The First Political Commentary on Tacitus," in his *Essays in Ancient and Modern Historiography* (Oxford, 1977), 222–24; this essay was first published in the *Journal of Roman Studies*, 37 (1947): 91–101. The later version includes as an appendix a reprint of Momigliano's review of Jose Ruyschaert, *Juste Lipse et les Annales de Tacite* (Louvain, 1949), itself a valuable work on the editorial methods of Lipsius. In the review Momigliano discussed whether the oration in question was actually delivered by Lipsius at Jena in 1572 and pointed out that Lipsius also remarked in the oration that Tacitus was more relevant to his own times than were Sallust and Livy. Also see Jehasse, *La Renaissance de la critique*, 213.

⁶³ Lipsius, *Les six livres des politiques, ou Doctrine civile de Justus Lipsius*, trans. Charles le Ber sieur de Malassis (La Rochelle, 1590).

⁶⁴ Jehasse, *La Renaissance de la critique*, 394–96; Saunders, *Justus Lipsius*, 65; and J. Ijsewijn, "Neo-Latin Satire: *Sermo* and *Satira Menippeae*," in Bolgar, *Classical Influences on European Culture, A.D. 1500–1700*, 49.

BEFORE THE NEW CURRENT had become firmly established in France a curious echo of that phase of Huguenot resistance theory that had enrolled Cicero and Tacitus together under its banners sounded in the propaganda of the Catholic League. The most weighty ideological opponent of the last Valois and the first Bourbon kings was Jean Boucher, doctor of the Sorbonne and associate of the revolutionary group of the Sixteen. His *De justa Henrici Tertii abdicatione* appeared in 1589, just after the monarch it sought to depose had been assassinated by a Leaguer fanatic. To illustrate the nature of tyranny Boucher cited Tacitus's judgment of Sejanus together with some of the extracts from *De officiis*, *Pro Milone*, *Ad Brutum*, and *Tusculanae disputationes* that formed the corpus of humanist sources on the subject. From *De amicitia* he repeated Cicero's words on the climate of tyranny: "Under it there can be no trust, no love, no kind of enduring good will: suspicion and unease lurk everywhere, and there is no place for friendship."⁶⁵ This, Boucher maintained, was precisely that atmosphere surrounding Henry of Valois and his would-be successor, Henry of Navarre. Boucher's pseudonymous colleague, "Gulielmus Rossaeus," published a better reasoned and less personal treatise to the same effect: *De justa reipublicae Christianae in reges impios et haereticos* (1590). Rossaeus cited one of Cicero's letters to Atticus and, a little later, the second book of *De officiis*, to establish the difference between a king and a tyrant. Each reference to Cicero was associated with a quotation from Tacitus on the reign of Tiberius.⁶⁶ Another enemy of Navarre was the Leaguer publicist and *parlementaire* Louis Dorléans, whose humanist inclinations had been shaped by Dorat and whose first polemics for the League were written in the guise of "the Catholic Englishman." Like Boucher, Dorléans fled to the Netherlands when Henry IV recovered Paris in 1594. He returned in later years after making his submission, and he eventually issued his *Novae cogitationes in libros Annalium Taciti*. Here Tacitus was represented not as the critic of imperial tyranny but as the ultimate guide to prudent kingship. During his exile Dorléans had evidently become a disciple of Lipsius. Formerly, he argued, it was customary "to praise the declamation of Cicero, but Tacitus is more deserving in our age." To Dorléans Tacitus had become "a flower among authors and a prince among historians."⁶⁷

The first separate political commentary on Tacitus was published in Paris in 1581, three years before Navarre had become the next heir to the French throne and the League had been revived to oppose his claim. It was issued with the text of the first four books of the *Annales*, and its author was a Piedmontese, Carlo

⁶⁵ [Jean Boucher] *De iusta Henrici Tertii abdicatione e Francorum regno* (1589; Lyon, 1591), 258 (mispaginated as 266).

⁶⁶ Rossaeus, *De iusta reipub. Christianae in reges impios et haereticos auctoritate* (Paris, 1590), 17r, 18v, 22r, 22v. On the identity of Rossaeus, who used the pseudonym only in the 1592 Antwerp edition of the work, see J. H. M. Salmon, *Society in Crisis: France in the Sixteenth Century* (London, 1975), 274; and Frederic J. Baumgartner, *Radical Reactionaries: The Political Thought of the French Catholic League* (Geneva, 1975), 145-47. The use by Rossaeus of Cicero's *De officiis* is very like that of the French monarchomachs; see pages 318-19, above. He is probably directly indebted, however, to the Scottish monarchomach, George Buchanan, to whom he referred on several occasions with surprising respect. Rossaeus's citation from *De officiis* is the same as Buchanan's: Buchanan, *De iure regni apud Scotos* (1579; n.p., 1680), 28. And see Cicero, *De officiis*, 2: 12, 41 (p. 75).

⁶⁷ Dorléans *Novae cogitationes in libros Annalium Cornelii Taciti qui extant* (Paris, 1622), unpaginated dedication.

Pasquale.⁶⁸ He was a protégé of the statesman and man of letters, Guy du Faur de Pibrac, a widely respected popularizer of Seneca. Pasquale turned to Seneca in his preface to the reader when he wished to commend those who, as Tacitus had shown, knew how to respond to unexpected adversity and even to death. He used Seneca, too, to defend the enigmatic brevity of the historian's style, remarking that language responded to changes in public attitudes.⁶⁹ Lipsius was popularizing gnomic political utterances on the Tacitean model, and when Pasquale republished his commentary in 1600 he chose to retitile it *Gnomae seu axiomata politica ex Tacito*.⁷⁰

No single writer better illustrates the shift in moral attitudes, the new mode of discourse that displaced humanist rhetoric, and the altered relationship between Cicero and Tacitus than does Montaigne in the first two books of his *Essais*, which were published in 1580, and then in their revised form, together with the third book, in 1588. Montaigne developed his own early Stoicism through Seneca and Plutarch quite independently of Lipsius, and only after Lipsius praised his essays in 1583 did they begin to correspond. Indeed, apart from two mentions of Lipsius in the third book, Montaigne's acknowledged and unacknowledged debts to *Sospitator Taciti* were included only in the revisions of the essays made after 1588.⁷¹ By his own account Montaigne first paid close attention to Tacitus soon after the time that Bodin praised the historian in the *Methodus*. Nevertheless, most of the references to Tacitus in the essays of 1580, including even the passage on the death of Seneca, were indirectly acquired through Bodin's work and other sources. Montaigne was in Rome when Muret lectured on Tacitus in the winter of 1580–81. He reread Tacitus when composing the third book, and he offered his most important judgments upon him in "De l'art de conférer." He noted how Tacitus revealed the effects that the cruelty of the emperors produced upon their subjects, and declared that this kind of material offered more insights than standard accounts of battles and high matters of state: "This type of history is by far the most useful: public affairs depend more upon the whim of fortune, private ones upon our own conduct." Montaigne found more precepts and judgments than narration in Tacitus: "It is a seed-bed of ethical and political discourses for the use and ornament of those who have status in the management of the world."⁷² As Bodin and Muret had done, the essayist defended Tacitus for his adherence to the state religion. Like Lipsius, Montaigne held it to be the duty of a citizen to follow the

⁶⁸ Momigliano, "The First Political Commentary on Tacitus," 205–29.

⁶⁹ Pasquale, "Genus dicendi, ut ait Seneca, imitatur publicos mores," in Tacitus, *C. Cornelii Taciti Equitis Romani ab excessu Divi Augusti Annalium libri quatuor priores et in hos observationes Caroli Paschali cuneatis* (Paris, 1581), unpaginated preface to the reader.

⁷⁰ See Lipsius's own collection of Tacitean and other political axioms, *Iusti Lipsii monita et exempla politica libri duo, qui virtutes et vitia principum spectant* (Paris, 1605). Once again, *civilis prudentia* is represented as the governing concept in this volume.

⁷¹ The section that follows is heavily dependent on Pierre Villey's *Sources et l'évolution des Essais de Montaigne*, vol. 1: *Les Sources et la chronologie des essais* (Paris, 1908), 224–26 (Tacitus), 98–103 (Cicero), 214–16 (Seneca), 198–99 (Plutarch), 161–62 (Lipsius).

⁷² Montaigne, *Essais*, 2: 377–78 (3: 8). (Book and chapter numbers in these and subsequent references to Montaigne appear in parentheses.)

religion established by law, and, while he was more tolerant toward the individual conscience than was the scholar of Leyden, he agreed with Lipsius that lack of religious uniformity was an invitation to civil strife.

Montaigne developed a strong interest in the personality of Tacitus, and offered a few criticisms, largely in the revisions for the 1595 posthumous edition of the essays. Tacitus was said to conceal himself too much from the reader and even from himself. His judgment of individuals was occasionally at fault, and there were times when he wrongly preferred the rules of civility to frankness and truth. Nevertheless, he was "a great personage, upright and courageous, endowed not with a superstitious nature but philosophical and generous." The value of his history was never in doubt. No one else had managed to blend consideration of manners, attitudes, and particular motives with a record of public events. His opinions were sound, and he had taken the right side in Roman politics. His role had been "proper in a sick and troubled state, as ours is in the present age."⁷³

As it might be expected from Montaigne's humanist background, the essays abounded in references to Cicero, but, on nearly every occasion where Montaigne offered personal judgments in the versions of 1580 and 1588, they were of a negative sort. Montaigne admitted that Cicero deserved his reputation for eloquence, but that was not the author's concern in the essays. His main interest lay in moral ideas, and Cicero's were not to his taste. He made considerable use of such works as *De amicitia*, *De senectute*, and *De finibus*, but he apparently did not study Cicero's speeches or his treatises on rhetoric; Montaigne's references to such works were stock ones borrowed from other sources, including the *Dialogus de oratoribus* of Tacitus. As a man, Montaigne found Cicero vain and untrustworthy. His harshest comments, however, were reserved for Cicero's style, which he described as "lasche et ennuyeuse."⁷⁴ Cicero's work was filled with prefaces, definitions, and digressions without reaching the substance of his discourse. One could read him for an hour and come up with no more than a bag of wind. Montaigne despised both the subtleties of the grammarians and the logic-chopping of the Aristotelians. Cicero, he declared, might be suitable for the schoolroom or the bar—or, indeed, to the kind of sermon where one could go to sleep for fifteen minutes and awake to find the same thread of argument. Cicero's artifice negated the point he wished to make. If Montaigne wanted to hear a speech on love of liberty, he preferred Brutus to Cicero, and, if he wished to cultivate disdain for death, he read Seneca before Cicero.⁷⁵

After 1588, however, Montaigne's view of Cicero changed in the last respect. His developing interest in metaphysical questions led him to see Seneca and Cicero as complementary, and he made considerable use of Cicero's philosophical works, especially *Quaestiones academicae*, *Tusculanae disputationes*, *De natura deorum*, *De divinatione*, *De officiis*, and *De finibus*. Much of the scepticism of the *Apologie pour*

⁷³ *Ibid.*, 380, 378 (3: 8).

⁷⁴ *Ibid.*, 1: 454 (2: 10). (The 1595 version omits *lasche*.)

⁷⁵ *Ibid.*, 1: 455 (2: 10), 2: 121 (2: 31).

Raymond Sebond had dropped away, and Montaigne renewed his earlier interest in the tempered and resigned Stoicism of Seneca. There were aspects of Cicero, especially in *De finibus*, which could easily be reconciled with this stance, but Montaigne never varied his view of Cicero's rhetoric. Like Pasqualè, he remembered what Seneca had written on the adaptation of linguistic structure to the concerns and moral conventions of the age. His own opinions resembled the moral and political attitudes of Seneca and Tacitus, and his mode of discourse in the vernacular was the counterpart of the new Latinity of Lipsius.

Guillaume du Vair shared the attitudes of Montaigne and Lipsius on the inefficacy of the individual to affect the capricious twists of fortune, on the need for a resolute demeanour in personal conduct, and on the disastrous political consequences of religious innovation. He served in the *parlement* of the Catholic League, where his patriotism and his dislike of the revolutionary element in the Catholic faction moved him to oppose the suggestion that the Spanish Infanta should become queen of France. Du Vair composed his treatise on constancy during Henry IV's siege of Paris in 1590 and published it when the king finally entered his capital four years later. His work was inspired by Seneca and, more immediately, by Lipsius. Du Vair had written on Epictetus and on Stoic moral philosophy in general, but he was also an admirer of Cicero, whom he adapted to fit the neo-Stoic mood. The future keeper of the seals was the author of *De l'éloquence française*, where he provided Cicero's *Pro Milone* as an example for emulation, and explained the decay of French rhetoric in terms of the loss of the old Ciceronian ideal of the union of the orator and the citizen. Despite his lament about present evils and the powerlessness of the individual to hold back the tide, Du Vair continued to plead for the active life. In this vein he wrote *Exhortation à la vie civile*, which became attached to his tract on constancy. Moreover, he was an advocate of political prudence who despised a priori political theorists: "They want to reduce political government, which depends upon particular prudence, to the general rules of some universal science, and in applying rules in this way where they ought to be applying exceptions they have distorted judgment in everything."⁷⁶

Editions of Cicero were still being produced and read, based upon the Aldine versions, especially that of Paulus Manutius, and those of French scholars such as Charles Estienne and Denis Lambin.⁷⁷ Yet even in the editorial process a change was evident. In 1588 the jurist Denis Godefroy published at Lyon a revised text of Cicero's *Opera omnia* as prepared by Lambin. In his preface Godefroy criticized his predecessor for being too concerned with trivialities of language at the expense not only of philosophical issues but also of practical matters of politics and individual conduct. "I have always thought," he wrote, "that we should rather pay attention to what concerns the *mores* of our own

⁷⁶ Du Vair, *Traité de la constance et consolation es calamitez publiques*, ed. Jacques Flach and F. Funck-Bretano (Paris, 1915), 201. The other works mentioned are Du Vair's *De l'éloquence française* (Paris, 1590) and *La Sainte philosophie, la philosophie des Stoïques, manuel d'Epictète, civile conversation, et plusieurs autres traictés de piété* (Lyon, 1500). On Du Vair in general, see R. Radouant, *Guillaume du Vair: L'Homme et l'orateur* (Paris, 1909); and L. Zanta, *La Renaissance du Stoïcisme au XVI^e siècle* (Paris, 1914).

⁷⁷ Villey, *Sources et l'évolution des Essais de Montaigne*, 102-03.

country, family life, and government, than wear away childishly this fleeting, brief, and miserable life in wrestling with the forms of letters: it is more important to live well and wisely than to learn to speak too scrupulously and exactly." In consequence, Godefroy omitted many of Lambin's philological notes. Godefroy had been trained at Louvain as a Ramist and had later become a Calvinist. Immediately after editing Cicero, in his Genevan place of exile, he set to work on the textual emendation of Seneca's philosophical works, not by the comparison of manuscripts but rather by conjecture. Like his edition of the *Corpus juris civilis*, his edition of Cicero went through many reprintings in later generations. It was dedicated to Elector Frederick William IV, who became Godefroy's patron. He was invited by the jurist to pay less attention to examples of Cicero's elegance than to Cicero's practical precepts, which should be applied to government and the reformation of public morals.⁷⁸

Times were indeed changing. Montaigne and Pasquier both remarked that Ciceronian eloquence could only apply where the vulgar had power: it was irrelevant in France. The *Satyre Ménippée*, the Politique riposte to the quasi-democratic propaganda of the league, had its most serious spokesman, Daubray, refer to a page of Tacitean history and to Tacitus himself as "an evangelist to some."⁷⁹ There was a hint here that Tacitus could be a source of amoral counsel, which was also the view of Giovanni Botero, a Savoyard diplomat who had visited Paris in 1585. Botero's *Della Ragion di Stato* (1589) was the first work directly to associate Tacitus with Machiavelli, and it was published in French translation in 1599.⁸⁰ Such an opinion long remained uncommon in France. The Huguenot poet and historian, Théodore-Agrippe d'Aubigné, who upheld an intensely moral position in his later years, described Tacitus as *mon maître*.⁸¹ Scipione Ammirato invoked Tacitus to refute Machiavelli's republicanism and composed his *Discorsi sopra Cornelio Tacito* (1594) as a counter to Machiavelli's *Discourses on Livy*. Ammirato saw Tacitus as the supreme advocate of political prudence. Jean Baudouin, who provided a new French version of the works of Tacitus, also translated Ammirato's *Discorsi*. Evidently he did not expect his readers to be shocked by the Italian's personal explanation: "If I gave here precepts and lessons on how to dissimulate, it is sufficient for me to record the words of holy scripture where it is said that even God dissimulates the sins of men so that they may be converted."⁸²

Even if Machiavelli was still regarded with some suspicion in France, political prudence, soon to be linked with reason of state,⁸³ was becoming an accepted

⁷⁸ Cicero, *M. Tullii Ciceronis opera omnia, praeter hactenus vulgatam Dionysii Lambini editionem, accesserunt D. Gothofredi*, 1 (Lyon, 1588): unpaginated dedication and preface to the reader.

⁷⁹ *Ménippée*, 1 (Ratisbon, 1726): 146–47. On the background of the *Satyre Ménippée*, see J. H. M. Salmon, "French Satire in the Late Sixteenth Century," *Sixteenth-Century Journal*, 6 (1975): 57–88.

⁸⁰ Schellhase, *Tacitus in Renaissance Political Thought*, 124.

⁸¹ Thuau, *Raison d'état et pensée politique à l'époque de Richelieu*, 35; and Tacitus, *Les Oeuvres de C. Corn. Tacitus . . . avec des discours politiques* by Ammirato, ed. and trans. Jean Baudouin (Paris, 1628), *Discours* (separately paginated), 10. On Ammirato, see Schellhase, *Tacitus in Renaissance Political Thought*, 142–45.

⁸² Botero, *Raison et gouvernement d'état* (Paris, 1599).

⁸³ On the affiliation between such terms as prudence, interest, and reason of state, see Anna Maria Battista, "Morale 'privée' et utilitarisme politique en France au XVII^e siècle," in Roman Schnur, ed., *Staatsräson: Studien*

part of the ideology of monarchical absolutism. Pierre Charron, the disciple of Montaigne, represents the completion of the shift in moral philosophy. The third book of Charron's *De la sagesse* discussed "the four moral virtues of prudence, justice, strength, and temperance." Strength (*force*) had been substituted for the traditional fortitude, and prudence had replaced wisdom. Prudence, indeed, had become "the general queen, superintendent, and guide of all the other virtues," and Cicero was cited as the interpreter of the two complementary kinds, private and public.⁸⁴ In actuality Tacitus and Seneca dictated the manner in which Charron interpreted Cicero. They were cited together to demonstrate that sovereignty required equity in its exercise and should not be confused with a tyranny in which the prince was above the law. Yet the world was full of malicious men whose treachery was wont to subvert the state unless the prince mixed prudence with justice and composed his cloak of both the pelt of the fox and the skin of the lion. It was in this context that one acted for the public welfare and cited the Ciceronian maxim *salus populi suprema lex esto*.⁸⁵

Cicero had counseled the statesman to be watchful, to believe nothing and guard against all eventualities, and Seneca had taught the necessity of dissembling. As Tacitus had indicated in *Agricola*, the prince must keep himself informed of all that happened, must never relax, and must constantly employ dissimulation. Many references of this kind played fast and loose with the texts they purported to cite, and the names of Tacitus, Seneca, and Cicero in the margins of Charron's book did not necessarily imply a just reflection of what these authors had actually written. Tiberius and Vespasian joined the ranks of "great princes and wise statesmen" with Augustus, Trajan, Hadrian, and the Antonines. Tacitus had taught that evil princes were the instrument of God's justice and must be suffered, and when Cicero had cited Greek opinions about tyrants he had referred to those who had usurped authority, not to legitimate princes. Seneca, for his part, had instructed the counselor of rulers to trim his sails to fit the wind, while Tacitus had taught him discretion not to pry into royal secrets that should be kept hidden.⁸⁶ Piety, honor, and equity were words that Charron employed at intervals, but they, like everything else, were twisted to conform with the new linguistic mode in which wisdom had become the slave of prudence.

IT WOULD BE EASY TO ASSUME that the linguistic and ideological shift evident in the alternating fortunes of Cicero and Tacitus was merely the by-product of the religious strife and social dislocation experienced in sixteenth-century France. Certainly, the personal lives of those whose allusions to Cicero and Tacitus have

zur Geschichte eines politischen Begriffs (Berlin, 1975), 87-119; and J. H. M. Salmon, "Rohan and Interest of State," in *ibid.*, 121-40.

⁸⁴ Charron, *De la sagesse: Trois livres* (1601; Paris, 1604), 477-78, 482. On Charron in general, see J. B. Sabrié, *Pierre Charron: De l'humanisme au rationalisme* (Paris, 1913); and H. Busson, *Le Rationalisme dans la littérature française de la Renaissance, 1533-1601* (Paris, 1957).

⁸⁵ Charron, *De la sagesse*, 488-89. The image of the lion and the fox was not necessarily from Machiavelli: Charron could have found it in *De officiis*; see Cicero, *De officiis*, 1: 13, 41 (p. 17).

⁸⁶ Charron, *De la sagesse*, 497, 526, 504.

been traced in these pages were profoundly affected by persecution and civil war. Dolet and Ramus met violent deaths. Muret and Lipsius accommodated their outward creed to the places in which they found refuge. Among the opponents of absolutism, Hotman and Bèze were intimately concerned with the conduct of the Protestant cause, and Boucher and Dorléans with that of the Catholic League. Those whose opinions were in some sense *Politique*—Gentillet, Bodin, Du Vair, and even Montaigne—were to a greater or lesser degree involved in negotiations or administrative responsibilities in time of civil faction and religious conflict. Undeniably, the emergence of Tacitean prudence and the reinterpretation of Cicero owed much to the triumph of *Politique* compromise and the mutual exhaustion of Catholic and Protestant enthusiasts.

Yet the wave of humanist culture in the early part of the century and the determination of its Epigoni to give it practical expression in the higher levels of the royal administration contained undercurrents of intellectual change, whatever the political environment. Ciceronian rhetoric embraced too many disciplines. The debates among the philologists, the dialecticians, and the defenders of eloquence for its own sake destroyed the unity of humanist endeavor and fostered the advance of new theories of politics and history. This was the malaise after the reign of Francis I, which Bodin recalled as "the bitterest sense of grief that those brilliant flashes of talent which shone throughout all France have been extinguished in desolation and want."⁸⁷ Ultimately for this reason Montaigne came to profess his contempt for the grammarians and the logicians. It was also true that war and fanaticism had caused the humanists either to appear irrelevant or to adapt their moral and political ideas to inappropriate ends. Disillusionment resulted in the triumph of the least virtuous of the virtues. Prudence, the art of particularity and dissimulation, emerged in both its public and its private aspects, the one to shield the arcana of the absolutist state, the other to shelter the citizen in the fortress of the individual mind. Attitudes to Cicero and Tacitus in sixteenth-century France reflected the destruction of that humanist ideal whereby a highly educated elite might participate in an enlightened administration. State and society stood in antithesis to one another, and the French variety of Renaissance humanism disappeared from intellectual life.

⁸⁷ Bodin, *Method for the Easy Comprehension of History*, 7.

Incumbered Wealth: Landed Indebtedness in Post-Famine Ireland

L. P. CURTIS, JR.

"Your income from lands is so cut up by interest of money,
annuities to poor relations and to fosterers, so that,
in the best of times, you have but little to *actually touch*."

"An Irish estate is like a sponge,
and an Irish landlord is never so sick as when he is sick of his property."¹

OVER THE YEARS IRISH LANDLORDS have had a rather poor press. Writers of both history and fiction have depicted them as personifying almost all of the seven deadly sins with few, if any, redeeming virtues. Such hallowed works in the nationalist repertoire as Michael Davitt's *The Fall of Feudalism in Ireland*, Seumas MacManus's *The Story of the Irish Race*, and Dorothy Macardle's *The Irish Republic* contain angry or bitter allusions to this "alien class" that stole the land from the people and then spent the next three centuries in pursuit of pleasure at the expense of the "native Gael."² A recent best-selling historical romance dealing with the struggle for independence, *Trinity*, reinforces the familiar stereotype of the slow-witted (the wits dulled by drink or drugs) and rapacious landlord draining the countryside of capital in the form of rackrents and giving nothing in return except "evictions, rent raises, and public floggings."³ The political attractions of this view make it unlikely that the scholarly studies of landlord-tenant relations and landlordism by W. A. Maguire, Barbara L. Solow, James S.

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¹ James O'Connell to Daniel O'Connell, March 28, 1822; and the Earl of Dufferin to the Duke of Argyll, May 7, 1874.

² For Davitt's indictment of landlordism, see *The Fall of Feudalism in Ireland* (London, 1904), esp. xv-xviii, 26-33. MacManus accused "the absentee landlord" (as if there were no resident landlords) of having evicted thousands of tenants from their holdings; see MacManus, *The Story of the Irish Race* (rev. ed., New York, 1967), 603, 606. In her lengthy account of the Irish struggle for independence, Macardle claimed that "until the end of the [nineteenth] century the 'hostile army' of landlords was helping to empty Ireland of its inhabitants while in neighbouring countries populations multiplied"; Macardle, *The Irish Republic* (London, 1968), 43.

³ Leon Uris's sketch of the wicked father of Morris Hubble, Earl of Foyle, perpetuates the familiar stereotype: "The old Earl passed on, basking in the Indies in the arms of an extremely young, beautiful, black mistress. He had lived in supreme style and died from an unmentionable disease, the pain of which had been removed by a heavenly veil of opium." Uris, *Trinity* (New York, 1977), 70-71. For other glimpses of Uris's views of Irish landlordism, see *ibid.*, 45, 126-30.

Donnelly, Jr., and William E. Vaughan will do much to alter, much less improve, the notorious reputation of the landlord—at least in Irish or Irish-American eyes.⁴

Central to the popular image of Irish landlords has been their acquisitive instinct, their seemingly insatiable appetite for wealth. To judge from nineteenth-century Irish novels, the country was filled with reckless or impecunious members of the gentry who spent money as fast as they could beg, borrow, or marry it. Indeed, circumstantial evidence suggests that the Irish landed elite devoted much of its private life to money cares, which meant in practice searching for low-interest loans and dodging bill collectors. To argue for an objective as well as analytical approach to Irish landlordism does not mean that evicting or rack-renting landlords should be ignored, but professional historians, unlike the makers of myths and partisans of earlier generations, have to concern themselves with questions that go beyond the pursuit of landlord “villains.”⁵ Such historians must chart the behavior of a whole range of landlords who cannot simply be put into categories marked “good” and “bad.” In Irish historical literature it is all too easy to find tales of landlord avarice or iniquity. What is rare is documentary evidence covering the whole history of an estate from its creation in the centuries of conquest and confiscation to its liquidation through sale in the years after 1885. Because histories of landed estates vary as much as the lives of the people who lived on them, it is not unusual to find estates in the same barony undergoing different experiences during good times or bad.

Irish estates varied greatly in size, resources, credit, and management practices, and generalizations about their operations often founder on exceptions so numerous as to disprove the rule. Ideally, the study of landlordism in Ireland, as elsewhere, requires an aggregate approach. Case histories of great estates—usually so much better documented than small estates—may reveal many important facets of the landlord system; but, without knowledge of how medium- and small-sized estates functioned in the same period, the extent to which the great estate was typical of the whole can never be fully understood.⁶ During the 1870s

⁴ For works by these scholars, which epitomize concern for evidence and objectivity; see Maguire, *The Downshire Estates in Ireland, 1801–1845* (Oxford, 1972); Solow, *The Land Question and the Irish Economy, 1870–1903* (Cambridge, Mass., 1971); Donnelly, *The Land and the People of Nineteenth-Century Cork* (London, 1975); and Vaughan, “Landlord and Tenant Relations in Ireland, 1850–1878” (Ph.D. thesis, University of Dublin, 1974).

⁵ It is easy to manipulate the rather ambiguous statistics relating to eviction and to assume that all ejectment proceedings ended in final (not to say, forcible) expulsion from holdings. Contrary to partisan accounts, very few landlords relished evictions, and they usually had a hard time finding tenants prepared to take on the stigma and the strain of working an evicted holding. Eviction did not always mean final separation from a holding: according to one official estimate, 21,340 of the 90,107 families evicted from their farms between 1849 and 1880 (23.7 percent) were readmitted as either caretakers or tenants. Solow has contended that the percentage of evicted families readmitted in the early 1880s ranged between 40 and 50 percent, and she has estimated that the overall eviction rate between 1855 and 1880 was less than 3 percent of the six hundred thousand holdings in 1855. Solow, *The Land Question and the Irish Economy*, 54–57. There were, of course, some harsh and large-scale clearances on various estates during and after the Famine, and no amount of statistical finesse can prove that the suffering caused by these evictions was a figment of the Land League’s imagination. Fear of eviction was widespread among the poorer tenantry, and the “collective folk memory” of eviction formed a vital part of both Irish folklore and agrarian political consciousness. Also see F. S. L. Lyons, *Ireland since the Famine* (London, 1971), 103–04, 156, 159–60. And, for allusions to the callous behavior of some landlords during the Famine, see Cecil Woodham-Smith, *The Great Hunger* (London, 1962), 70–72, 123–24, 163–64, 183, 298–99, 324–28.

⁶ See, for example, Maguire, *The Downshire Estates in Ireland, 1801–1845*. Probably no other resident Irish

Ireland had more than six thousand "substantial" estates—defined as holdings in excess of five hundred acres or £500 annual valuation.⁷ Given the number of and profound disparities between estates, it is risky to draw even mildly dogmatic conclusions about Irish landlords as a whole from the anatomy of one or two estates, no matter what their size and resources.

The powerful forces of neglect and destruction have been at work on Irish records long enough to rule out the chance of obtaining a full accounting of landed wealth and indebtedness in Ireland. Only in rare instances do the surviving documents contain a reasonably complete and reliable record of an estate's financial condition, and even then the rent books and accounts do not shed much light on the social relations prevailing on that particular estate over time.⁸ For a class commonly reputed to have possessed as many debits as credits, little is known about the nature and extent of landlord indebtedness during the nineteenth century. Like the proverbial iceberg, only what lies above the water line is visible, even if a decent run of rentals and accounts is available. But measuring with accuracy the bulk of the ice—or indebtedness—lying beneath the surface is no simple matter. What follows is an attempt to explore the dimensions of landed (or landlord) indebtedness in post-Famine Ireland. Beyond this objective lie the possibilities, however tenuous, of advancing some arguments about the consequences of indebtedness on certain kinds of estates and making some connections between the economic problems of the landlords and their responses to the social and political unrest of the Parnellite era.⁹

Several assumptions underlie this study, and these should be exposed before proceeding to examine the material evidence. The first assumption involves the existence of a vital and close connection between a landlord's debts and assets—that is, between his annual income and the annual operating costs arising out of his estate and his family. In short, a landlord's net worth, once he had deducted his fixed costs and overhead, weighs far more heavily in this study than does his gross rental—that is, the sum of the rents and arrears due to him twice a year. The incumbrances or capital debts of Irish landlords may help show something about the actual solvency, as distinct from the reputed wealth, of estates great and small. And solvency is the product of a constant interplay between annual rental income and the myriad expenses of running an estate and maintaining the life-style appropriate to a landed gentleman.¹⁰

landowner possessed the wealth, resources, and credit of the Marquesses of Downshire. Their lands in six counties had an official valuation of £91,563 and amounted to 110,273 statute acres.

⁷ These are the criteria used by U. H. Hussey de Burgh in his useful but erratic compilation, *The Landowners of Ireland* (Dublin, 1878), based on the government's "Return of Owners of Land in Ireland," *Parliamentary Papers* [hereafter, *PP*], 1876, 80: 422.

⁸ Both public and private archives in Ireland contain many fragments of estate papers, including runs of rentals and accounts and "out-letter books," which consist of copies of letters written by the agent. But there are relatively few complete collections of family and estate records. In some cases landed families entrusted the bulk of their deeds and financial documents to the care of solicitors who kept or destroyed all of the papers in question. Documents pertaining to the most heavily incumbered estates were usually the first to disappear, and, since much more documentation has survived about solvent estates, there is a danger that the evidence creates a false impression of more affluence than insolvency.

⁹ As used here, the term "landlord" refers to owners of tenanted land. Almost all substantial owners were landlords. But there were just enough small landowners in Ireland who had no formal tenancies to justify preserving the slender distinction between these two terms.

¹⁰ Landlords receiving an annual remittance or "profit" from their estates of at least 30 percent of the rents

The second assumption of this study is the presence of a widespread credit-debit nexus in Irish rural society. Both landlords and tenants, in other words, depended for their well-being, if not for their survival, upon an elaborate and secretive system of short- and long-term loans. In the post-Famine period, after the rural economy had become fully monetized, virtually every tenant farmer owed money to someone at one time or another. If the tenant could not afford the interest rates of the local moneylender or gombeen man, he might find a friend or relative who would lend the necessary amount—to be repaid, he hoped, once the pig, the bullock, or the harvest had been sold.¹¹ A crop failure or sharp fall in prices could, of course, wreak havoc on this vulnerable credit system. In times of prolonged distress, notably after 1878, both the borrowers and the moneylenders suffered severe losses. What is too often forgotten is that landlords were just as dependent on credit as were their tenants—only on a much grander scale. Anything likely to cause hardship among the tenantry was bound, sooner or later, to affect the economic position of the landlords. Mounting arrears of rent could hardly be used to pay the coach maker, the wine merchant, or the tailor, not to mention the unrelenting moneylender in Dublin or London. However different the motives and mechanisms of landlord and tenant indebtedness and however higher the credit rating of the man of property, the closeness of the landlord-tenant connection made it virtually impossible for either party to escape unscathed from the misfortunes of the other. Any downturns in the agricultural economy, for example, could not help but embarrass those landlords and tenants who were already paying more in the way of interest than they could afford in the best of years.

ACCORDING TO THE NOVELS OF ANGLO-IRISH GENTRY LIFE, all landed families were seemingly engulfed in debt and all owners were apparently plagued by parasitical moneylenders looking for a chance to profit from the misfortunes of an ancient family. From *Castle Rackrent* to *The Big House of Inver*, the novelists of landed society depicted a class that habitually lived beyond its means.¹² Because credit came so easily to landowners many of the Irish gentry mortgaged their estates to the hilt in order to impress others with their ability to afford not only the necessities but the luxuries of the social seasons. The obsession of many pre-Famine Irish landowners with conspicuous spending meant that the stock figure of the fortune-hunting squire and squireen was not just a figment of the novelist's imagination. The younger sons, brothers, and nephews of the "mere gentry" produced more than their share of rakes and adventurers whose heavy gambling, drinking, wenching, and duelling enlivened not only the novels of Edgeworth, Lever, Lover, and Thackeray but also Irish life.

received, after the agent had paid all the bills and costs of incumbrances, should be considered solvent. Any amount less than this would have made it difficult for a gentleman dependent on rental income to keep himself and his family in the manner appropriate to his social standing.

¹¹ For insights into the operations of this rural credit system, see the "Report of the Departmental Committee on Agricultural Credit in Ireland," *PP*, 1914-16, 80, Cd. 7675: 51-62.

¹² Both Maria Edgeworth and Edith Somerville, along with "Martin Ross," had direct experience of dealing with tenants and managing estate finances.

If nineteenth-century novelists treated the foibles of Irish landed society with both irony and satire, the realities of the Big House were not far removed from the world of the Rackrents, the Clonbronzys, and Barry Lyndon. The agricultural boom of the late eighteenth and early nineteenth centuries increased the appetite of many landowners for the symbols and comforts of wealth. As in England, so in Ireland the mortgage loan was the indispensable instrument of social as well as economic progress. Few landowners managed to avoid borrowing money at some stage in their lives.¹³ By means of loans scattered among various creditors, some of whom were relatives and others professional moneylenders, owners could continue to maintain a privileged lifestyle long after their rental income had ceased to pay the bigger bills. When crop failures or depressions occurred, landowners who had mortgaged their properties to the legal limit found it well-nigh impossible to pay their interest installments due twice a year. By the unwritten law of the survival of the least incumbered estates, owners who had exhausted their credit with the moneylenders and who had found no heiresses to wed were the first to experience the indignity of receivers appointed over their estates or, after 1849, to witness the sale of their lands in the Incumbered Estates Court.

Conspicuous consumption might not have caused Irish landowners quite so much grief had they enjoyed the additional sources of income available to so many of their English and Scottish counterparts.¹⁴ Ownership of coal mines, canals, and property ripe for urban development or investments in commercial and industrial enterprises might have helped to pay for the aristocratic tastes of the Irish gentry. But no more than 1 percent of all resident Irish landowners was fortunate enough to own either profitable coal mines, urban estates in Dublin or Belfast, or good farm land in England. When an estate had been mortgaged to the legal limit, the owner, if desperate for ready cash, might struggle to raise a second or even a third mortgage with some other—presumably unsuspecting—creditor. But it was only a matter of time before such estates were declared insolvent and put up for sale in the courts created to dispose of them.¹⁵

Like poverty, wealth is relative, and many Irish landowners lived in a financial limbo, caught somewhere between luxury and austerity. The degree of comfort was determined by the quality of the harvest, price fluctuations, and the presence of a dowager whose maintenance—by means of a jointure—could drain a small estate over the years. Of the various causes of landed indebtedness

¹³ For examples of heavy mortgages and family charges, see Maguire, *The Downshire Estates in Ireland, 1801–1845*, 65–106; and R. F. Foster, *Charles Stewart Parnell* (London, 1976), 49–53, 98–104, 127–30, 190–96. For mortgaging activity by English landowners, see David Spring, *The English Landed Estate in the Nineteenth Century* (Baltimore, 1963), 35–40, 189–90, and “The English Landed Estate in the Age of Coal and Iron, 1830–80,” *Journal of Economic History*, 11 (1951). Also see the writings of F. M. L. Thompson on English landownership; for convenient citations to them, see David Cannadine, “Aristocratic Indebtedness in the Nineteenth Century: The Case Re-opened,” *Economic History Review*, 2d ser., 30 (1977): 625 nn. 1–4. Cannadine has also referred to landowners who borrowed from institutions, friends, and relations; *ibid.*, 633–37.

¹⁴ For the business interests of English landowners, see F. M. L. Thompson, *English Landed Society in the Nineteenth Century* (London, 1963), esp. 256–68, 317–26. Also see the articles by David Spring cited in his *The English Landed Estate*, 201; J. T. Ward and R. G. Wilson, eds., *Land and Industry: The Landed Estate and the Industrial Revolution* (Newton Abbot, 1971); and Richard Perren, “The Landlord and Agricultural Transformation, 1870–1900,” *Agricultural History Review*, 18 (1970): 36–51.

¹⁵ See the returns of sales of insolvent estates in the Landed Estates Court and its successor, the Land Judges Court, *PP*, 1880, 60, no. 377, and *PP*, 1890, 60, no. 74.

by far the most tangible was the construction of a country house, a castellated or palladian showplace surrounded by elaborate pleasure grounds. But so obvious an explanation can mislead, and there is scant evidence to prove that owners of Irish estates slid into bankruptcy under the pressure of building mortgages alone. The eagerness of Irish landowners in the eighteenth and early nineteenth centuries to emulate the English aristocracy did indeed drive them to spend beyond their immediate means on building an impressive Big House;¹⁶ but the building boom of the late Hanoverian era derived much of its impetus from the prosperity of agriculture in a time of increasing demand for foodstuffs in the British Isles. After the 1760s, rents began to rise in response to the pressure of an expanding population on both land and farm produce, and the increased value of land made moneylenders all the more willing to do business with the gentry and aristocracy.¹⁷

Most Irish landowners who built new houses after the 1760s seem to have taken the costs in stride. The exceptions included small landowners whose appetites for display did exceed their ability to pay and also compulsive spenders like the famous Bishop of Derry (Frederick Hervey, Earl of Bristol), who commissioned two palatial houses in county Londonderry during the 1770s and 1780s. Hervey had inherited a magnificent house and estate in Norfolk, but his income did not permit him to maintain three great houses—and he never spent a night in Ballyscullion, which had cost almost £80,000. His passion for building and living on a grand scale engulfed him in debt and certainly hastened his financial collapse.¹⁸ Of other cases of costly Big Houses, few came close to Hervey's extravagance. Castle Coole, near Enniskillen in county Fermanagh, cost the first Earl of Belmore some £54,000 during the 1790s; and the second Earl of Gosford paid almost £80,000 for his crenellated showplace, Castle Gosford, in the 1830s.¹⁹ But in neither case could the owner's financial difficulties be traced directly or exclusively to building expenses.

A number of other costs, especially those arising out of family charges, accounted for the heavy incumbrances that eventually forced landowners to part with their properties by means of voluntary or involuntary sale. In the long run, the cumulative cost of an aristocratic life-style drove many owners of estates to the moneylender. No single expense but the combination of all of the bills associated with running an estate and maintaining high status tended to exhaust an owner's income. When a town house in London became a necessity for most

¹⁶ Too many of the recent books on Irish country houses belong on the coffee table rather than on a desk. There is need of the kind of historical analysis found in Mark Girouard's *Victorian Country House* (Oxford, 1971) and Lawrence and Jeanne Stone's "Country Houses and their Owners in Hertfordshire, 1540-1879" in W. O. Aydelotte et al., eds., *The Dimensions of Quantitative Research in History* (Princeton, 1972), esp. 115-23.

¹⁷ For a discussion of the agricultural and commercial development of the Irish economy after 1750, see L. M. Cullen, *An Economic History of Ireland since 1660* (London, 1972), 77-103; and Geraróid Ó Tuathaigh, *Ireland before the Famine, 1798-1848* (Dublin, 1972).

¹⁸ For the Earl-Bishop of Derry's building mania, see Constantia Maxwell, *Country and Town in Ireland under the Georges* (Dundalk, 1949), 88-90; and, for an allusion to his other passions, see Brian de Breffny and Rosemary Ffolliott, *The Houses of Ireland* (London, 1975), 166-68.

¹⁹ For the building of Castle Coole, see Maxwell, *Country and Town in Ireland under the Georges*, 90-91; and Desmond Guinness and William Ryan, *Irish Houses and Castles* (Dublin, 1971), 163-68. For Castle Gosford, see William Greig, *General Report on the Gosford Estates in County Armagh*, ed. F. M. L. Thompson and D. Tierney (Belfast, 1976), 8-9. In 1817 Lord Gosford was spending roughly 57 percent of his rental income on interest arising out of mortgages and on annuities.

Irish peers and the wealthiest gentry after the passing of the Act of Union, a host of new expenses as well as new temptations came into play and often stretched an estate's resources to the breaking point.

With the exception of losses at the gaming table, some of which had devastating effects on the gambler and his family, expenditure on what has been called "the paraphernalia of gentility" had only a nuisance value when compared with the cost of family charges incurred through marriage settlements. In his pioneering study of Irish landed wealth in the latter eighteenth century, David Large discovered that the actual operating costs of an estate, including money spent on improvements, accounted for only a small fraction (in some cases less than 15 percent) of the annual rental. Big House construction and a lavish social life could, indeed, deplete an estate's resources, but even the extravagant second Baron Cloncurry managed to survive the heavy costs of building Lyons in county Kildare. Without benefit of quantification Large attributed most of the indebtedness on the estates in his small sample to charges arising out of family settlements. As Edward Wakefield observed long ago, even great landowners knew the anguish of watching "the whole of their fortune being absorbed either by payment of a mother's jointure, the fortunes bequeathed to brothers or sisters, or debts contracted by themselves or left them by their predecessors." And, when dowagers survived their husbands by an average of twenty-one years, as did the widows of Irish peers in the 1780s, the expense of jointures appropriate to a countess or marchioness could not be taken too lightly.²⁰

If marriage settlements devoured landlord incomes in the late eighteenth century, as Large has contended, it is not hard to imagine how much heavier the burden of three or four generations of such charges could be, especially when the rental income on many estates had barely doubled between 1815 and 1875. Only a rare combination of "fortuitous" circumstances—the early demise of a dowager, an heir apparent who did not gamble, and a marriage alliance with an heiress bearing a handsome dowry—could have relieved lesser landowners of that cumulative debt burden that drove so many of that class to the brink of insolvency—and beyond—after 1878.

Although far too fragmentary, the evidence for the post-Famine period does indicate that family charges, especially portions for younger children, accounted for the heaviest incumbrances on many Irish estates. Providing capital sums for the children of landed families tied up precious assets at a time when these might have been gainfully invested in commercial or industrial enterprises yielding 4 or 5 percent. The dictates of custom and status forced landowners to incumber their estates with huge liabilities in order to keep their closest kin in comfort; and such obligations could easily cripple an estate over several generations, especially if the portions were generous and large arrears of rent were tolerated.²¹ Had landowners kept marriage settlements down to a minimum of

²⁰ Large, "The Wealth of the Greater Irish Landowners, 1750–1815," *Irish Historical Studies*, 15 (1966): 33–35, 37, 38.

²¹ Although there are no comparative studies of the debt burdens on English, Scottish, Irish, and Welsh estates, it is reasonable to assume that English (and a few Scottish) aristocrats had an easier time coping with their incumbrances than did all but a handful of Irish or Anglo-Irish peers who owned property in Great Britain. For present purposes and in spite of the ambiguities of "ethnic origins," such peers as the Earl of Dunra-

support and had they invested more capital in stocks and bonds, they would have had far less need of the services of professional moneylenders. Portions of up to £1,000 for each younger child were common enough among the substantial gentry, and peers above the rank of viscount often made provision for sums up to £10,000 per child when the estate was settled or resettled at the time of the heir apparent's marriage. In 1856 some 40 percent of the incumbrances on the Downshire estates derived from family charges or marriage portions, the result of "successive family settlements since 1741." Such charges might not have embarrassed owners while harvests were good and prices high, but every decade had its poor seasons and the Great Famine of 1845-48 left landowners ruining the day when they or their ancestors had tied up so much capital in wholly unproductive investments. When agriculture fell upon hard times in the early 1860s and again, more dramatically, in the late 1870s, incumbered owners had to turn and return to the moneylender for transfusions of cash sufficient to maintain appearances or pay the outstanding interest on previous loans.²² Obsession with status and commitment to familial privilege thus burdened owners with debts that passed from owner to heir until the day of reckoning.

As the scion of an old Irish landed family has observed, many landowners simply "could not resist the temptations of prosperity."²³ Even the richest landowners found it hard to avoid the temptations of low interest loans. Maguire's meticulous study of the Downshire estates reveals just how easily credit came to a magnate of the marquess's stature. His land agents actually had to rebuff offers of financial help from merchants, lawyers, and neighboring landowners with capital to spare, who were looking for a "blue chip" investment.²⁴ To be the creditor of a landed magnate must have brought social as well as financial profit to an ambitious and unaristocratic investor. Such eagerness to underwrite the obligations of a great landowner for a mere 4 to 4.5 percent interest rate could only have whetted the appetite of those with a taste for luxury goods and services. Not all Irish estates, of course, looked as attractive to mortgagees as did Lord Downshire's, for here was an owner who could and did borrow some £186,500 between 1810 and 1840 without straining his resources.

In general, then, the larger or wealthier the estate, the easier loans at low interest were to obtain. On small estates, where owners depended wholly on rental income, a large loan might prove harder to find, depending on the state of the money market and the charges already weighing upon the owner or tenant-for-life. Heavily incumbered owners might have to pay as much as 5 to 5.5 percent interest in the 1870s to compensate for the greater risk to the creditor. Peers, on the other hand, usually enjoyed interest rates around 4 percent, and the only commoners who could match their high credit rating were the rare owners of lucrative coal mines, like the Prior Wandesforde's of Castlecomer, county Kil-

ven, the Marquess of Londonderry, and the Earl of Meath are considered here as Anglo-Irish, whereas the Duke of Devonshire, the Marquess of Lansdowne, the Earl of Pembroke, Baron Leconfield, and Lord John Russell, all of whom owned large estates in Ireland, are classified as being more English than Irish.

²² See James S. Donnelly, "The Irish Agricultural Depression of 1859-1864," *Irish Economic and Social History*, 3 (1976): 33-54; and Solow, *The Land Question and the Irish Economy*, 168-94.

²³ Conversation with the Hon. Bryan Bellew, March 2, 1970, Barmeath Castle, Dunleer, county Louth.

²⁴ Maguire, *The Downshire Estates in Ireland, 1801-1845*, 101-04.

kenny, or a great linen manufacturer, like John Mulholland of Belfast, who became Baron Dunleath in 1892. In virtually every mortgage, what mattered most was not the face amount or principal but the relationship of the interest due on that sum to actual income.²⁵ The dimensions of this "debt burden," the percentage of rental income paid out to service these loans, as well as its implications constitute the principal concern of this study.

Two basic questions emerge: first, roughly how much of the annual income of these estates went to pay interest on mortgages and other family charges, and, second, what could incumbered owners afford to spend on servicing those debts before they began to sink into insolvency? Given the disparity of resources and management practices on Irish estates, these questions admit of no easy or precise answers. Deficiencies of evidence, moreover, are an obstacle to understanding: countless estate papers have been lost or destroyed, and the records of family solicitors and private moneylenders have not surfaced. To make matters worse, the sample of incumbered landlords used in this study is too small—two hundred and twenty-five, or only 3.7 percent of the substantial landowners of Ireland—to constitute the degree of proof one would prefer. When faced with such obstacles, historians are wont to say, rather defensively, that a small sample is better than none, and that is the case here.

Most of the documentation comes from two sources, the Church of Ireland and the Irish Land Commission.²⁶ The first set of records consists of the mortgage ledgers of the Representative Body of the Church of Ireland (or RCB). Under the Disestablishing Act of 1869 the Church of Ireland received compensation of almost £8.5 million, and the annual yield from this endowment helped defray operating expenses, including subsidies to needy clergymen.²⁷

²⁵ Interest rates varied in accordance with money supply, credit rating, connection, timing, and other factors, but they remained remarkably stable for mortgages from the 1850s to the early 1880s. In the 1830s a reasonably wealthy landowner could usually obtain a mortgage loan at 3.75 percent and in the early 1870s at 4 to 4.25 percent. Small landowners with less "margin" often had to pay close to 5 percent. The penalty for borrowers who fell behind in their payments could run up to 1 to 1.5 percent over the standard rate. See page 341, note 28, below. Even heavily incumbered landlords could borrow more money by the simple expedient (allowed under Irish law) of declaring themselves by deed to be judgment debtors, which entitled them to raise loans on the security of their land. See W. L. Burn, "Free Trade in Land: An Aspect of the Irish Question," *Transactions of the Royal Historical Society*, 4th ser., 31 (1949): 69. "Indebtedness" primarily means those mortgages and other debts, including borrowings by bond or personal arrangement, on which interest had to be paid. This category does not, therefore, include any government loans authorized in the various land improvement and drainage acts passed after 1847, which enabled the Board of Works to advance roughly £3.1 million at 3.5 percent to landowners who wished to drain, fence, or reclaim their land. See Cormac O Gráda, "The Investment Behaviour of Irish Landlords, 1850-75: Some Preliminary Findings," *Agricultural History Review*, 23 (1975): 139-55. Because these loans involved a modest outlay for the borrower and were voluntary, I treat them here as part of the annual operating cost of an estate. Indeed, such an investment could lead to a gradual increase in both productivity and rental, sufficient in some cases to ease the owner's debt burden by a modest amount. Where the evidence permits, such lesser liabilities as personal debts by bond or private agreement and bank overdrafts have been counted as part of the overall debt burden. Unfortunately, few estate papers contain records of an owner's personal finances, except to register the remittances or profits from the rents received that were deposited in his private bank account.

²⁶ I am most grateful to the secretary and staff of the Representative Body of the Church of Ireland, Church House, Rathmines, Dublin, for permission to work on the mortgage ledgers and other records in their keeping. Thanks are also due to the secretary of the Irish Land Commission (at the time of my research, T. O'Brien) for allowing me to examine the records of the Estates Commissioners pertaining to sales under the Wyndham Act, and also to the trustees of St. Patrick's College, Maynooth, for permission to see the papers relating to the college's seven mortgage loans dating from the 1870s.

²⁷ For the financial provisions of the Irish Church Act of 1869, see Hugh Shearman, "Irish Church Finances after the Disestablishment," in H. A. Cronne *et al.*, *Essays in British and Irish History* (London, 1949), 278-302.

In 1870, the RCB set up a Finance Committee to manage or invest the capital in safe securities. Because land was still considered one of the soundest possible investments in the early 1870s, the committee sank almost half of the RCB's capital in mortgages on Irish estates at interest rates ranging from 4 to 4.5 per cent.

In theory the Finance Committee approved loans only to those applicants whose estates had been found solvent after careful inspection of the books by its solicitor. In practice, however, connection or influence may have helped the owners of some heavily incumbered estates to obtain mortgages from the RCB. Since most of the mortgagors were either parochial or diocesan pillars of the Church, it is reasonable to assume that the Church used its capital not only to realize profit but also to support loyal members of the Church (and the Ascendancy) who needed financial help. Once the word spread that the RCB was prepared to extend loans secured by land at low rates, applications poured into Church House. Most of the requests came from owners who wanted to consolidate their numerous debts into one giant mortgage loan, which would greatly simplify their payment problems while it ensured a low interest rate. No doubt owners also looked forward to a long and happy association with a mortgagee whose spiritual credit and goodwill could not be impugned.

Between 1871 and 1876 the Finance Committee approved one hundred and twenty requests for loans and may well have turned down four to five times that many applications. In these six years the committee accepted 82.8 percent of the total of one hundred and forty-five mortgagors whose names appear in the ledgers up to 1920.²⁸ At its peak in 1877-78 the RCB's mortgage fund amounted to £3.5 million, all of which was tied up in Irish estates. The onset of agricultural depression and agrarian unrest in the late 1870s alarmed all investors in Irish land, and the Finance Committee (or FC) called a halt to all mortgaging operations for several years. Only ten new mortgages were approved between 1877 and 1895.²⁹ The balance of the RCB's capital was placed in such "safe" securities as railway shares and debentures as well as Russian and Ottoman imperial bonds. (These imperial bonds, needless to say, became less than safe after 1914.) Had it not been for the various land purchase acts (1885-1909), which subsidized the sale of tenanted land to the occupiers, few of these mortgagors would

Also see Donald Akenson, *The Church of Ireland* (New Haven, 1971), 260-68, 309-20; and Michael Hurley, ed., *Irish Anglicanism, 1869-1969* (Dublin, 1970), 14, 18, 21-22.

²⁸ The average interest rate for the one hundred and twenty mortgage loans in this sample was 4.32 percent. Only a few of the richest borrowers managed to secure a rate of 3.75 percent from the RCB, whereas most of the smaller landowners had to pay 4.5 percent with a penalty rate of 5 to 5.5 percent. As for adequate security, the FC tried to follow the policy that "the sum lent should never exceed what would leave a surplus of at least half the rental after payment of the mortgage interest." RCB, *31st Report of Proceedings . . . 1901*, Church House, Rathmines. In other words, owners whose debt burden would be in excess of 50 percent counting the mortgage requested from the RCB were, at least in theory, not eligible for a mortgage loan.

²⁹ Between 1871 and 1920 the RCB approved a total of one hundred and forty-five loans to one hundred and forty individuals, taking into account different loans to the same owner over time. (There were two transfers of mortgages from the original mortgagor to his heir.) Four mortgagors could not be located in the "Return of Owners of Land in Ireland, 1876," and these have been omitted from the sample here. The chronological distribution of these loans is as follows: 1871-76, 120 loans approved; 1877-79, 0; 1880-85, 10; 1886-95, 0; 1896-99, 3; 1900-11, 5; 1912-20, 7. Mortgage Ledgers D1-D3 and Y, Church House, Rathmines [hereafter, ML/RCB]. Also see the *Annual Reports* of the RCB from 1871 to 1901. The amount invested by the RCB in mortgages reached an all-time high of £3,512,598.8 in 1876. Interest on this sum amounted to £154,825.8 in 1877 (equivalent to an average interest rate of 4.4 percent).

have been able to repay principal to the RCB, which was severely buffeted by the death or exodus of so many communicants during and after the Great War.

The second major set of records derives from the Irish Land Commission, which originated the Final Schedule of Incumbrances (or FSI). When landlords came to sell portions of their estates under the land purchase acts, their solicitors had to submit a host of documents to the Land Commission giving particulars as to location, size, rental, valuation, and charges pertaining to the lands in question. Under the laws of real property, all who had a superior interest in the land being sold were entitled to compensation out of the proceeds of the sale. In their applications to the commission, the landlords' solicitors itemized all of the charges on the lands being sold along with the names of the incumbrancers. The FSIs thus contain summaries of every charge, ranging from head rents, quit rents, and tithe-rent charges to mortgages and solicitors' fees for preparing the necessary information.³⁰ Running at times to twenty to thirty pages, the FSIs provide some fascinating, if fleeting, insights into the shadowy world of money-lenders, where mortgages and judgments for debt were often assigned and re-assigned from one creditor to another over several generations. Because most of the FSIs contain the land court judges' rulings on the validity of each claim, it is possible to calculate the amount of money paid out of the original purchase money to the various claimants as well as the balance or residue left to the actual vendor.

Neither the FSIs nor the ecclesiastical mortgage ledgers tell the whole story of landed indebtedness in Ireland, but they do constitute two of the largest pieces of a puzzle from which so many other pieces are missing, and they reveal enough about the extent of incumbrances to permit some assertions about the nature and extent of landed indebtedness in the post-Famine period.

THE AVAILABLE MORTGAGE DATA begin to make sense only when linked to income. Without some idea of the actual earnings of these incumbered estates, the assorted mortgage loans are about as useful as an inventory or laundry list. The relative scarcity of rent books and accounts, however, means that income in the majority of cases has to be estimated or improvised, and this calculation is always risky. The most useful source from which to derive such an improvisation is the official valuation carried out by Sir Richard Griffith's staff between 1852 and 1865.³¹ As every Irish historian knows, Griffith's valuation was never in-

³⁰ For instructions concerning the drafting of the Final Schedule of Incumbrances under the various land purchase acts, see George Fottrell and John G. Fottrell, *Practical Guide to the Land Purchase Acts (Ireland)* (Dublin, 1891), 306-09, 476-80, 517, 652-55. Also see R. R. Cherry, *The Irish Land Law and Land Purchase Acts, 1860 to 1891* (Dublin, 1893), 607-09, 644, 659. FSIs deriving from sales prior to 1903 were not available for inspection in the Records Branch of the Land Commission. For a brief introduction to these documents, see K. L. Buckley, "The Irish Land Commission as a Source of Historical Evidence," *Irish Historical Studies*, 8 (1952): 28-36.

³¹ For details of Griffith's valuation, see Solow, *The Land Question and the Irish Economy*, 59-68; Select Committee [House of Commons] on General Valuation, Etc. (Ireland), *PP*, 1868-69, 9, no. 362; and the Irish Land Committee's pamphlet, "The Land Question, Ireland, No. 1: Notes upon the Government's Valuation of Land in Ireland" (June, 1880). For our purposes, the valuation is least useful, *mutatis mutandis*, as a guide to net rental in those counties surveyed during the early 1850s—most of which lay in Leinster and Munster. The valuers assessed Ulster and parts of Connaught in the latter 1850s and early 1860s, when prices had recovered from the depression of the Famine years.

tended to serve as a measure of rental or sale value; rather, it was designed to provide a basis for local taxation. The valuers based their assessments on the prevailing prices of such staples as wheat, oats, barley, butter, and livestock, and they also took into account access to urban markets for those goods. Since the prices for all of these staples fluctuated sharply between the 1850s and 1880s, the valuation could not be expected to reflect the hypothetical earning power of holdings or estates much before or after the valuers' visit. If, however, there are some compelling reasons for not treating Griffith's valuation as equivalent to rental income, there are even stronger reasons for doing just that, because, ironically enough, the valuation does provide a rough guide to rents actually received on many estates after 1878.

As poor harvests and falling prices eroded farmers' income after 1878 and as tenants in increasing numbers began to obey the Irish Land League's orders to pay their landlords no more than the equivalent of Griffith's valuation, arrears of rent soared upward and rental income fell correspondingly. Analysis of the rents received on two dozen estates (mostly from the years 1875 through 1884) reveals a close proximity between valuation and income. The average rents received on these twenty-four estates over a ten-year period exceeded the valuation by only 1.7 percent.³² Admittedly, most of these estates lay in eastern Ulster and Leinster, where rents tended to be paid more regularly than in the west, especially during the land war. It is important to note the difference between the rental (rents due) and rents received: on twenty-three of these same estates, annual rents due exceeded not only rents received by 2.7 percent but also the valuation by 4.1 percent. In other words, the rents collected on these estates came much closer to the valuation than did the nominal rental.

Adverse economic conditions in the latter 1870s, which were not confined to Ireland or even the British Isles, and agrarian agitation organized by the Irish Land League combined to depress rental incomes to the point where many owners were receiving the rough equivalent of Griffith's valuation, no matter how arbitrary or outdated that valuation may have been. Another cause of the shrinking incomes of landlords after 1881 was the operation of the fair rent clauses of Gladstone's second Land Act. Between 1882 and 1893 the rents on 288,054 holdings all over the country were reduced by an average of 20.7 percent.³³ For these compelling reasons, then, the rental income on virtually all Irish estates began to fall off significantly from the peak attained between 1875 and 1877.

³² Of these twenty-four estates, nine were in Ulster, eight in Leinster, five in Connaught, and two in Munster. Average rents received on these estates over a decade came to £283,487.6, and the total valuation was £278,785—a difference of 1.7 percent. Where possible the rentals in this sample covered both the peak years of income in the mid-1870s and the trough years of the early 1880s. For permission to use these rentals and accounts, I am grateful to the owners or trustees of the following estate papers, from the Public Record Office of Northern Ireland [hereafter, PRONI]: Abercorn, Antrim, Downshire, Erne, Gosford, Ranfurly, and Stewart; and from the National Library of Ireland [hereafter, NLI]: Clonbrock, Crofton, Drake, Hodson, Inchiquin, Leitrim, Leslie, O'Hara, and Ormonde. For the use of rentals and accounts in private hands (at the time they were examined), special thanks are due to Col. Errold Cosby of Stradbally, the Earl and Countess of Dunraven, the trustees of the Pembroke Estate, Josephine O'Connor of Clonalis, the Hon. Thomas Pakenham of Tullynally Castle, and the late Lord Talbot de Malahide of Malahide Castle.

³³ "Report of the Irish Land Commissioners . . . 22 August 1891 to 31 March 1893," *PP*, 1893-94, 24, C. 7056, Tables 4, 48.

TABLE 1
Mortgage Loans of the Representative Church Body, 1871-1907

<i>Estate Valuation in £</i>	<i>Number of Estates</i>	<i>Total Valuation in £</i>	<i>Total Mortgage Loans</i>	<i>Average Mortgage Loan</i>	<i>Ratio: Mortgage/ Valuation</i>	<i>Annual Interest Due</i>	<i>Average Rate of Interest</i>	<i>Debt Burden %</i>
SMALL: 100-1,000	34	18,938.8	299,176.5	8,799.3	15.8	13,420.0	4.49	70.9
MEDIUM: 1,000-5,000	63	147,998.7	1,350,050.4	21,429.4	9.1	60,112.5	4.45	40.6
LARGE: 5,000-15,000	11	111,566.0	678,198.5	61,654.4	6.1	28,688.2	4.23	25.7
GREAT: Over 15,000	12	321,584.5	1,484,365.0	123,697.1	4.6	62,390.5	4.20	19.4
TOTALS: All Estates	120	600,088.0	3,811,790.4	31,764.9	6.4	164,611.2	4.32	27.4

SOURCE: Mortgage Ledgers, D1-D3, Y, RCB MSS, Church House, Rathmines, Dublin.

Further evidence of the correspondence between rents and valuation can be found in the reports of the Irish Land Commission concerning sales of estates under the Ashbourne Act of 1885. According to these summary returns, the tenement valuation of all of the holdings sold under that measure up through March 31, 1890, came to £274,358.8, yet the gross rental on those properties was £300,370.7.³⁴ Thus rents due exceeded valuation by 8.7 percent. But this figure must be adjusted downward by at least 5 percent in order to cover the gap between rents due and rents received, a gap of particular importance for the years after 1885, when both rent strikes and the fixing of fair rents caused serious erosions of income on many estates.

In sum, the rents received on many Irish estates after 1878 came much closer to Griffith's valuation than has been hitherto realized. And the convergence of rental income over a three- or five-year span with the official valuation of those estates makes it possible to use the valuation as a rough and ready guide to the income on those estates for which mortgage data are available. The imperfections of Griffith's valuation and the small size of the sample used here are bound to invite some skepticism. But only by taking such risks can we move beyond the elementary stage of enumerating the incumbrances of a few Irish landlords in the late nineteenth century. Using the valuation of those estates whose owners managed to obtain loans from the RCB, an estimate of the percentage of rental income spent on interest during the last quarter of the nineteenth century is possible. Table 1 shows the estimated debt burdens of the one hundred and twenty mortgagors who borrowed over £3.8 million from the RCB between 1871 and 1907.³⁵ A standard interest rate of 4.5 percent has been applied across the board.

Breaking the RCB data down into categories of wealth or income leads to some rather interesting results. Distributing the estates of the RCB mortgagors within a schema of small, medium, large, and great estates, again based on valuation, demonstrates the contrasts between the debt burdens of relatively rich and poor landowners.³⁶ The average debt burden of these one hundred and twenty mortgagors, using valuation as roughly equivalent to income, comes to 27.4 percent. Owners of small estates in this sample had to pay slightly over 70 percent of their estimated income to the RCB, whereas the owners of great estates were paying just under 20 percent in interest.³⁷ These estimates point to a conclusion that should cause no surprise among students of income distribution in capitalist societies: the absolutely rich could afford to borrow huge sums, while the relatively poor had a hard time coping with even modest loans. The

³⁴ "Return of Landowners . . . Purchase of Whose Properties . . . Sanctioned by the Irish Land Commission," *PP*, 1889, 61, no. 81: 1-17, and *PP*, 1890, 60, no. 115: 1-12.

³⁵ In view of the 1.7 percent discrepancy between rents received and the valuation found in the sample of twenty-four estates above, some readers may wish to add 1.7 percent to the valuation of the one hundred and eleven estates in Table 1—making a total of £598,816.6—in order to arrive at a more "realistic" estimate of rental income. Using this figure, the total debt burden falls only half a percentage point—down to 27 percent. The data in Table 1 derive from ML/RCB, D1-D3, Y.

³⁶ However crude these categories, they are preferable to a schema based on acreage because of the great disparities in both the value and the productivity of land among different parts of the country.

³⁷ The figures are 71.5 percent and 19.4 percent respectively.

TABLE 2
Solvent and Insolvent Mortgagors of the RCB

<i>Category</i>	<i>Number of Estates</i>	<i>Total Valuation in £</i>	<i>Average Valuation in £</i>	<i>Total Mortgage Loans</i>	<i>Average Mortgage Loan</i>	<i>Ratio: Mortgage/ Valuation</i>	<i>Annual Interest Due</i>	<i>Debt Burden %</i>
MAGNATES: Mortgages over £50,000	19	364,127	19,164.6	2,235,899.0	117,678.9	6.1	96,794.8	26.6
"DOUBTFULS": Interest in Arrear	18	31,712	1,761.8	454,637.5	25,257.6	14.3	19,732.9	62.2

SOURCE: Mortgage Ledgers D1-D3, Y, RCB MSS, Church House, Rathmines, Dublin.

thirty-four small landowners in this sample must have been stretched to the limit to pay so much of their income to the RCB while meeting all their other obligations.³⁸ Solvency for them presumably depended on other sources of income above and beyond their rents. Some may have had a profession and others may have lived off private income. By way of contrast, the average mortgage loan on the twelve great estates was more than fourteen times larger than that on the small estates, yet the interest payments of these magnates came to less than one-fifth of their estimated rental income.³⁹ The richer landowners also enjoyed a lower interest rate than those in the other categories (see Table 1).

The differing degrees of solvency (and insolvency) among the RCB mortgagors come into clearer focus by concentrating on those borrowers who fell behind in their interest payments after 1880. The FC eventually classified these debtors as "doubtful" and wrote off thousands of pounds in both interest and principal shortly before the outbreak of war in 1914. Table 2 compares eighteen of these "delinquent" debtors with the nineteen magnates whose mortgages exceeded £50,000.⁴⁰ Most of the "doubtfuls" were obscure owners of small estates. Their average valuation of £1,761.8 could barely sustain an average mortgage of £25,257.6. Only six of these estates were valued at more than £2,000.⁴¹ The ratio of mortgage principal to valuation—14.3—was high enough to suggest that these owners must have had other assets besides rents, as the FC could hardly have considered this ratio a safe investment. In theory these "doubtfuls" were paying an average of almost 65 percent of their estimated rental income to the RCB. In practice, however, few of them paid the full interest after the early 1880s; and by 1900 some of them owed up to eight years' back interest.

As Tables 1 and 2 show, the actual amount of the mortgage loan mattered far less than the relationship of interest to income; and, for most owners of small- and even medium-sized estates, the burden of debt weighed heavily. In the long run the RCB lost a significant fraction of its investment in Irish land through default and also through the repayment of principal under the land purchase acts in depreciating stocks and bonds rather than cash. By 1930, some forty-three mortgagors had still not completely repaid the principal; and, by 1940, the FC had written off £161,105.8 in both interest and principal for thirteen of these borrowers.⁴² The average debt burden of the "doubtfuls" does indicate that, when mortgagors had to devote more than 60 percent of their rental in-

³⁸ These loans preceded the age of amortization. Had "modern" lending procedures been in effect, only a minority of Irish landowners in the "medium" and "small" categories could have afforded to amortize their debts before or after the Famine.

³⁹ Twelve of the RCB's mortgagors owned "great" estates. Their combined loans amounted to £1,484,365 or 43.8 percent of the RCB's investment in Irish land as of 1880. ML/RCB, D1-D3, Y.

⁴⁰ There were nineteen persons in the "doubtfuls" category, one of whom, William Percy Jones, does not appear in the "Return of Owners of Land in Ireland, 1876." He has, therefore, been omitted from Table 2.

⁴¹ The median average valuation of these "doubtful" estates was £1,382. Four of them were located in Kerry and three in the much richer county of Meath. The reasons for their insolvency remain unknown.

⁴² The RCB's records do not contain summaries of the losses sustained on the investment in Irish estates. The figures presented here reflect occasional entries in the minutes of the FC's meetings, and the total amount written off may well have been much higher than the sums stated here. Before 1914, the FC established a sinking fund in order to cover some of the losses on the mortgages.

come to interest payments, they were heading for irrecoverable arrears, insolvency, and other embarrassments.

The RCB mortgage ledgers do not, unfortunately, provide answers to a number of pressing questions. First, what other mortgages, if any, did these borrowers have, to what amounts, and from which moneylenders? Second, what other sources of income did these landowners possess in addition to their rents? Third, given the costs of operating an estate—including head rents, tithe rent charges, improvements, wages, salaries, pensions, supplies, and subscriptions—how did any heavily incumbered owner manage to survive for more than a few years if he had to spend over 50 percent of his annual income on servicing fixed debts and charges? On many estates after 1850 the normal annual expenditure under all headings except incumbrances and family charges came to at least one-third of the rents received; so it is hard to imagine how owners coped with debts that ate up more than half of their income without resorting to more borrowing. Fourth, these mortgage data reveal nothing about other liabilities. Apart from jointures and portions for younger children that were presumably paid out of the sums borrowed from the RCB, there were personal debts secured by bond and also bank overdrafts, which had to be redeemed sooner or later. It is more than probable, then, that the RCB data represent the bare minimum spent by these landowners on their financial obligations.⁴³

It would be helpful to know, moreover, just how typical of the landed class these RCB mortgagors were. Were they more or less incumbered than all other Irish landowners, and did the FC choose them out of a host of applicants for loans because they were more solvent or because they were prominent members of the Church of Ireland? If there are no precise answers to these questions, the fragmentary evidence suggests that, while the FC cared most about protecting its investments, such criteria as rank and influence may have played some part in deciding to approve or disapprove certain requests for money. Granted that the RCB advanced money to a few of the richest landowners in the country including the Marquess of Downshire and John Mulholland,⁴⁴ some of their mortgagors hardly deserved full confidence. As Table 2 shows, the RCB was prepared to advance to some mortgagors—the “doubtfuls”—loans worth 14.3 times the annual valuation. But the average ratio between mortgage loans and valuation for all one hundred and twenty estates came to 6.4. In a few instances, the ratio fell well below this average—to a mere 3.6 for the Marquess of Downshire’s mortgage of £329,000.

In the case of Anna, the dowager Countess of Kingston, the FC approved a loan of £236,000, which exceeded valuation by a factor of 13.1. Although a cau-

⁴³ Jointures and annuities could account for as much as 10 or 20 percent of rental income. On the Doneraile estate in north Cork, one jointure of £1,000 and pin money of £500 to Lady Doneraile absorbed 22 percent of the rents received (£6,807.5) in 1890. Interest payments of £800 on one mortgage (for £20,000) and some other loans consumed 46.1 percent of all receipts that year. Doneraile MSS, NLI.

⁴⁴ John Mulholland borrowed £120,000 from the RCB in July 1873 and set a record for promptness by repaying the whole amount on January 7, 1881. He may have used this sum to buy new “plant” for his factory, the York Street Spinning Company, in Belfast or to complete the purchase of the Ballywalter Park estate in county Down from his own mortgagor, the Earl of Dufferin, to whom he had loaned £135,346.9 by 1872. ML/RCB, D3. Also see note 68, below.

tious investor might have hesitated before lending money to an estate with such a history of financial as well as emotional instability, the FC apparently did not. Even though the countess was paying out roughly 55.7 percent of her estimated income in interest (£10,006.6 during the 1890s), she did not apparently fall into arrears, and the entire principal was paid off after the sale of fourteen thousand acres of the Cork estate in 1907–08.⁴⁵

What the FC could not have predicted, of course, was the calamitous effect of agricultural depression and agrarian turmoil upon landed property after 1878. These two intimately related phenomena scared off investors and lowered rental income to the point where marginally solvent estates became insolvent.⁴⁶ These estates ended up under receivership in the Land Judges Court or were offered for sale to occupying tenants under the Ashbourne Act and its successors. Aided and abetted by the fair rent clauses of the Land Act of 1881, rental income declined all over Ireland, and this downturn automatically increased the debt burden of incumbered owners. During the early 1880s many landlords wrote distressed letters to their creditors pleading for more time in which to pay the interest due as their tenants had paid little or no rent for months. The FC did call a halt to further mortgages at the height of the land war, but it could do nothing to safeguard those loans already made. The harsh realities of Irish agriculture and agrarian unrest explain why, by 1901, the arrears of interest owed to the RCB reached £134,899—a sum equal to one full year's interest on the £3.1 million invested in mortgages in that year.⁴⁷

THE SECOND POINT OF ENTRY into the problem of landed indebtedness may be called the Land Commission route. As shown earlier, landlords wishing to sell parts of their estates (tenanted land) under the land purchase acts had to submit a host of documents to the Land Commissioners, including a complete list of all outstanding charges or claims on the lands being sold. These Final Schedules of Incumbrances contain extremely valuable information about the various debts and charges that the majority of landlords carried through the nineteenth century.⁴⁸ The results of analyzing the incumbrances in a sample of one hundred and five FSIs may be seen in Table 3, which reveals an average debt burden of 16.3 percent for these vendors of land under the Wyndham Act of 1903.⁴⁹

⁴⁵ For the massive debts of the third Earl of Kingston, who was "of unsound mind," and the troubles of this estate during and after the Famine, see Donnelly, *Nineteenth-Century Cork*, 70, 113, 255, 278–82, 335, 337–47. Lady Kingston eventually sold 21,417.2 acres or 90.8 percent of her Cork estate under the Wyndham Act.

⁴⁶ Investors' fears about conditions in Ireland after 1878 were voiced by spokesmen for British and Scottish insurance and assurance companies, which had invested several million pounds in Irish mortgages. See Canadine, "Aristocratic Indebtedness," 646. In Ireland the mortgage market dried up rapidly for a few years during the land war, and needy landowners had a hard time finding loans at less than 6 percent. For a case in point, see page 358, below.

⁴⁷ RCB, *Annual Report*, 1901.

⁴⁸ For the inherent limitations of this evidence, see note 31, above. Whether the land court judge's decision to "disallow" any claim in the FSI meant that the claim itself was invalid or that the claimants had waived their claim or had failed to provide adequate documentation in support of same is not always clear. The FSIs are located in the files for each estate sold by the Estates Commissioners, under the Land Purchase Acts of 1903 and 1909, in the Records Branch of the Irish Land Commission [hereafter, EC/ILC].

⁴⁹ The one hundred and five FSIs here represent something other than a random sample. Not only are they

TABLE 3
FSI Sample of Landlord Indebtedness by Estate Valuation

<i>Estate Valuation in £</i>	<i>Number of Estates</i>	<i>Total Valuation in £</i>	<i>Total Incumbrances in £</i>	<i>Ratio: Mort./ Value</i>	<i>Annual Interest at 4½%</i>	<i>Debt Burden %</i>
SMALL:						
100-1,000	33	12,721.4	84,147.7	6.6	3,786.6	29.8
MEDIUM:						
1,000-5,000	33	92,320.1	475,469.8	5.2	21,396.1	23.2
LARGE:						
5,000-15,000	24	235,715.0	1,107,486.8	4.7	49,836.9	21.1
GREAT:						
Over 15,000	15	368,896.1	907,617.0	2.5	40,842.8	11.1
TOTALS:						
All Estates	105	709,652.6	2,574,721.3	3.6	115,862.4	16.3

NOTE: The incumbrances consist of mortgage loans and marriage portions, exclusive of annuities and jointures.

SOURCE: Estates Commissioners Papers, Records Branch, Irish Land Commission, Dublin.

Like the RCB data, the FSI sample underlines the heavy debt burden carried by owners of small estates. Although the percentage spread between the debt burdens on great and small estates is not as wide in this table as in the RCB sample (18.9 for the FSIs and 51.5 for the RCB mortgages), the relative solvency of the great estates stands out prominently in both.⁵⁰ The FSI data emphasize the truism that "richer is better"—at least where incumbrances are concerned. Owners of small estates had so much less with which to cushion themselves against the adverse conditions of the rural economy after 1878 and before the sale of land became the obvious solution to agrarian discontents.

Even more than the RCB data, these FSIs represent a minimal state of indebtedness, because these FSIs rarely derive from the very first sale of land by an owner and because none of the FSIs covers the entire estate owned by the vendor. At least half of the vendors in this sample may have sold some land be-

too few to constitute an adequate measure but there are proportionately more FSIs for the eastern than for the western counties, which raises the possibility that the data are skewed in favor of more solvent properties.

⁵⁰ In other words, the debt burdens in the RCB sample range from 19.4 percent for the "great" estates to 70.9 percent for the "small," while the range in the FSI sample is 11.1 percent for "great" to 29.8 percent for "small" estates.

fore this particular sale, and these previous sales would have enabled them to redeem some of their charges or debts. Naturally, any incumbrances discharged at an earlier time would not show up on subsequent FSIs.⁵¹

Despite the artificial nature of most administrative boundaries there is something to be said for classifying landed indebtedness according to region or province. For years economists and agricultural experts have recognized distinctions between the eastern and western parts of the country—roughly, a distinction equivalent to the division between Ulster (excluding Donegal) and Leinster on the one hand and Munster and Connaught on the other. The contrast in resources and wealth between northeast Ulster and western Connaught represents the extreme limit of these regional variations, some of which persist to this day.⁵² Analysis of the mortgage data from our two sources by the province in which the bulk of each mortgagor's property lay shows Ulster as the undisputed leader in terms of solvency or lightness of debt burden. As Tables 4 and 5 indicate, these northern landowners had to divert significantly less of their income to interest payments than did their nearest rivals in Leinster.

The apparent discrepancy between the overall debt burdens of the two samples—27.5 percent for the RCB mortgages and 16.3 percent for the FSI landlords—reflects the basic difference between these sources. The RCB ledgers contain, naturally enough, the names of known mortgagors, whereas the FSIs concern landlords whose lands may or may not have been incumbered, or vendors who may have redeemed their mortgages through earlier sales. Both samples are clearly too small to warrant unequivocal conclusions about the economic condition of all Irish landowners; and the figures in these two tables are slightly skewed by the fact that the largest owners often had estates in more than one province.⁵³ In Table 4 column 7 represents the value of all land owned by the vendor in the province where the sale occurred, and column 8 contains the valuation of all land owned by the vendor in every province. If we assume that the landlords in this sample had no incumbrances other than those charged on the lands being sold (a very risky assumption indeed), then we may decide that column 8 contains a more accurate impression of landlord indebtedness. If, on the other hand, we are concerned with indebtedness by province, then the figures in column 7 should be taken as a rough index of solvency.⁵⁴ In either case the disparity between the debt burdens of the Ulster landlords and those in the

⁵¹ In many cases the FSIs for earlier sales under the Wyndham Act were missing from the Estates Commissioners' files. Some of these missing FSIs may have been extracted and retained by solicitors acting on behalf of the vendor. The Allocation Schedules, which contain much the same information as the FSIs and which seem to have been stored elsewhere in the Land Commission, were available in only a few cases.

⁵² For discussions of regionalism in Ireland, see T. W. Freeman, *Pre-Famine Ireland: A Study in Historical Geography* (Manchester, Eng., 1957); S. H. Cousens, "Regional Death Rates in Ireland during the Great Famine," *Population Studies*, 14 (1960): 55–74, and his two articles on emigration and population change, *Economic History Review*, 14 (1961): 275–88, and 17 (1964): 301–21; and Ó Tuathaigh, *Ireland before the Famine*, chap. 4.

⁵³ This is especially true of Baron Ashtown, Viscount de Vesci, the Marquess of Downshire, the Earl of Longford, and Viscount Massereene, all of whom owned estates in more than one province.

⁵⁴ As Table 4 reveals, the average debt burden of the one hundred and five landlords in the FSI sample comes to 16.3 percent on the provincial basis and to 14.7 percent counting all land owned in Ireland. If annuities, jointures, judgments for debt, and bank overdrafts are included, then the provincial debt burden becomes 20.3 percent and the all-Ireland debt burden rises to 18.2 percent.

TABLE 4
Distribution of FSI Mortgages by Province

Province	Number of Estates	Total Mortgage Loans	Portions for Younger Children	Total Incumbrances in £	Annual Interest at 4½%	Valuation by Province	Valuation of All Land in Ireland	Debt Burden % Pro- vincial Value	Debt Burden % All Ireland Value
CONNAUGHT	13	569,587.9	107,357.4	676,945.3	30,462.5	76,339.3	101,233.9	39.9	30.1
LEINSTER	37	479,410.9	378,810.1	858,221.0	38,620.0	207,839.6	247,564.8	18.6	15.6
MUNSTER	26	148,123.3	203,676.7	351,800.0	15,831.0	77,672.5	82,391.5	20.4	19.2
ULSTER	29	536,092.2	151,662.8	687,755.0	30,949.0	347,801.1	358,248.7	8.9	8.6
ALL IRELAND	105	1,733,214.3	841,507.0	2,574,721.3	115,862.5	709,652.5	789,438.9	16.3	14.7

NOTE: The valuation categories represent, first, the valuation of the mortgagor's estate in the province wherein his principal property lay and, second, the valuation of all of the land owned by the mortgagor in every province.

SOURCE: Estates Commissioners Papers, Records Branch, Irish Land Commission, Dublin.

TABLE 5
Distribution of RCB Mortgages by Province

<i>Province</i>	<i>Number of Estates</i>	<i>Total Valuation in £</i>	<i>Total Mortgage Loans</i>	<i>Ratio: Mort./ Value</i>	<i>Annual Interest Due</i>	<i>Debt Burden %</i>
CONNAUGHT	19	93,587.0	603,854.1	6.5	25,868.4	27.6
LEINSTER	39	152,639.4	1,068,474.3	7.0	45,333.2	29.7
MUNSTER	39	157,482.3	1,111,422.0	7.1	48,714.1	30.9
ULSTER	23	196,579.3	1,028,040.0	5.2	44,695.5	22.8
ALL IRELAND	120	600,088.0	3,811,790.4	6.4	164,611.2	27.4

SOURCE: Mortgage Ledgers D1-D3, Y, RCB MSS, Church House, Rathmines, Dublin.

other provinces, especially Connaught, remains distinctive. The debt burden of the Ulster landlords in the FSI group averages 8.6 percent compared with the 19.7 percent average for those in the other three provinces.⁵⁵ These figures should be treated with the caution due to anything less than an adequate sample. But they do point toward the relative success of these northeastern landlords in managing their estates and their debts more efficiently than did their counterparts in the rest of Ireland.

Table 6 reduces the FSI figures on provincial indebtedness to mean averages (using the all-Ireland basis for valuation). The percentages of debt burden are the same as those in Table 4, but an attempt has been made to distinguish mortgage loans from portions for younger children.⁵⁶ The results of this separation,

⁵⁵ The Ulster sample does contain a higher proportion of wealthy and aristocratic owners than do the other provincial samples. Slightly over half of these Ulster landowners are peers, and the percentage of "great" estates here is 27.6, compared with 15.4 percent for Connaught, 10.8 percent for Leinster, and 3.8 percent for Munster. This bias in the Ulster sample accounts for only a part of the solvency of the estates in this sample. If all of the "great" estates are excluded from the FSI sample and the debt burdens for the remaining ninety owners of estates valued at less than £15,000 are estimated, the Ulster estates still emerge as the most solvent. In ascending order of debt burdens by province the figures are Ulster, 6.5 percent; Leinster, 28 percent; Munster, 29.2 percent; and Connaught, 53 percent. The lowest debt burden in the entire sample—a mere 4.1 percent—belongs to the five Ulster landlords who are in the "medium" category. The lighter debt burden of the Ulster landlords cannot, therefore, be explained away simply by the presence of so many magnates.

⁵⁶ The distinction between ordinary mortgage loans and marriage portions is sometimes blurred, and the incumbrances listed in the FSIs and outlined in the vendor's Originating Application may have involved family charges without specific indication thereof. In most cases, however, the abstracts of the incumbrances listed in the FSIs contain some clues about the presence or absence of portions for younger children. Mortgages arising out of marriage settlements usually point to portions and contingency jointures. Unfortunately, the FSIs remain silent about the causes of mortgage loans for nonfamilial purposes. In Table 6 the last column refer-

TABLE 6
Mean Average Debt Burden of FSI Landlords by Province

Province	Number of Estates	Mortgage Loans in £	Portions for Younger Children	Total Incumbrances in £	Annual Interest at 4½%	Valuation by Province	Valuation of All Land in Ireland	Debt Burden % All Ireland Valuation	Average Outlay on Other Debts and Charges
CONNAUGHT	13	43,814.5	8,258.3	52,072.7	2,343.3	5,872.3	7,787.2	30.1	177.5
LEINSTER	37	12,957.1	10,238.1	23,195.2	1,043.8	5,617.3	6,690.9	15.6	346.9
MUNSTER	26	5,697.1	7,833.7	13,530.8	608.9	2,987.4	3,168.9	19.2	121.0
ULSTER	29	18,485.9	5,229.8	23,715.7	1,067.2	11,993.1	12,353.4	8.6	330.3
ALL IRELAND	105	16,506.8	8,014.4	24,521.2	1,103.5	6,758.6	7,518.5	14.7	265.4

NOTE: In the last column, the figures represent the average annual outlay on other debts and charges, including payments on annuities, jointures, bank overdrafts, legacies, and judgments for debt.

SOURCE: Estates Commissioners Papers, Records Branch, Irish Land Commission, Dublin.

however imprecise, reveal that marriage settlements cost the landowners of Munster and Leinster more (57.9 percent and 44.1 percent, respectively, of total incumbrances) than those in Ulster and Connaught (32.7 percent and 15.9 percent, respectively). Perhaps the incumbered owners in Connaught could not afford generous marriage settlements owing to the heavy debts they had inherited. The Ulster landowners in this sample seem to have kept a tighter rein on marriage portions than did their counterparts in Leinster and Munster. The fact that the Ulster sample contains more magnates or wealthy peers than do the other provincial samples (witness the high average valuation of the Ulster owners in Table 6) may go far to explain the light debt burden of these northern landlords. But such distinctive features of the northeast as systematic improvement of land, efficient estate management, close ties to the linen textile industry, and the widespread recognition of tenant right or the tenant's interest in his own holding must also be taken into account as reasons for the superior economic performance of the Ulster landlords in this sample.⁵⁷

UP TO THIS POINT the extent of landed indebtedness has been hypothesized by equating valuation with rental income and by assuming that these landowners had no other debts or sources of income apart from those indicated. Such assumptions can and do mislead, especially where the income factor is concerned. What about the realities? Do the actual rentals and accounts of Irish estates confirm or contradict the debt burdens estimated above? A partial answer to this question derives from the records of ten estates located in various parts of the country.⁵⁸

The mean average debt burden on these estates comes to 27 percent and the rental income factor places three of them in the "great" category and five in the "large."⁵⁹ Contrary to the results obtained in the RCB and FSI samples, the debt burdens of the four Ulster estates in this group surpass those of the other six by a margin of 33.7 percent to 18.8 percent. But the Duke of Abercorn's heavy debt burden (40.2 percent) accounts for much of this disparity, and, if his liabilities are eliminated, the debt burden of the three remaining Ulster land-

ring to the "Average Annual Outlay on Other Debts and Charges" represents an assessment of 4.5 percent interest on all contingency jointures, annuities, judgments for debt, bank overdrafts, and legacies. The additional annual outlay on these other liabilities comes to a total of £27,869.9, and, by combining this burden with the amount spent on servicing the mortgage loans, the total debt burden becomes 18.2 percent for the landlords in this sample. EC/ILC.

⁵⁷ The Ulster estates had lighter debt burdens in every category of wealth but one: in the "great" estates category, the four Leinster magnates in Table 6 paid an average of 6.7 percent of rental income on interest, compared with 10.5 percent for the eight Ulster magnates. EC/ILC.

⁵⁸ For permission to consult and publish the rentals and accounts needed to construct Table 7, I would like to thank the owners or trustees of the Antrim, Clonbrock, Crofton, Bellew, Inchiquin, Ormonde, Ranfurly, Stewart, and Wheeler-Cuffe papers. In this connection special thanks are due to the Hon. Bryan Bellew, the Hon. Grania O'Brien, and Captain A. C. Tupper. Lord Bellew's interest payment here is a net figure. In this year he earned £1,012.2 in interest on one mortgage loan, and he paid out £2,042.5 on his own loans. Bellew MSS, Barmeath Castle, Dunleer, county Louth.

⁵⁹ The average valuation of these ten estates—£12,704.8—exceeded average rents received in the years indicated—£12,563.2—by only 1.1 percent.

TABLE 7
Indebtedness on Ten Estates

<i>Name of Owner</i>	<i>Owner's County Seal</i>	<i>Valuation of Estate in £</i>	<i>Rents Received in £</i>	<i>Date of Rental</i>	<i>Interest Paid on Mortgages</i>	<i>Est. Amount of Mortgages at 4½%</i>	<i>Debt Burden %</i>	<i>Other Charges and Loans</i>
ABERCORN	Tyr.	35,802	35,936.5	1886-87	14,447.3	321,051.1	40.2	0.0
ANTRIM	Ant.	20,910	15,790.0	1875	4,273.8	94,973.3	27.1	2,782.2
BELLEW	Lou.	5,093	3,166.9	1905	1,030.3	22,895.6	32.5	0.0
CLONBROCK	Gal.	11,442	10,425.2	1880-84	395.4	8,786.7	3.8	0.0
CROFTON	Ros.	6,386	5,953.8	1865	1,911.7	42,482.2	32.1	0.0
INCHQUIN	Cla.	11,681	13,691.5	1878-87	4,548.8	101,084.4	33.2	1,173.5
ORMONDE	Kilk.	16,357	21,168.3	1880-84	2,525.3	56,117.8	11.9	178.1
RANFURLY	Tyr.	11,237	10,772.8	1902	1,693.8	37,640.0	15.7	32.5
STEWART	Tyr.	6,752	6,841.9	1876	2,968.2	65,960.0	43.4	400.0
WHEELER-CUFFE	Kilk.	1,388	1,885.0	1871	185.0	4,111.1	9.8	500.0
TOTALS		127,048	125,631.9		33,979.6	755,102.2	27.0	5,066.3

NOTE: I have estimated all mortgages at 4.5 percent interest, even though some of the owners were paying closer to 4 percent interest on their loans.

owners falls to 26.8 percent.⁶⁰ Of course, this sample is far too small to undermine the hypothetical debt burdens found in Tables 4 and 5. Although the average debt burden of these ten estates does come close to the averages in the RCB and FSI samples, the data in Table 7 emphasize the tentativeness of the provincial estimates. In several cases, the dimensions of indebtedness turn entirely upon the date of the sample: a landowner could be relatively unincumbered at one point in time and then find it necessary, especially after 1880, to borrow large sums of money in order to compensate for huge arrears of rent.⁶¹ The data in Table 7 also point up the danger of averaging small samples: the massive debts of only one or two landowners can easily distort the debt burdens of several dozen modest mortgagors in the same cohort.⁶² There were, after all, some prudent Irish landlords who defied the popular stereotype by living within their limited—or diminishing—means.

The records of individual estates contain a spectrum of solvency, marginal affluence, and rapidly approaching insolvency. The difficulty lies in finding the typical estate for any particular region, time, and cohort. Starting with extremes of solvency, one looks in vain for significant incumbrances on the vast Fitzwilliam estate surrounding Coollattin in county Wicklow, on Sir John Leslie's estate at Glasslough, county Monaghan, or on the Earl of Erne's "great" estate in county Fermanagh.⁶³ The distinct advantage of owning urban property becomes apparent in the case of the Earl of Longford, whose agricultural rents were supplemented and then bolstered by rising income from the suburban estate in Kingstown (Dun Laoighaire), which he owned jointly with Viscount de Vesci. The combination of a substantial income in the 1870s—averaging £21,262.8 during 1870–74—and the termination of a long-lived and costly jointure in 1880 enabled Lord Longford to reduce his debt burden from 31.2 percent in the early 1870s to a mere 6.7 percent in the early 1880s.⁶⁴

⁶⁰ According to the Duke of Abercorn's agent, who provided a summary of the "Permanent Charges" on the estate in 1886, "estate charges" cos: the duke £9,371.6 and "family charges" amounted to £14,447.3 in that year. At 4 percent interest the principal of the latter would have been £361,181.3. Tithe-rent charges, charges on Church lands, and a Board of Works loan for land improvement have been excluded. Interest on family charges ate up 40.2 percent of the duke's rental income in 1886–87. Between 1886 and 1890 he sold 66.1 percent of his Donegal estate and 27.5 percent of his Tyrone estate for a total of £279,008, and he used this money to pay off most of these charges. By 1889, the duke's debt burden could not have been much more than 13 percent of rents received (£25,235.6). Abercorn MSS, PRONI, D. 2400/1/60; and *PP*, 1889, 61, no. 81, and *PP*, 1890, 60, no. 115. Sir John Marcus Stewart of Ballygawley Park, county Tyrone, had twenty-two different charges or mortgages on his estate in addition to a jointure of £400 paid to Lady Elizabeth Stewart. In 1877 his debt burden, excluding the jointure, came to 43.4 percent of rents received (£6,841.9). PRONI, D. 1716/3A–5A.

⁶¹ Lord Ranfurly's interest payments and annuities, for example, came to only 1.3 percent of rental income (approximately £13,000) in 1872. But, when he sold most of his Tyrone estate in 1904–05, the accounts revealed mortgages totaling £38,500. Interest payments thereon amounted to 15.7 percent of receipts in 1902. Ranfurly MSS, PRONI, D. 1932/4/33–34.

⁶² If the three most heavily incumbered owners are excluded from this sample (their average debt burden was 38.9 percent), the debt burden of the remaining seven falls to 17.4 percent.

⁶³ On Lord Fitzwilliam's estates in Wicklow, Wexford, and Kildare, the agent paid out a mere £633.8 in interest and annuities during 1885–86. The 89,891 acres of this estate were valued at £46,444. Fitzwilliam MSS, NLI. Leslie's Glasslough estate in county Monaghan was valued at £11,540, and his Pettigo estate on the border of Donegal and Fermanagh at £4,126. He owned a total of 49,968 acres in six counties (almost all in Ulster), valued at £21,051. In the 1870s he was paying out around £25 in interest (on only one loan).

⁶⁴ In 1844–45, Edward, the third Earl of Longford, borrowed £50,000 to cover marriage portions and other expenses. The interest payments of William, the fourth earl, fell from £2,948.2 in 1874–75 to £267.2 in 1876–

On the opposite end of the spectrum could be found hundreds and, after 1880, thousands of insolvent estates in receivership. Hard times forced the owners of heavily incumbered estates to enter the land market at a time when prices were low and mortgage money scarce. The number of estates in Ireland over which receivers were appointed by court order rose dramatically from one hundred and sixty-five in 1881 to six hundred and eighty-nine in 1886. Between 1883 and 1887 the Land Judges Court, which had inherited the functions of the Incumbered Estates Court, approved the sale of four hundred and six of these estates for the sum of £1,406,023.6—the average price being £3,463.1.⁶⁵ In the absence of the internal records of these financially stricken estates one can only surmise that the lack of adequate credit as well as a crushing debt burden resulted in the high casualty rate among small estates during a time of depression and agrarian unrest.

In the middle range of marginal solvency could be found landlords like Sir William Verner, third baronet (1856–86), of Church Hill, Verner's Bridge, county Armagh. Sir William inherited 24,144 acres in four counties, valued at £12,820, along with a mortgage of £10,000 and marriage portions to the sum of £18,000. The poorer tenants on the Armagh and Tyrone estates could no longer pay the full rent due after 1879, and, as the arrears mounted, so too did the anxieties of creditors to whom Sir William owed money. His long-suffering agent, James Crossle, spent much time in the early 1880s searching for a mortgage loan of some £60,000 at 4.25 percent or 4.5 percent, presumably to stave off the foreclosure of other mortgages. Evidently the Dublin moneylenders considered this estate a poor risk because they refused to lend money at less than 5 percent. By 1882, several potential creditors were talking 6 percent. Eventually, the agent and the family solicitor in London found someone prepared to advance £60,000, but the struggle to pay bills and meet interest payments on time continued, and Lady Verner's demands for more remittances from the agent did not help matters. The cumulative strain of trying to keep this estate solvent took its toll on Sir William's health and may well have hastened his death in 1886 at the age of thirty.⁶⁶

77. Georgiana, dowager Countess of Longford, survived her husband, Thomas, second earl (who died in 1835) by almost forty-five years. At £2,000 per annum, hers was a costly jointure by almost any standard. Selina, the widow of William, the fourth Earl of Longford, also received a jointure of £2,000 after his death in 1887. Good grazing lands in Longford and Westmeath, efficient management by a professional land agency in Dublin, and receipts from the Kingstown estate kept Lord Longford's rental income at an average of £21,199.3 during 1881–85. Longford MSS, Tullynally Castle, Castlepollard, county Westmeath.

⁶⁵ These were mostly small estates burdened by heavy arrears as well as incumbrances. The one hundred and sixty-five insolvent estates in 1881–82 had an average rental of £599.4, and the six hundred and eighty-nine estates in this category in 1886–87 had an average rental of £513.9. In this latter case the rents received in 1887 represented 61.2 percent of the rents due—a figure expressive of mountainous arrears. "Return to an Order of the House of Commons . . . relating to the Landed Estates Court, Land Judges Branch of the Court of Chancery (Ireland)," *PP*, 1888, 83, no. 400. Also see "Returns of Purchase Money Realised by Sale of Estates in the Landed Estates Court, 1870–79," *PP*, 1880, 60, no. 303. A government return, dated February 1890, contains the names of over two thousand properties "under the control" of the Land Judges Court since January 1, 1878. *PP*, 1890, 60, no. 74.

⁶⁶ See *Copy Out-Letter Books of James and Henry Crossle, Agents for the Verner Estate in Cos. Armagh, Tyrone and Monaghan, 1857–1886* (issued by the PRONI; Belfast, n.d. [1975]). On May 2, 1882, James Crossle, Sir William's land agent, wrote to the Verner's London solicitor, "We are very uneasy about the estate payments; if we cannot meet the interest of the mortgages they will be foreclosed. Last year we were able to borrow 'untill the rents

Several great estates also foundered in a sea of debts after the late 1870s. But these may be instances of exceptions proving the rule. The earls of Lucan had managed to run up mortgages amounting to some £246,258 charged on their vast estate in county Mayo, and only extensive sales of land in 1905 extricated the family from a debt burden approaching 85 percent of estimated income.⁶⁷ Then, too, there is the saga of the first Earl and Marquess of Dufferin and Ava, who inherited an estate in north county Down valued at £16,752. Dufferin was no hedonist or gambler but an ambitious and thoughtful politician with Whigish leanings. In pursuit of high office and ambassadorial pomp, and anxious to be known as an improving landlord, he began to overspend his income by thousands of pounds during the 1860s.⁶⁸ When he took over the estate in 1847, incumbrances came to £29,261.5. By 1862 the estate had been charged with loans to the amount of £142,157.5, and ten years later this figure reached the high-water mark of £304,824. By 1872, Lord Dufferin had exhausted his income and most of his credit. Gladstone's tamperings with landlords' rights in 1870 made Dufferin all the more nervous about his future as a heavily incumbered landowner. With average receipts in the early 1870s of £21,181.2, Dufferin was paying close to £13,717.1 in interest (reckoned at 4.5 percent) plus the sum of £4,115.6 in annuities and jointures to family members. His total debt burden must have come close to 84.2 percent.⁶⁹ Having seen the writing on the wall, Dufferin decided in 1874 to sell the bulk of his estate in the Landed Estates Court. Sales began soon after, and by 1880 he had realized £370,042 in purchase money—a sum that enabled him to discharge all of his incumbrances while leaving a residue of £54,850, most of which he invested in Canadian property.⁷⁰ Lord Dufferin was one of the very few great landowners to sell his estate well before the era of land purchase acts. The staggering size of his debt burden explains why his decision was not wholly voluntary. If he succeeded in escaping from the crushing burden of those debts, he did not elude disputes over tenant right with some of his former tenants.⁷¹

came in' but this year it could not be managed either from banks or private individuals. The charges upon this estate now (particularly in such very unfortunate times) are very heavy and . . . the expenditure is far in advance of the incomings." PRONI, D. 236/488/3, 112.

⁶⁷ Excluding a land improvement loan of £5,700 and a contingency jointure of £2,000, Lord Lucan's incumbrances amounted to a total of £240,557.9, of which marriage portions accounted for roughly £34,666.6. The valuation of his lands in Mayo and Dublin came to £13,119.5. Figuring interest at 4.5 percent, his debt burden on the mortgages would have been some 82.5 percent. The sale of Lucan's Mayo estate in 1905 realized £102,370.8, plus a bonus of £12,284.5, almost all of which went to pay off claimants. FSI, E.C. 656 and C.D.B. 1146, EC/ILC.

⁶⁸ Lord Dufferin claimed that he had spent over £100,000 on improvements to his estate, but there is no proof that this was the real cause of his financial embarrassment. See Sir Alfred Lyall, *The Life of the Marquis of Dufferin and Ava*, 1 (London, 1905): 185, 192. For Dufferin's indebtedness to John Mulholland, see note 44, above. Dufferin was a keen student of land law and wrote memoranda as well as pamphlets about landlord-tenant relations in Ireland.

⁶⁹ These figures derive from the Dufferin accounts, PRONI, D. 1071A/C3/39-40, D. 1071A/N4, and D. 1071A/B4/1. I am most grateful to Andrew Harrison and Patrick Geary for drawing these documents to my attention.

⁷⁰ For particulars of these sales, see PRONI, D. 1071A/C3/40. Five wealthy businessmen from the Belfast area, including John Mulholland, bought most of the estate.

⁷¹ For Dufferin's painful disputes with some of his former tenants over compensation for improvements to their holdings, see Lyall, *Life of the Marquis of Dufferin*, 188-201. Dufferin expressed his views on landlord-tenant relations in a pamphlet, *Contributions to an Inquiry into the State of Ireland* (London, 1866).

If space permitted, many other case histories could be cited showing how hard-pressed landowners tried to cope with their debts. But the purpose here is to generalize not particularize. Although the mortgage data and the estate accounts remain silent about the reasons for all this mortgaging, the scattered evidence does suggest that once the economy had recovered from the worst effects of the Famine, landowners already burdened with family charges and other liabilities jumped at the chance offered by both private and institutional lenders, themselves in search of a good investment, to consolidate all of their debts in one giant loan at rates ranging from 4 to 4.5 percent.⁷² In this way an incumbered owner could pay off the old incumbrancers and greatly simplify his interest payments to the new creditor who had a first charge on the lands in question. No doubt the hard times after 1878 drove many owners to seek loans that would permit them to maintain their affluent life-style. But when creditors began to shy away from investing in Irish land during the land war, incumbered owners had a hard time finding mortgages at less than 5.5 or even 6.5 percent.

Although distinctions between ordinary mortgage loans and family charges are often difficult to draw, the FSI's do contain some clues about the role of marriage settlements in causing indebtedness. Roughly one-third of the incumbrances redeemed at the time of sale involved portions for younger children, and the proportion may well have been higher than the figures shown in Tables 4 and 6.⁷³ If this estimate applied to the rest of the landowning class, then the amount of capital tied up in this way must have run into millions of pounds. Had these portions and jointures been invested in productive capital ventures, the landlords of Ireland might not have been so vulnerable to rent strikes and falling agricultural prices. But in the long run most landlords cared more about preserving status than maximizing profit. Excepting such fortunate owners of Dublin city property as Lords Pembroke, Meath, Carysfort, de Vesci, and Longford,⁷⁴ the majority of landlords were forced to reduce their personal expenses

⁷² Among the institutional lenders in Ireland, besides the RCB and various banks, were the Presbyterian Church, which had advanced £121,000 to the ninth Earl of Shaftesbury by 1872, and St. Patrick's College, Maynooth, which loaned a total of £314,670.2 to seven mortgagors in the 1870s. See PRONI, D. 811/257/1; and Mortgage Papers, Maynooth. British insurance companies loaned several millions to Irish as well as English landowners during the nineteenth century. For example, in 1847 the Law Life Assurance Society allowed a mortgage for £160,000 to Thomas Martin of Ballinahinch and a few years later "bought in" this insolvent estate in Connemara for £186,000. By 1894 the Earl of Lucan owed £191,969.8 to both the Law Life and the Eagle Life Assurance companies. By 1902, Lord Charlemont had borrowed £205,000 from the Royal Exchange Assurance Company. And so on. See Padraig G. Lane, "The Management of Estates by Financial Corporations in Ireland after the Famine," *Studia Hibernica*, 14 (1974): 67-89; FSI (Lucan), E.C. 656, EC/ILC; and Cannadine, "Aristocratic Indebtedness," 637.

⁷³ Gerald, fifth Duke of Leinster, inherited in 1887 incumbrances totaling almost £292,077, at least half of which consisted of portions for younger children (approximately £154,000). The pressure of these and other debts moved the trustees to sell £256,204 worth of the county Kildare estate under the Ashbourne Act (1885-91). The remainder of the Kildare and Meath estate, excluding Carton demesne and some lands in hand, was sold in the early 1900s for a grand total of £676,038. *PP*, 1889, 61, no. 81: 12; *PP*, 1890, 25, C. 6202 and 60, no. 115; *PP*, 1890-91, 25, C. 6384 and C. 6508; *PP*, 1907, 70, Cd. 3447, Cd. 3547, and Cd. 3531. Also see FSI (Leinster), E.C. 82-85, EC/ILC.

⁷⁴ The combined valuation of the Dublin property owned by these five peers amounted to £82,857, but this figure may well mislead because income from the ground rents steadily increased after 1870. Gross receipts on Lord Pembroke's great urban estate in south Dublin came to £31,835 in 1870, £35,339.3 in 1880, and £36,923.5 in 1890. Pembroke Rentals, PRO, Dublin.

rather severely after 1878, and almost all of them curtailed the already modest sums being spent on improvements to the estate.⁷⁵

Landowners who succeeded to estates after the 1850s, and especially those stirred by evangelical imperatives, tended to act more frugally than their fathers and grandfathers had done. In the post-Famine era many owners worked hard to make their home farms as well as their estates pay. They could usually rely on more efficient or less unscrupulous land agents to help them achieve this end, even though the quality of agents varied widely between the amateur, feckless nephew or cousin of the owner and the highly professional land agency firm with headquarters in Dublin or Belfast. What the landlords did not count on was the steady erosion of deference among their tenants and the fall in agricultural prices during the 1870s, two processes that culminated in the land war of 1879–82. The rapidity and severity of the agricultural crisis caught many landowners by surprise, particularly those who had embarked on ambitious projects to expand demesne farming, modernize the Big House, or enhance the pleasure grounds. In the easy credit situation of the 1860s and early 1870s landowners had no trouble in financing these ventures through loans. Then came the succession of bad harvests, falling prices, declining rental, agrarian agitation directed at landlordism, a tight mortgage market, and defaults on mortgage payments. The dramatic rise in the number of estates passing from solvency to insolvency in the 1880s bears mute but irrefutable witness to the impact of these blows on Irish landed society. For landowners saddled with the heaviest debt burdens the day or month of reckoning came that much sooner and with painful finality.

At the end of his article on Irish landed wealth, David Large raised an important question about the timing and, hence, the motivation of the debts incurred by the few landowners in his sample. Although lacking statistical evidence, he speculated that, contrary to Tocqueville's view about heavy borrowing between 1815 and 1849, "indebtedness was a serious matter for many landowning families" before 1815.⁷⁶ The evidence gathered for the present study would suggest a different conclusion or set of inferences. The chronology of indebtedness herein shows a distinct preponderance of mortgage activity after, rather than before, the Famine. No doubt many landlords had to sell some or all of their estates in the Incumbered Estates Court simply to avoid bankruptcy proceedings. But the FSIs reveal a series of strenuous efforts dating from the 1850s through the 1870s to refinance family charges and other liabilities contracted in more prosperous

⁷⁵ The amounts spent on agricultural improvements by Irish landlords varied widely. To confuse the issue some agents tended to count as improvements money spent on the gardens or pleasure grounds, plantations, and game-keeping. For political reasons many landlords exaggerated the size of their investment in capital improvements to the estate. Analysis of the accounts of four estates (Cosby in Queen's county, Dunraven in Limerick, Longford in Westmeath and Longford, and Crofton in Roscommon) reveals the following percentages of rental income spent on permanent improvements between the 1860s and the 1880s: 9.6 percent, 8.8 percent, 24.8 percent, and 6.9 percent, respectively. Cosby MSS (Stradbally); Dunraven MSS (now on deposit in the PRONI); Longford MSS (Tullyally Castle); and Crofton MSS (NLI). Much more work needs to be done on estate accounts before any firm conclusions can be drawn about the average expenditure by Irish landowners on this important item.

⁷⁶ Large, "The Wealth of the Greater Irish Landowners," 43–44.

TABLE 8
The Chronology of Indebtedness:
Distribution by Decade of 270 Mortgages

<i>Decade</i>	<i>Number of Mortgage Loans</i>	<i>Amount of Mortgage Loans</i>	<i>Average Size of Loan</i>	<i>Percent of Total Mortgage Loans</i>
1770-79	2	18,000.0	9,000.0	0.9
1780-89	3	25,115.4	8,371.8	1.3
1790-99	5	51,600.0	10,320.0	2.6
1800-09	1	6,261.5	6,261.5	0.3
1810-19	7	101,078.3	14,439.8	5.1
1820-29	4	44,000.0	11,000.0	2.2
1830-39	13	143,120.6	11,009.3	7.2
1840-49	16	203,130.7	12,695.7	10.2
1850-59	21	268,066.5	12,765.1	13.5
1860-69	38	378,727.6	9,966.5	19.1
1870-79	38	269,869.6	7,101.8	13.6
1880-89	29	119,475.6	4,119.8	6.0
1890-99	48	180,328.0	3,756.8	9.1
1900-09	37	151,445.6	4,093.1	7.6
1910-19	8	26,200.0	3,275.0	1.3
TOTALS	270	1,986,419.4	7,357.1	100.0

SOURCE: See note 77, below.

times. In a number of cases the marriage settlements of the 1820s and 1830s fell due for payment—in the form of portions and jointures—after 1850. And, even if landowners did run up some huge debts before 1815, there can be no doubt about the zeal with which the landlords in this sample borrowed money after 1850. What remains unclear is how much of a role necessity rather than desire or vanity played in these transactions.

Table 8 shows the distribution by decade of two hundred and seventy separate mortgage loans culled from the FSIs of landlords in Leinster, Munster, and Connaught.⁷⁷ As can be seen, some 70.2 percent of all of these borrowings took place in the post-Famine period, whereas only 10.2 percent date from the last third of the eighteenth century. The extent of mortgaging in the thirty years after the Famine—46.2 percent of the total—may reflect a tendency to raise fresh capital or refinance old debts after a time of agricultural depression and when there were signs of stability or prosperity close at hand. The absence of concrete evidence about why these landlords borrowed when they did makes it necessary to seek clues among the skeletal remains of once vital and complex indentures of mortgage, the full circumstances of which can only be a matter of guesswork or conjecture.

MANY QUESTIONS ABOUT THE ORIGINS, nature, and effects of landed indebtedness in Ireland remain either unposed or unanswered in this study. Had more evidence survived, for example, it might have been possible to estimate the number of landowners who were free from incumbrances or mortgages. All that can be said on the basis of the FSI sample is that only sixteen of the one hundred and five landlords in this group (15.2 percent) had no mortgages listed against the lands being sold at that time.⁷⁸ Such unincumbered FSIs may, of course, reflect the fact that the vendor had sold land previously and had used the proceeds to discharge his outstanding debts. Even given the inherent limitations of the evidence, the mortgage data used in this study do point toward several conclusions.

First, the great or wealthiest estates seem to have carried on balance the lightest debt burdens of any category of owner regardless of the actual size of the mortgages. Conversely, the most heavily incumbered estates tended to be those with a net rental of less than £1,000. Because this study has focused on the Irish landlord class as a whole and not just on aristocratic owners, it is not possible to enter fully into the debate about the nature of aristocratic indebtedness in England, carried on in a “genteel” fashion by David Spring, F. M. L. Thompson,

⁷⁷ The data in Table 8 come from the FSIs for the vendors in the sample of these three provinces; EC/ILC. No distinctions have been made between old and new pounds. This sample of two hundred and seventy mortgages and charges contains fifty-three identifiable marriage settlements that provided portions to the amount of £542,605, or 27.3 percent of the face value of all of these incumbrances. But many of the other mortgages in this sample may have arisen out of earlier marriage settlements not listed in the FSIs sampled here.

⁷⁸ The Earl of Antrim, Baron Leconfield, the Earl of Carysfort, the Earl of Belmore, the Earl of Lanesborough, and the Marquess of Londonderry had no mortgage loans listed in their FSIs in this sample. When Lord Leconfield sold almost 95 percent of his county Clare estate in 1894, his FSIs contained no incumbrances. ILC 694C, EC/ILC.



Figure 1: The evils of landlordism. The nationalist cartoonist, Thomas Fitzpatrick, depicted a rapacious Anglo-Irish landlord steering the Tory government of Lord Salisbury toward another campaign of eviction and repression in Ireland. The torn and tattered coat, breeches, and boots are meant to suggest heavy debts or lack of rental income rather than simple sartorial neglect. Fitzpatrick's cartoon appeared in the *Weekly Freeman*, December 4, 1886.

and, more recently, David Cannadine.⁷⁹ The presence of an aristocratic element in the mortgage samples, however, makes it possible to assert that owners of "great" estates in Ireland behaved in most respects like their analogues in England: huge debts inherited from extravagant forebears were supported effectively by huge incomes. Strategic sales of land and occasionally shrewd investments in business ventures enabled these magnates to retain their wealth and status long after the lesser landowners with their heavier debt burdens had sold everything except the demesne and the family silver. As in England, so in Ireland, the wealthiest landowners could afford to live with mortgages and family charges in excess of £200,000—a burden that would have crushed a landowner worth even £10,000 a year. Admittedly, a severe depression, an imprudent marriage to an "actress," or a passion for gambling could jeopardize a great estate, but only if the debt burden was already onerous.⁸⁰ In spite of the pronounced differences between England and Ireland, Cannadine's reflections about English landowners apply with almost equal force to Irish landed society:

⁷⁹ Cannadine has summarized the main points in this debate and provided the bibliographical background; see his "Aristocratic Indebtedness," 624–50.

⁸⁰ An example of the ruinous effects of this combination of circumstances may be found in the case of Edward, seventh Duke of Leinster (1892–1976), who gambled away a fortune he did not expect to inherit (being the third son of Gerald, fifth Duke of Leinster). Around 1919 he sold his reversionary interest in the much-reduced estate to the English clothier and moneylender, Sir Harry Mallaby-Deeley, in order to raise some cash. When Edward finally succeeded to the estate and the title on the death of his eldest brother, Maurice, the sixth duke, in 1922, Sir Harry became the legal owner of the Carton property. W. A. Maguire has studied another aristocratic gambler's "progress" and the high cost of high living in an earlier period; see Maguire, "The 1822 Settlement of the Donegal Estates," *Irish Economic and Social History*, 3 (1976): 17–32.

Retrenchment, sales of land, new or enlarged debts, the abdication of traditional paternal and political roles—all of these activities characterized aristocrats of moderate means in the years from the 1870's. For the squirearchy and minor gentry beneath—those with incomes from £1,000 to £10,000 a year—the pressure of debt was greater, the impact of depression more marked, and decline in consequence more rapid and complete.⁸¹

From an Irish perspective this argument requires at least one important qualification: "ruinous debt," as distinct from tolerable debt burdens, played a far more active role in deciding the fate of Irish landlords outside Ulster than was the case in England.⁸²

The unequal distribution of wealth or debt burdens in Ireland goes some way to explain why so many small landlords could not afford to be too lenient toward their tenants' arrears and also why they usually had to back down before Land League threats of rent strikes and grant large abatements on the rents due after 1879.⁸³ These smaller and poorer landlords, moreover, had to sell more land sooner than they may have wished, and, if they did not sell voluntarily, the Land Judges Court was likely to appoint receivers and conduct the sales for them.

Second, there can be little doubt that mortgage loans and family charges weighed heavily on many Irish landlords and that the misfortunes of agriculture in the latter 1870s and 1880s drove many marginal estates into insolvency. Excepting the great estates and those landowners who were fortunate enough to own urban property or coal mines, the extent of this indebtedness runs directly counter to James S. Donnelly's optimistic assumption that the operation of the Incumbered Estates Courts after 1850 relieved the landlords of a "major problem"—indebtedness—and freed them to invest fruitfully in estate improvements.⁸⁴ The court certainly did disincumber landlords with the heaviest debt burdens in the 1840s and after, but the redistribution of almost five million acres under its auspices could not possibly have alleviated the problems of those owners who were caught up in the struggle to stay solvent.⁸⁵ As the RCB and FSI samples reveal, many landlords carried the burden of pre-Famine incumbrances right through the nineteenth and well into the twentieth century. And that burden became a "major problem" in the years after 1878 for landlords who were already spending upwards of 40 percent of their income on in-

⁸¹ Cannadine, "Aristocratic Indebtedness," 648.

⁸² Cannadine has concluded that "the distinction between early Victorian crisis and mid-Victorian recovery has perhaps been overdrawn; and . . . debt, but not ruinous debt, was a common feature of life for many [English] landed families." *Ibid.*, 649.

⁸³ Both the Land League and the Plan of Campaign organized rent strikes and other forms of agitation on estates known to be heavily incumbered and whose owners could not afford to hold out long (at least not without the help of an owners' syndicate) against tenants who withheld the rent. See L. P. Curtis, Jr., *Coercion and Conciliation in Ireland* (Princeton, 1963), 233–41, 248–52; and F. S. L. Lyons, "John Dillon and the Plan of Campaign, 1886–90," *Irish Historical Studies*, 14 (1965): 313–47.

⁸⁴ Donnelly's rather rosy view of the landlords' post-Famine adjustment deserves quoting: "The incubus of a largely bankrupt or debt-ridden landlord class was cast off by the operation of the incumbered estates court in the 1850s"; *Nineteenth-Century Cork*, 164. Also see Cannadine's comments on Donnelly's argument; "Aristocratic Indebtedness," 633, n. 6.

⁸⁵ For the operation of the Incumbered Estates Court, see John E. Pomfret, *The Struggle for Land in Ireland, 1800–1923* (Princeton, 1930), 43–46; and Padraig G. Lane, "The Encumbered Estates Court, Ireland, 1848–1849," *Economic and Social Review*, 19 (1972): 413–53.

terest and other charges.⁸⁶ When one adds to the mortgages all of the other compulsory payments faced each year by the landlords (head rents, crown rents, quit rents, tithe-rent charges, county cess, and poor rates), then the amount of "profit" in the "profit rent" shrinks all the more. Not until the passage of the land purchase acts after 1885 did these incumbered owners find a way out of a threatening situation, and even then their spokesmen in the House of Lords clamored for some kind of tangible compensation to atone for all their losses between 1880 and 1900.⁸⁷

Third, the analysis of the mortgage data suggests that Ulster landlords on the whole enjoyed a greater degree of solvency than those in the other provinces. For various reasons, including improvement of farm land, less troubled landlord-tenant relations, and their proximity to the flourishing linen textile, shirt-making, and ship-building industries, these northern landlords had more liquidity or capital at their disposal. In their politics and religion they were less alien members of provincial society than their counterparts in Munster or Connaught, and many of them had social as well as business ties to the commercial and industrial magnates of Belfast and other northern cities. The greater wealth of these northern landowners enabled them to invest in business ventures and to maintain their social and political ascendancy in the countryside.

Relatively unincumbered wealth made it possible for Ulster aristocrats like Lords Londonderry, Belmore, Erne, Downshire, Farnham, and Ranfurly to preserve their privileged position no matter how much land they may have sold after 1885. Landlords in the "south," on the other hand, were retreating on all fronts before the forces of democratic and Catholic nationalism. Prominent in leading as well as financing such antinationalist organizations as the Property Defence Association and the Irish Unionist Alliance, these northern landlords also helped to launch and supervise the Ulster Volunteer Force in 1912-14.⁸⁸ Ulster Unionism, in short, represented a formidable coalition of landed wealth, industrial-commercial capital, and working-class sectarianism. Relying heavily on the bigotry and the "muscle" of the Orange order, the leaders of Ulster Unionism played on Protestant working-class fears of an insurrection by the supposedly disloyal elements in Catholic working-class communities.⁸⁹ The

⁸⁶ As Barbara Solow has written, "In Ireland in the 1880's, the landlord class bore the brunt of the blows. Rents, traditionally lagging and sticky, were rapidly readjusted to the falling price level under the influence of the Land Acts. The landlords were left to be squeezed between their inflexible costs and their declining rent rolls." *The Land Question and the Irish Economy*, 181.

⁸⁷ See the debates in the House of Lords in 1899 and 1900, when such Representative Peers of Ireland as Lords Inchiquin, Clonbrock, Farnham, and Templetown made impassioned pleas for state loans and other benefits as compensation for some twenty years of "confiscation" of landlords' rights. *Hansard, Parliamentary Debates*, 4th ser., 74 (July 18, 1899): 1128-63, and 85 (July 6, 1900): 728-60.

⁸⁸ For passing allusions to the role of landlords in organizing Unionist resistance to Home Rule, see Fred H. Crawford, *Guns for Ulster* (Belfast, 1947), 10-19; H. S. Morrison, *Modern Ulster* (London, 1920), 158-68; A. T. Q. Stewart, *The Ulster Crisis* (London, 1967), 32, 37, 52, 65, 69-73, 93-95; and Patrick Buckland, *Irish Unionism: Two, Ulster Unionism and the Origins of Northern Ireland* (Dublin, 1973), 21, 51-53, 58-61, 86. The relationship between "old" landed wealth and "new" business money in shaping and financing Ulster Unionism has been neglected by historians and deserves much deeper study.

⁸⁹ For the case for the (imperial) exploitation of (colonial) sectarian and class animosities, see Liam de Paor, *Divided Ulster* (Harmondsworth, 1970). For a decidedly more structuralist and controversial account of the "hegemonic" nature of Ulster Unionism (in which the landlords played no serious role), see Peter Gibbon, *The Origins of Ulster Unionism* (Manchester, Eng., 1975).

solvency of Ulster's larger landlords and the strength of the sectarian context in which they operated provide some explanations as to why Unionism proved an irresistible force in Ulster. In political terms the real difference between "northern" and "southern" landlords was that the former managed to endure well into the twentieth century as a class, sharing power and to some extent property through investments with the moneyed or business elite, while clinging to the high status of the old Protestant Ascendancy. By contrast, even the relatively solvent landlords (or landowners after the compulsory Land Purchase Act of 1923) who stayed on in the "south" after World War I survived only as individuals or families. They could still savor privileges denied to the mass of the population, but the former landlord element in the Protestant Anglo-Irish minority grew ever more isolated from the main currents of Irish life that pulsed just beyond the demesne wall.

In the long run, the economically depressed and politically volatile conditions of the late 1870s and 1880s inflicted far more damage on landlords than tenants. Incumbered owners who had been diverting only 20 to 30 percent of their income to interest payments suddenly found themselves paying upwards of 40 to 50 percent of their reduced receipts to meet these same obligations. The combination of heavy incumbrances and declining incomes spurred landlords to sell most of their estates after 1903. By selling holdings or farms to the occupying tenants through the agency of the Land Commission these landlords extricated themselves from a hopeless financial bind with some honor and, perhaps, a little cash in hand, once all the claimants had been paid off.⁹⁰

This analysis of Irish landed indebtedness has deliberately stressed the liabilities and neglected the assets of the landlords. It would be absurd to imply, let alone argue, that Irish landlords were paupers in princely guise or that all of their mortgages, bonds, annuities, and jointures prevented them from enjoying the traditional preoccupations of that class—Big House partying, shooting, pursuing foxes and heiresses, drinking and gossiping in gentlemen's clubs, competing with the local priest for the minds (if not the hearts) of the tenantry, improving the pleasure grounds, and subscribing money to anti-Parnellite associations. In the eyes of their tenants and laborers, these landlords were so rich that they could well afford the abatements of rent that were first requested and then demanded after 1878. But the combination of heavy incumbrances inherited from the past and economic adversity in the latter 1870s hastened the fall of a class whose capacity to govern had been steadily declining—at least outside Ulster—since well before the Famine.

⁹⁰ The 12 Percent Bonus on the purchase price, provided under the Wyndham Act, could be spent by the owner of a settled estate as he wished, while his trustees took over the residue remaining after all claims on the estate had been settled.

Reviews of Books

GENERAL

R. F. ATKINSON. *Knowledge and Explanation in History: An Introduction to the Philosophy of History*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. x, 229. Cloth \$14.95, paper \$6.95.

R. F. Atkinson's book introduces the reader to philosophy of history as it is currently understood. It does this job well, concisely, and in a reasonably sophisticated way.

Chapter 1 is rather traditional, setting up the distinction of analytic from substantive philosophy of history and identifying the two relevant senses of the word "history." The chapter's third section traces the subject from roughly the time of Oakeshott, Croce, and Collingwood (the 1930s) to von Wright (1971); this history is very capsulized. The section also offers a useful discussion of historians on the topic of philosophy of history, again very compressed.

Chapter 2 ("Knowledge of the Past") tells us that historical statements are not based on direct observation or, for the most part, on memory. Even testimony (from the past) requires special treatment to count as historical knowledge. What one would like then, but does not adequately get from the book, is a positive theory of evidential knowledge. This chapter also discusses, without resolving them, some of the interesting skeptical questions raised respecting historical knowledge. (I mean such questions as whether statements about the past are meaningful, and if so, whether we can know about the past evidentially, and whether there really was/is a past for such statements to fix on.) But that its pages should fail to "dry up the swamps of skepticism" (Danto's phrase) is, perhaps, not an important lapse in an introductory text.

Chapter 3 (on objectivity) is quite a good one and discusses a topic of real interest to the working historian. Chapter 4 (on explanation) sets out from the convention, established by Walsh and repeated by Dray, of dividing the field of explanation theory between the positivists (or "covering law" people) and

the "idealists" (whom Atkinson more properly calls the proponents of "rational" explanation). The important feature of this chapter, though, is the fact that the author introduces a third major theory of historical explanation: that such explanation is *sui generis* insofar as it consists in coherent narrative. Or, to put Atkinson's thesis differently, narrative history (under which he includes both narrative, in the conventional sense, and analytical treatments) is *per se* explanatory (see pp. 136-38 for a summary of his basic approach).

Atkinson's argument in chapter 4 rests, I believe, on a confusion. He shows, correctly, that the *meaning* of "explanation" is such that ordinary historical narratives must be regarded as explanatory. Moreover, he argues that neither of the standard theories—neither law explanation nor rational explanation—can capture all that is included under explanation through historical narrative. It does not follow from this, however, that ordinary historical narrative constitutes an account or theory of explanation. Atkinson has confused two distinct issues: which kinds of things are explanatory (as being in accordance with the *meaning* of "explanation") and which things are theories of explanation. Hence, historical narrative does not, simply in being explanatory, offer a theory of explanation at all, let alone an alternative to the standard ones.

Chapter 5 (on causation) follows lines laid down by White, Gardiner, and Collingwood. Atkinson stresses (p. 162) the openness and variability of historians' causal judgments. The chapter also includes interesting discussions of methodological individualism and of Marxism. The last chapter (on values) is well done. I particularly liked section 3 on moral judgments in history. I also thought section 4 on progress followed well. Since the two sections here are of central concern to historians and their students, it is fortunate they are good ones.

In sum, I think the book is well written and well organized. It compares favorably with the two principal introductory texts (by Walsh and by Dray) currently available. One of the strengths of Atkin-

son's book lies in its chapters, not found in the other two, on statements about the past and on values. On points where the books are comparable Atkinson's book holds up well, mainly because it is more complete (in the survey sense) and much more up to date. Atkinson's book does not include material on speculative philosophy of history. But inexpensive books of readings from the standard classics or good studies in paperback of such philosophies could be used to supplement Atkinson's book in a course.

Accordingly, I would stress the considerable intrinsic interest of the Atkinson book and its usefulness as a textbook. It should have an important place in philosophy of history.

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ANTHONY D. S. SMITH. *Nationalism in the Twentieth Century*. New York: New York University Press. 1979. Pp. x, 257. Cloth \$17.50, paper \$8.95.

Anthony D. S. Smith is emerging as one of the most important investigators of nationalism since Karl Deutsch. In *Theories of Nationalism*, Smith elaborated a sophisticated typology but fell short: in his effort to establish nationalism as an ideology in which value is transferred from religious tradition to the community. In this, his second major book, Smith uses some of the best ideas of *Theories of Nationalism*, including the idea that nationalism is an ideology of rejuvenation, to analyze nationalism in the twentieth century.

The first few chapters are somewhat halting. Smith makes the sound but unexceptional points that nationalism is not really a millennial movement, that it must be clearly distinguished from fascism, which was not an extension of nationalism but a special European response to interwar problems, and that it should not be confused with racism. In these chapters he exhibits the same clarity of definition that characterized *Theories of Nationalism* but leaves the impression that the total result will be flawed, as was his first work.

In chapter 5, however, "Communist Nationalisms," Smith hits his stride. He suggests that Marxism and nationalism have conceptual meeting points that make it possible for them to coexist despite their overt antagonisms. The mating of the two ideologies occurs in the peripheral regions of the world capitalist economy, where delayed modernization and disappointment in the results of democracy force them together. The intelligentsia in these areas discover that Leninist party organization can provide political tools for modernization and that populist nationalism can mobilize the forces of ethnic or racial community to legitimate

the process. Their purpose is not social revolution, as their Marxist coloration might suggest, but the construction of political order, as Huntington called it, so that their nation can take an honored place in the international system of states.

Smith believes that nationalism will continue to prosper for at least two reasons. On the international level, the existence of a functioning worldwide system of states exerts enormous pressure for conformity to the ideal of the nation-state. On the local level, what Smith calls the bureaucratic cycle will continue to throw up ethnic nationalisms even within what seem to be well-integrated states. This cycle begins with the struggle for independence and the original organization of the nation. The next step is centralization of the state by the victors, which in turn generates revolt against the new center by an ignored periphery. At both the international and national level, the inhabitants of politically weak regions find it possible to make ethnic differentiations, sometimes very finely drawn, between themselves and the strong. The latent cultural affinities thus uncovered authenticate the political movements that seek to overcome the weakness.

Smith is a bit old-fashioned. He does not appear to be a Marxist, a structuralist, a model-builder, or a data-cruncher. His interpretation remains political. Nonetheless (should I say "therefore"?), Smith provides some satisfying explanations of why nationalism is still with us and why it probably will remain with us for a long time.

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NIKOLAI V. SIVACHEV and NIKOLAI N. YAKOVLEV. *Russia and the United States*. Translated by OLGA ADLER TITELBAUM. (The United States in the World: Foreign Perspectives.) Chicago: University of Chicago Press. 1979. Pp. xv, 301. \$12.95.

Rightly advertised as "a different view" of Russian-American relations, this book by two Soviet historians conveys what the Soviet government would like Americans to think the Russians think of those relations. For this reason, it differs from the other books in the distinguished University of Chicago series, which attempts to present genuine foreign perspectives on America's relations with particular countries. Moreover, the new Soviet venture in diplomatic history also differs from most previous ones by serving unabashedly the cause of detente as Moscow understands it.

Laudable as that cause may be, it does not necessarily provide the correct inspiration for good history. To be sure, compared with the thesis-infested and cantankerous Soviet historiography of the past,

Nikolai V. Sivachev and Nikolai N. Yakovlev's opus is almost a model of restrained factual analysis. But still, the thesis comes first, and the facts are harnessed to fit into it.

According to the thesis, Soviet-American collaboration is possible and inevitable for "objective" reasons, something that the Soviet leadership, privy to the wisdom of Marxism-Leninism, has always grasped perfectly but that less enlightened capitalist statesmen have frequently ignored to mutual disadvantage. Though sometimes compelled by the force of circumstances to promote better relations despite their worse selves, American leaders have all too often missed real opportunities for collaboration. Given these premises, the story of Soviet-American relations that unfolds in the book is fairly predictable, though not necessarily convincing for those not already convinced.

Thus, for example, the reader's credulity is stretched paper-thin by the authors' claim that the Soviet government bore no responsibility for the activities of the Comintern or by the assertion that Stalin's famous "election" speech of 1946 (cited in a suitably doctored version) was really quite conciliatory. Juxtaposed are examples of alleged American perfidy recorded, it is true, with more sadness than indignation. We are distressed to learn that the U.S. ultimatum to Japan in November 1941 was actually calculated to unleash a Japanese attack on the Soviet Union. In an effort to beat the dead horse of the second front, Elliott Roosevelt (of all people) is summoned as the crown witness to testify about Washington's putative scheme to delay the crucial operation in order to benefit from everyone else's exhaustion. When the Normandy invasion at last materialized in 1944, the hidden motive was presumably to counteract the triumphant spread of Soviet influence in Europe.

Besides the standard myths, clad in more respectable garb, there are some real surprises in the book, none greater than the authors' disposition to go out of their way to whitewash the tsarist autocracy for the sake of detente. Harsh words are reserved for the elder George Kennan and other, supposedly Jewish-manipulated, American critics of tsarist repression before World War I. This stunning condemnation of the nineteenth-century "slogan of Russian despotism as a hindrance to good Russian-American relations" (p. 15) speaks eloquently about the sensitivities and self-image of the Soviet Union's present rulers, for whom "the value of detente . . . lies in both nations adhering strictly to the principle of noninterference in each other's affairs" (p. 257).

On balance, the book is as good and as bad as the "peaceful coexistence" it is intended to buttress—a coexistence conceived as a "special form of the class struggle" (p. 40). In this respect, the study does not offer anything that American leaders or the public

do not already know or surely ought to know. Nor does it substantially enhance the horizons of professional historians by its selective bringing together of material familiar to them from elsewhere. For a book so redundant, prospective readers should think twice before paying even the very reasonable price for which it sells.

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WOLFRAM FISCHER. *Die Weltwirtschaft im 20. Jahrhundert*. (Kleine Vandenhoeck-Reihe, number 1450). Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. 110. DM 9.80.

Wolfram Fischer, the distinguished economic historian of the Free University of Berlin, has sought to compress the history of the world economy in the twentieth century into approximately seventy pages of text, eleven pages of endnotes, eighteen pages of tables, and five pages of bibliography. The book is an enlargement of a lecture given in Hamburg in October 1978, and the author regrets that he saw the fat volume of W. W. Rostow, *The World Economy: History and Prospects* (1978), too late to take it into account.

Three main parts consist of the structure of the world economy, trends and cycles in world economic development, and the developing countries in the world economy. The part on structure is divided into trade, terms of trade, production, services, income distribution, and summary. The ten pages on cycles cover the popular Kondratieff, recently adopted by Rostow, Forrester, and many others, the question of whether long swings have political causes, and imperialist exploitation of the Third World. The longer passage on the developing countries raises questions of agriculture, industrialization, export-led growth, and import substitution. The focus is evidently on developing countries in parts 1 and 2, as well as explicitly in part 3.

It is difficult to know what to make of this. Fischer is an able historian and economist, and what he covers he does well, if tersely. Where, however, are the themes of the decline of Britain as the dominant world power; the takeover by the United States after a dangerous interregnum that helped to produce or at least to intensify the world depression; World Wars I and II with their economic causes, if any, and their (different) economic aftermaths; the contrast between the tangle of war debts, reparations, and currency disarray in the 1920s and lend-lease, the Marshall Plan, and Bretton Woods in the 1950s? And so on. The data in the tables are fragmentary and overemphasize primary products. Chrome, bauxite, manganese, and molybdenum are

worthy products, but why no steel, automobiles, employment in engineering industries, electricity production? Many of the tables begin only in the 1930s, because they rely on the Woytinskys, who did not try to cover the whole century.

Perhaps the difficulty lies in the overambitious title.

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PETER KARSTEN. *Patriot-Heroes in England and America: Political Symbolism and Changing Values over Three Centuries*. Madison: University of Wisconsin Press. 1978. Pp. ix, 257. \$25.00.

Patriotism in the United States was one of the casualties of the Vietnam War. Today, diminished respect for authority and spreading cynicism, together with a decline in historical consciousness, make it difficult to engender enthusiasm for the great men of American history. It would seem that hero worship has become a thing of the past.

Peter Karsten does not choose to deal with the contemporary phenomenon. Rather, he is concerned with a time in American and English history when patriot veneration was a serious matter, especially for the elites who obtained political mileage from the practice. These elites made use of the symbolism associated with the names of various patriots to influence public opinion in ways considered desirable by them. It is Karsten's thesis that specific kinds of heroes were brought forward at different historical periods depending on the needs of the time. Later, they were discarded when their particular symbolism no longer served elite purposes. Thus, in a certain sense, each generation was made to celebrate distinct kinds of patriot-heroes.

When the American colonists rebelled against British tyranny, they cited the precedent of individual English heroes, like Algernon Sydney and John Hampden, who themselves had resisted tyranny. Decades later Americans used the memory of one of their own, Thomas Jefferson, to oppose the growing centralization of government. But a century later, the names of these three had all but been forgotten as the requirements of a new era called for different kinds of patriot-heroes. In an age of increasing governmental control and overseas expansion, figures who typify power and authority now came to the fore. Strong leaders of past ages, like Oliver Cromwell in England and Abraham Lincoln in the U.S., achieved first rank. The book concludes with the observation that the choice of patriot symbols reflects shifting values in political culture.

Karsten is probably correct when he argues that elite needs at different times help to determine which heroes get revered, and the book is full of ex-

amples and citations to bolster his views. But the kinds of general categories that he emphasizes, statism and antistatism, hardly exhaust the possibilities for the utilization of hero symbolism. Elites always seek to build up national fervor and loyalty, and this would be especially true in a country whose immigrant population needed to be Americanized. For this purpose, heroes like George Washington come to be bestowed with timeless virtues.

Moreover, the book occasionally falters when the author conjectures about the motivation or goals of ruling groups who, incidentally, are never identified. Frequently, the reasons Karsten gives for certain fashions in hero worship tend to be speculative ones. It can be demonstrated that particular names appear more often in contemporary texts at specified points in history, but establishing why this occurs is an entirely different matter and very hard to prove. *Patriot-Heroes in England and America* has raised some interesting questions about how ideas reflect changing social conditions, but it has not sufficiently answered them.

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ANCIENT

N. G. L. HAMMOND and G. T. GRIFFITH. *A History of Macedonia*. Volume 2, 550–336 B.C. Oxford: Clarendon Press. 1979. Pp. xxii, 755. \$65.00.

This book is the second volume of N. G. L. Hammond's projected three-volume series on the history of Macedonia. The present work covers the period from 550 B.C. to the death of Philip II in 336 B.C. and is divided into two parts. The first part is from the pen of N. G. L. Hammond and concerns the period of Macedonian history from 550 B.C. to the ascension of Philip II to the throne in 359 B.C. The historical sources for this epoch are notoriously scattered among various ancient writers, epigraphical evidence, archeological finds, and numismatic works. It is one of the important contributions of the present volume that all this material is conveniently assembled to elucidate historical events, movements of peoples, and the influence of Greek culture on Macedon. Interpretation of such unsatisfactory evidence is often difficult, and not all will agree with some of Hammond's conclusions. He accepts the Argive origin of the Macedonian royal dynasty, for example, rejecting the idea that it was a later invention to gain respectability in the Greek world. Hammond also advances strong arguments to support his notion that the term Argead refers not to the royal family but to a Macedonian tribe or clan. The narrative is illuminated by an excellent

color map (the lack of which was a shortcoming in volume 1).

The second and more extensive part of the book was written by G. T. Griffith and concerns the reign of Philip II. As Griffith observes, the major problem confronting historians is: how did Macedonia, heretofore a weak, divided state of negligible political and military importance, become during Philip's reign the ruler of the Greek world? The answer for Griffith and a great many others is that Philip himself was largely responsible for this achievement. Consider the situation in Macedon at Philip's ascension in 359 B.C. The former king Perdiccas had just been killed and the Macedonian army annihilated in a catastrophic battle against the Illyrians who were now preparing a major invasion of the country. Other neighboring Balkan nations were pillaging Macedonian territory. At home, Philip faced no fewer than five would-be usurpers, some supported by powerful states such as Thrace and Athens, which controlled much of the Macedonian seacoast. Griffith meticulously documents how Philip, by a shrewd combination of diplomacy, military intervention, and bribery gradually transformed his nation into the most formidable power in the Western world. Few will quarrel with Griffith's contention that Philip was instrumental in this transformation.

Of course, there is much controversial material here, and not everyone will accept all of Griffith's conclusions, especially concerning Philip's assassination, admittedly a frequently debated topic. Griffith accepts Aristotle's statement that Philip was killed for the private revenge of the assassin Pausanias, because Aristotle (who had close connections with Philip and Alexander) would never write such a statement if it was not generally known to be true. But how much knowledge did anyone really have of Pausanias's motives since he was killed within minutes after the assassination? Also, although there is a detailed chronology of Philip's plans to invade Persia, we are left quite in the dark about the reasons for Philip's decision. What events in Persia contributed to the decision to intervene?

But these are trifles. We are all in the authors' debt for the skillful and comprehensive way they have elucidated such a difficult period of Macedonian history.

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HEINZ KREISSIG. *Wirtschaft und Gesellschaft im Seleukidenreich: Die Eigentums- und die Abhängigkeitsverhältnisse*. (Schriften zur Geschichte und Kultur der Antike, number 16.) Berlin: Akademie. 1978. Pp. 133.

Heinz Kreissig's little book is an essay in structuralist interpretation; it aims to define the forms of ownership of the means of production and the social and juristic status of the producing classes in the Seleucid kingdom. Mostly this means agriculture, but Kreissig treats crafts and trade as well. The conclusions may be summarized: the Seleucids found a state in which "ancient Oriental" patterns predominated, did not aim to alter the situation, and in fact left things much as they found them. Farmers were mostly free but tied to the royal land they worked; slavery was almost unknown in production. The Greek cities founded or developed in the area had only a modest impact in introducing the private ownership of land and use of slavery characteristic of the polis. Kreissig is a minimizer, who sets himself against the more sweeping interpretations of his predecessors. The corrective is valuable, and Kreissig's overall judgment is probably not far from correct.

All the same, the book is not very satisfactory. Many pages are occupied in repeated discussions of the same handful of inscriptions and literary passages, broken up by the artificial organization of the book, and these discussions have little new to add. The inscriptions in Welles's *Royal Correspondence* are the most important evidence, and little is added to Welles's thorough and intelligent commentary, partly because Kreissig shows none of the needed philological acuity (the widest divergence, page 85 on RC 3, is simply wrong). The remarks on the *technikai* of Dionysus (p. 81) are an irrelevant howler. Bibliography is sketchy and references not always up-to-date: Dura parchments, for example, are cited by the first edition (1926); the pertinent volume of the Dura final report (1959) is listed in the bibliography but ignored throughout the book (to its detriment).

The decision to treat the Seleucid empire separately was unwise. The evidence is insufficient and scattered—too much can be and is made of many items—and almost all long known; Kreissig repeatedly must adduce evidence from other monarchies (Antigonos, the Attalids, the Maccabees) to flesh out the picture. Better to have taken all Asia (i.e., the former Persian Empire) for the entire Hellenistic period. Kreissig ignores Ptolemaic Egypt—the one kingdom about which we know something substantial—altogether, except for borrowing its terminology for land tenure (which he admits is mostly not attested in the Seleucid kingdom). Only token notice is taken of archeological material, especially in the section on settlement patterns (pp. 17–31).

A rewriting of Rostovtzeff has—wisely—not been attempted, but what we have is too little and too much. An article would have sufficed to set out the gist of the argument and its slight novelty. Eco-

nomic historians and those interested in the Hellenistic East will want to read this essay for its points of interest; but the student or scholar wanting to learn about the subject will do better to read Rostovtzeff (*Social and Economic History of the Hellenistic World*) and Welles's *Royal Correspondence* for synthesis and evidence.

ROGER S. BAGNALL
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ION HORĂȚIU CRIȘAN. *Burebista and His Time*. Rev. ed. (Bibliotheca Historica Romaniae Monographs, number 20.) Bucharest: Editura Academiei Republicii Socialiste România. 1978. Pp. 252.

This abridged version of the second Rumanian edition, translated by S. Mihailescu, details the life of the great Dacian chieftain Burebista, who was an ally of Pompey the Great and enemy of Julius Caesar. Social reformer, military strategist, and conqueror, he held sway from ca. 82 to ca. 44 B.C. over the northeastern areas of the Roman "barbaricum" from the river Tisza (Theiss) to the Black Sea, an area somewhat larger than that of Greater Rumania.

In the introductory chapters the author surveys the development of an autochthonous Geto-Dacian culture in Transylvania that he considers to be the foundation for Burebista's power. He shows how Dacia, in the crossroads between East and South, could resist for centuries turbulent tribal movements and was able to rise against Roman threats under Burebista, "the first and greatest of Thracian kings" (Syll.I.G.³ 762). Relying on available literary evidence (a brief account by a contemporary of Burebista, Strabo the geographer, and four additional, indirect references), the author discusses Burebista's life and his internal and external policy in three chapters. Three concluding chapters deal with the territorial limits of the area under Burebista's control, fifty-eight fortified settlements and fortresses within this area, and the culture of their peoples. The author assumes that Burebista was assassinated by disgruntled aristocrats shortly after the assassination of Julius Caesar.

The book is historical detective work *par excellence*. The few data about Burebista's life are treated with expertise. Specialists in the field should find valuable the discussions of Burebista's alliance with Pompey (pp. 46 ff.) and his campaigns (chap. 5). Regrettably, exciting new archeological evidence set forth in the book is not focused on Burebista's age. Pottery, small finds, and architectural data fall into a broad chronological span between the fifth century B.C. and the third century A.D. They are indirect and less than satisfactory allusions to the

much disputed hypothesis of Daco-Roman continuity (R. W. Seton-Watson, *A History of Roumanians* [1934], pp. 9-13). Consequently, many of the author's conclusions remain hypothetical and, occasionally, tendentious. Neither Dacian cultural originality beyond the natural borders of Transylvania, nor the existence of a Geto-Dacian state above an advanced tribal order appear to be proven by the evidence proffered. An index would have been a useful addition. For non-Rumanian readers, special problems are caused by the lack of a detailed map with generally known toponymy.

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ALDEN A. MOSSHAMMER. *The Chronicle of Eusebius and Greek Chronographic Tradition*. Lewisburg, Pa.: Bucknell University Press. 1979. Pp. 366. \$24.50.

Alden A. Mosshammer argues that Eusebius' *Chronicle* is important as "the earliest work extant in anything like its original form that deals with early Greek chronology in a continuous and comprehensive manner" (p. 16). Largely complete before 303, the work was half "raw materials" (much is lost), half synchronized lists. Neither part survives except in translation or re-edition, but enough remains so that by peeling off Jerome's additions one can get close to the original. In spite of Eusebius's Christianity he gives twice as much information for pre-Christian times as for post.

In this study we learn of Jerome's arrangement in columns, Greek chronography in general, Eusebius in particular. Mosshammer denies that the earlier Christian Africanus gave Eusebius an Olympic victor list. Indeed, there is little in the *Chronicle* from Africanus anyway.

After analyzing Eusebius' sources, Mosshammer turns to the dates assigned to various personages in early Greek history from Lyncurgus to Euripides. In each discussion he examines Eusebius' date(s) and possible sources, their contaminations, and his or their errors. The upshot is likely to shake faith in Eusebius's skill and judgment, in spite of the author's soothing remarks. Take Hesiod. One date, 1017, is due to Africanus, who synchronized with Solomon; Eusebius further synchronized with the thousandth year after Abraham. The date 913 from *Latina historia* obviously comes from Jerome. This leaves 809, from Porphyry (who set Homer in 909, Hesiod a century later), and 767. Mosshammer shows that the last date was based on Sosibius's 867 for Homer, to which Eusebius himself added Porphyry's century. Another case: Thales. Here both Eusebius and Mosshammer seem on firmer ground,

though the extremely peculiar 747, found in the *Chronicle* with dates a century and a half later, seems neglected. (For 747, see Diels-Kranz *Vorsokratiker* 11 A 2 and 8.)

One can agree in principle with Mosshammer's conclusion ("every detail is suspect, but the traditional chronology is structurally sound" [p. 319]) while wondering how much help this gives historians working in areas supposedly served by the *Chronicle*. If every item has to be tested (as it does), what good is the whole work except as a sample of fourth-century encyclopedic erudition?

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MEDIEVAL

EDITH ENNEN. *The Medieval Town*. Translated by NATALIE FRYDE. (Europe in the Middle Ages, Selected Studies, number 15.) New York: North-Holland. 1979. Pp. 287. \$41.50.

This excellent survey of a vast and complex topic is doubly valuable. A comprehensive introduction to medieval urban history that will be of great value in undergraduate courses, it also introduces and comments on much of the field's extensive secondary literature. The thousand titles (plus a supplement) in Edith Ennen's bibliography represent a masterful work of selection, a list fifty-seven pages long made lively by citations in the text. On a very wide range of topics her text and its notes provide the best possible starting point for further reading.

The introduction begins (and the following two chapters continue) the task of defining what a town is and surveys urban civilization in the Roman Empire, emphasizing the West. Chapter 1 discusses the early medieval survival of Roman towns, their topography, institutions, trade, industry, and culture. Here, as always, analysis and narrative rest on a wealth of detailed information presented with great ease. Chapter 2 is devoted to new beginnings, the routes and settlements of merchants in Northern Europe in the early Middle Ages. Merchants who settled around Roman cities are discussed, but the emphasis is on the vast areas of Northern and Central Europe that Rome never ruled. Chapter 3 surveys the emerging medieval town in the tenth to twelfth centuries, ranging from Italy and Spain to Germany and discussing terminology, trade, architecture, fortifications, and more. Organization and governance are discussed in chapter 4, from the constitution of Genoa and other Italian cities to the foundation of new towns in Central Europe and to problems of social composition. Chapter 5, "The Organization of Economic Life," is less wide-ranging than the title implies; it briefly surveys fairs, the

origins of the Hanse, guilds, coinage, and Italian commercial sophistication. The longest and richest chapter is the sixth, "The European Urban Landscape," which begins with Italy and its trade, colonization, political struggles, and the *contado*. After discussing southern French towns, the author turns northward, ranging from the institutions of Cologne and Ghent to the topography of Novgorod. The Hanseatic League and its members are one of several groups of cities discussed and differentiated. Population, trade, and social unrest are the major themes of the last chapter; mention is made of the Jews, the clergy, and education. The attentive student will finish this book with an excellent basic knowledge of medieval European cities and an orientation to further reading, for, like most surveys, this one does not survey everything equally; there is relatively little, for example, on France. Institutional, political, and economic aspects predominate, reflecting the scholarly tradition shown so well in the bibliography.

Some passages struck this reviewer as confusing, unclear, or inaccurate; in every case the fault was the translator's. The Frankish legal system became Franconian (p. 100), *Muntleute* were "literally, mouthpeople" (p. 109). Elsewhere, the German text's meaning was reversed (pp. 74, 111, 113, 199), and confused renderings went uncorrected (pp. 71, 103, 152). In general, the English text lacks the clarity and liveliness of the original. Even so, the work itself remains excellent, a most important—and unrivaled—introduction to its subject. Its wealth of material on German cities is an enormous asset since so little is available in English on medieval Germany. The book is a welcome replacement of Rörig and Pirenne; despite its price, it should be in every library.

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JOACHIM W. STIEBER. *Pope Eugenius IV, the Council of Basel, and the Secular and Ecclesiastical Authorities in the Empire: The Conflict over Supreme Authority and Power in the Church*. (Studies in the History of Christian Thought, number 13.) Leiden: E. J. Brill. 1978. Pp. xvi, 514. f 144.

In recent decades, the Council of Basel has been studied extensively as a topic in ecclesiastical history, more specifically as a problem in the constitutional and intellectual history of the Church. Recent literature on fifteenth-century German constitutional history is for the most part available only to German readers, and even recent German publication on the period is not copious. This study is a superb example of a fruitful venture that ties to-

gether these two subfields in history, subfields that have all too often suffered from compartmentalization. Joachim W. Stieber focuses on the papacy and on the Council of Basel not as autonomous structures but as political powers negotiating with the German emperor and princes in an attempt to win their support. By the same token, he reviews German constitutional history in order to understand the context in which the German leaders made their decisions regarding ecclesiastical questions. The task is a bold one and the results are commensurate with the author's ambition. The notes and bibliographical appendixes alone constitute fundamental research tools for students of the fifteenth century.

The theme of the study is Realpolitik. The analysis of the motivations of the principal protagonist in the conflict is a prime concern of the author, and, in his view, lofty motives are hard to find. Eugene IV appears as a precursor of the Renaissance papacy, primarily concerned with protecting his political, financial, and military position. Likewise, the Council of Basel as it broke with Eugene IV is pictured as a self-seeking body concerned not about Church reform but about the defense of its power and prestige. A council called to reform the Church became ensnared in the same pursuit of wealth and power that it had hoped to extirpate from the Church.

Finally, the German princes, including the prince-bishops, emerge as "dynastic landlords." By this term Stieber means to reject any suggestion that princely actions resulted from systematic or planned programs characteristic of more modern rulers. Eager to protect and expand their landed base of wealth, the German princes were guided by these pragmatic concerns when they came together to discuss their reaction to the Council of Basel. Annates and clerical livings rather than spiritual loyalties directed their decisions. Frederick III is seen as little more than first among equals in decisions regarding ecclesiastical affairs and, indeed, as less than equal in regard to such qualities as decisiveness and boldness.

There are varying degrees of persuasiveness in the author's pronouncements about motivations. For the most part motives are discussed in social and political, not personal, terms. Only occasionally are more personal motives discussed, for example, those arising from the timid and cautious personality of Frederick III. At times declarations about motivations are substantiated by solid evidence regarding the financial or political advantages to be gained from certain decisions (for example, pp. 172, 195-96, 220-21). In other instances, however, the documentation about motivations is less convincing. The author retreats from the declarative to the subjunctive, the link between evidence and assertion is

less clear, and in some cases comments about motivation are speculations out of a general context of policy unsupported by specific footnotes (for example, pp. 164, 167-68, 219, 311, 320-21).

This study is historical art on a grand scale. The canvas is large, the color is striking, and the figure is sketched with clarity. For the present at least, this study offers a definitive interpretation of the relation between the Church hierarchy and Germany in the mid-fifteenth century.

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MICHAEL GOODICH. *The Unmentionable Vice: Homosexuality in the Later Medieval Period*. Santa Barbara, Calif.: ABC-CLIO. 1979. Pp. xvii, 164. \$14.95.

When does a subject of obvious current interest excuse the neglect of the most elementary canons of historical scholarship? Michael Goodich's volume insistently raises this question. The canons he has neglected range from the simple to the complex: (1) he cites documents he has not read, or, if he has read them, then he has not understood them; (2) he omits the contexts that might aid in interpreting his evidence; (3) he has no idea what conclusions to draw from his evidence; (4) his generalizations are consistently anachronistic. For want of space, I will present only one example of each type. Others may be found on almost every page.

(1) "One poem by the great courtly poet Guillaume IX of Aquitaine speaks of his love for the girl Agnes and for a certain Arsene (sic), perhaps a boy" (p. 6). The reference is to *Companho, faray un vers couinen*, where N'Arsen is clearly a woman. Arsendis, a common feminine name in the region, has no masculine equivalent.

(2) In presenting material from inquisitorial proceedings in chapter 1 and from various legal collections in the remaining chapters, the author pays no attention to the literary history of such accusations or legal enactments. He is therefore unable even to ask why attention was paid to homosexuality in some contexts and not in others and why the attention took the form it did.

(3) For just this reason, he does not ask (nor would he be able to give a reasoned answer had he asked) whether the accusations of "sexual non-conformity" (his phrase) imply that those accused were homosexuals or whether the inclusion of homosexuality among other sexual wrongs (such as clerical concubinage) implies anything about its prevalence.

(4) Chapter 3 opens (p. 41) with the interesting argument that the Church attacked sodomy in order to maintain a birth rate sufficiently high to sup-

port the colonization movements of the twelfth century.

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E. L. G. STONES and GRANT G. SIMPSON, editors. *Edward I and the Throne of Scotland, 1290-1296: An Edition of the Record Sources for the Great Cause*. Volume 1, *Introduction*; volume 2, *Texts*. (Glasgow University Publications.) New York: Oxford University Press, for University of Glasgow. 1978. Pp. xxvi, 285; x, 439. £50.00 the set.

Some six months after the death of the "Maid of Norway," who was the heiress apparent to the Scottish throne, Edward I summoned an assembly of Scots and English, the latter mainly of his council, to meet with him to decide who was the rightful king of Scotland. These meetings began in May 1291 and took place along the Tweed, at first in the environs of Norham and then at Berwick. They lasted with several adjournments until November 1292, at which time Edward decided that John Balliol had the best right to the vacant throne. The sessions during these eighteen months have been considered as one legal and constitutional process, and historians have given it the name of the Great Cause.

The present volumes are the result of twenty years of planning and work to bring together the records that were created by that process. These records and their composition are exceedingly complex in themselves. The most important were compiled by, or under the direction of, John de Caen, a royal notary, during the progress of the case. From various notarial materials he constructed a great roll in two exemplars of the proceedings. Many years later, between 1315 and 1318, another notary, Andrew de Tange, working under royal orders, compiled another great roll in three exemplars, based upon Caen and additional records. Both have been published, Tange's roll by Prynne (1670) and Caen's roll by Rymer (1705). Neither Prynne nor Rymer offered criticism of the rolls, and the two sources have never been compared.

These two notarial records, together with the *Annales Regni Scotiae*, form the three major narrative sources edited by E. L. G. Stones and Grant G. Simpson, and to these three they have added non-notarial documents, French and Latin texts and memoranda, and the opinions of French lawyers on the Great Cause. They have prefaced their edition of these materials with a comprehensive study of the major questions affecting the records. Considering the immense complexities of the subject, no one should be surprised that certain chronological questions and problems of textual derivation re-

main unresolved. The editors deserve only praise for the care and labor they have lavished upon this project.

Although Stones and Simpson did not intend to study the judicial process itself, they make clear their opinion that the assembly called by Edward should be considered a parliament despite the demurrer of Richardson and Sayles, that the proceedings were not an arbitration but an adjudication, and that Edward could have approached the question only through the essentially feudal posture of asserting overlordship in such a court of judgment. They believe that the tendency by scholars to regard the objectivity of notarial documents as unimpeachable should be abandoned, for a notary who creates records at the order of a king will execute many of the king's wishes in his work. On the question of why there are no Scottish records of the Great Cause, the truth would seem to be that, since a king, and especially the English king, did not permit record making in any of his law courts by anyone but his own clerks, he would presumably feel the same way about such a special court as that of 1291-92.

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MARTA CRISTIANI. *Dall'unanimitas all'universitas da Alcuino a Giovanni Eriugena: Lineamenti ideologici e terminologia politica della cultura del secolo IX*. (Istituto Storico Italiano per il Medio Evo, Studi Storici, numbers 100-02.) Rome: The Institute. 1978. Pp. 209. L. 12,000.

As usurpers who interrupted a hereditary line of kings, Carolingian rulers not surprisingly sought legitimacy and justification for their own rule in theories of kingship and society. These theories, propounded by Carolingian thinkers and expressed by their sovereigns in official acts, were hardly unassailable bulwarks of political control. For one, theories of state that appealed to the divine order of things as revealed in Scripture contradicted the observable fact that effective power rested on control of land and the loyalty of vassals. Furthermore, the concept of the king as a vicar of God entrusted with ministerial power enhanced the monarch while leaving him open to ecclesiastical supervision and correction. Finally, as the ninth century progressed, the concepts of unanimity and concord with which writers of Charlemagne's generation had expressed the sentiments of the new order gave way to internecine struggle and institutionalized disunity.

Marta Cristiani charts with sensitive readings of the sources the efforts of such thinkers as Alcuin, Jonz of Orleans, Agobard of Lyons, Hincmar of Reims, and John Scottus to bring ideological stabil-

ity to a world in continual flux. Her book is essentially an essay in the history of ideas that seeks to uncover the multiple meanings of law and right order in society according to successive generations of ninth-century thinkers. It is to her credit that Cristiani never loses sight of immediate political exigencies that conditioned theoretical constructs. She is also to be applauded for her continual awareness of fundamental ambiguities in Carolingian political theory.

The first Carolingian attempt to fashion an abstract notion of the state was expressed by the concept of *unanimitas* as the convergence of individual wills and their embodiment in the will of the sovereign. The proper ordering of Christian society, however, demanded that the will of the community and its leader coincide with that of God. Thus a tension was created between secular authority and ecclesiastical authority, specifically the bishops who offered themselves as the interpreters of the divine will. While kings grandly appropriated the concept of divine grace to refer to their own bestowals on their followers, Hincmar of Reims pointedly reminded one of them that the priest's power of consecration set him apart from secular authority. In addition to tracing efforts to define more clearly the role of the monarch and the bishop in leading society, Cristiani provides an important analysis of the scriptural texts that served as the bases for Carolingian political thought. Theory and practice are joined in a chapter devoted to Charles the Bald's efforts to integrate concepts of justice and right order with vassalage. Her discussion of the role of the mid-ninth-century predestination controversy is particularly original. Jean Devisse, the most recent student of the controversy, has labeled it a "quarrel over words." For Cristiani, the predestinarianism of Godescalc of Orbais amounted to much more than a suspect theological statement. In limiting Christian society and the Church to the elect, Godescalc called into question the theoretical foundations of both Church and empire. Hincmar and John Scottus, both inspired by the newly translated Pseudo-Dionysius, responded to Godescalc's assault on free will with the reply that salvific grace was a gift that man was free either to accept or to refuse. Christian society, furthermore, was a universal one whose laws embraced both the saved and the sinners, whom, it was hoped, would be called eventually to repentance through the mediation of the Church and its sacraments. In place of Godescalc's fractured view of society, Hincmar and especially John Scottus offered a unified, hierarchical structure. Cristiani argues that John Scottus further elaborated his theory of an organic society held together by immutable laws in his most important theoretical work, the *Periphyseon*—a work whose vision of society, because of the actual political cli-

mate, would remain incomprehensible to most contemporaries.

In the earlier portion of her book, Cristiani generously acknowledges the work of Beumann, Ganshof, Tabacco, Kantorowicz, and other scholars. (Curiously, the name of Karl Morrison is missing both from the footnotes and the bibliography.) In asking her readers to regard John Scottus, upon whose thought her own previous work has focused, as a politically inspired metaphysician, she makes her most interesting and stimulating contribution to the history of Carolingian political theory.

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JEAN LECLERCQ, *Monks and Love in Twelfth-Century France: Psycho-Historical Essays*. Oxford: Clarendon Press. 1979. Pp. x, 146. \$19.95.

Although much attention has already been paid to the courtly love poetry of the twelfth century and a number of contending theories, some literary, others sociological, have been presented in order to explain this sudden efflorescence of erotic interest, very little interest has been shown in its contemporary ecclesiastical counterpart. Thus, Dom Jean Leclercq's collection of essays—which includes discussions of the exegetical treatment of the *Song of Songs*, the influence of Ovid in the twelfth century, the literary and sociological milieu of Champagne, and the underlying psychological trends in St. Bernard's career—is a welcome introduction to the psychohistory of the post-Gregorian age that makes use of previously unexplored monastic material. Some tentative psychohistorical conclusions are drawn, but with none of the partisan rancor (often based on insufficient evidence) that has occasionally marred other attempts to distinguish conscious and unconscious double meanings in medieval literature.

One could, however, argue with several of his basic assumptions. He speaks of three dimensions of human need, the physiological, psychosociological, and spiritual; some would argue whether the "spiritual" has an independent existence. Despite the patent sexual imagery found in the works of Jean of Fécamp, Anselm, and others, Leclercq suggests that divine, chaste love, unhampered by stirrings of the flesh, lay behind these musings (the *Song of Songs*, for example, was often regarded as a symbolic description of the relationship of the believer to the Church or of a cenobite to his monastery) and that St. Bernard and his contemporaries were not obsessed with sexuality. If, as Leclercq argues, the twelfth century was a "less erotically preoccupied society" than our own, what explains the great wave of canonical legislation dealing with marital

and sexual matters, the erotic nonconformity of the new heretical sects, the vogue in monastic flagellation, the rise in female vocations, and the spectacular royal divorces that characterize the period? And how should contemporary treatises on friendship be treated? As continuations of the Ciceronian literary tradition or as documents of sexual interest? And what emotional conflicts underlay the cult of the Virgin? As Leclercq's work has shown, seemingly drab monastic literary meanderings may often mask a society undergoing profound psychological change.

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ANTONIO GONZALEZ GOMEZ. *Moguer en la baja edad media (1248-1538)*. (Publicaciones.) Huelva: Instituto de Estudios Onubenses "Padre Marchena." Excma. Diputacion Provincial de Huelva. 1977. Pp. 301.

In recent years the departments of medieval history of the Universities of Seville and La Laguna (Tenerife) have encouraged the study of local and community history in late medieval Andalusia. Antonio Gonzalez Gomez's work on the town of Moguer, offered as a doctoral dissertation in 1974, is an excellent example of these efforts. It is particularly welcome since few detailed analyses of the institutions and populations of towns under seigneurial jurisdiction have been published. Moguer is a small community on the Río Tinto, a few miles from the city of Huelva. Taken from the Muslims in 1248, it was granted as a seigneurial holding in 1333 and remained in the hands of the Portocarrero and Tenorio families for the remainder of the medieval period.

The description of Moguer consists of six chapters of unequal length, commencing with a study of the genealogy and careers of the Tenorio and Portocarrero families. Because of the extent of the subject and the limited evidence available, the treatment is necessarily uneven, but it provides the justification for the claim that the work covers the period from 1248 to 1538. The description of Moguer is, in fact, restricted to the late fifteenth and early sixteenth centuries. A short chapter on geography serves to set the scene but fails to provide any of the analyses that a well-developed historical, cultural, or economic geographical approach might have made possible. Separate chapters treat the economic exploitation of the district—primarily agricultural in accordance with seigneurial domination but including some husbandry, fishing, and salt-working—and its land and water commerce. The most extensive chapter is that devoted to the discussion of the economic and social groups comprising the local population. The author does not dispose of suf-

ficient data to allow any solid statistical or demographic analysis but is able to reiterate the dominant role played by the primary sector, and agriculture in particular, in the local economy. The work concludes with a short description of the offices and functions of the municipal government, a short summary of conclusions, and an appendix of fourteen documents, dating from 1333 to 1519, important to the history of the district.

In and of itself, the book has many limitations. It provides a description of Moguer at the turn of the fourteenth century and conveys little of the development of the community over any extended period of time. By concentrating upon the district so exclusively, Gonzalez fails to develop fully the role of the regular English and Breton sea-trade using the port of Moguer, the effect of the far-flung commercial and corsair activity pursued by the Moguerños themselves, or the influence of the cosmopolitan transient population of the port. There is far more data presented than evaluations or conclusions. Gonzalez's book should not be considered simply as an independent work, however, but within the context of the studies being carried forward by his school. A number of similar investigations of local communities will be necessary before the complexities of the era and region are unraveled and effective syntheses and interpretations can be formed. This study of Moguer is an effective step toward that goal.

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KARL JORDAN. *Heinrich der Löwe: Eine Biographie*. (Beck'sche Sonderausgaben.) Munich: Verlag C. H. Beck. 1979. Pp. xi, 316. DM 38.

While *kleindeutsch* historians perceived Henry the Lion as the first representative of a national German feeling and the proponent of eastward expansion, their *grossdeutsch* colleagues saw the Welf duke of Bavaria and Saxony as a rebel who betrayed Frederick Barbarossa on the eve of Legnano. Fundamental to both assessments was the belief that Henry and his cousin were antithetical figures. Such interpretations reached their climax in 1935 when the Nazis turned the church of St. Blaise in Brunswick, built by Henry and his burial place, into a national shrine. Thereafter, though, as Nazi foreign policy became more openly aggressive, it was the emperor who was hailed as the embodiment of Germany's imperial destiny. Karl Jordan, the author of several scholarly monographs about Henry, has written a popular and readable biography of the duke that is free of such nationalistic and anachronistic interpretations.

Jordan's Henry is a twelfth-century prince. The

old constitutional order of loosely organized tribal duchies, based on personal ties, had been destroyed during the investiture conflict and was being replaced by smaller territorial states. Since the Welfs possessed few allods in Bavaria, Henry focused his attention on Saxony, where, as the heir of several dynasties, he possessed extensive holdings as well as counties and advocacies. He tried to weld these together into a principality by a ruthless assertion of his rights and by further acquisitions. He was especially interested in Nordalbingia and Mecklenburg, which he conquered and opened to German settlement. Henry's fatal mistake was to forget that his success depended on his alliance with Frederick. When Henry refused to provide the emperor with military help at Chiavenna in 1176, Barbarossa became responsive to the complaints of the Saxon princes, whose own territorial ambitions had been thwarted by Henry. Henry's condemnation in 1180 destroyed the last vestiges of the Bavarian and Saxon tribal duchies and completed the transformation of Germany into a confederation of principalities organized along feudal lines.

The major shortcoming of this sensible book is suggested by its subtitle. The proprietary and legal orientation and the annalistic style of medieval sources are not conducive to the writing of biographies. Essentially, this is a traditional political history, in which Henry's actions have been placed in the broader context of imperial politics. There is, for example, no way of knowing Frederick's and Henry's personal feelings about each other before and after Chiavenna. Beyond that, Jordan pays little attention to the fundamental social and economic changes Germany was undergoing. The development of the territorial states depended on the extinction of most of the old German nobility, but Jordan provides no explanation of why this happened. Nevertheless, this book provides an excellent introduction to the most important German prince of the era.

JOHN B. FREED

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ILLUMINATO PERI. *Uomini, città e campagne in Sicilia dall'XI al XIII secolo.* (Collezione Strica.) Rome: Editori Laterza. 1978. Pp. 358. L. 12,000.

Since at least the *settecento* historians have observed the contrasts between northern and southern Italy and have debated to what extent the economic problems and poverty of the south can be explained in either geographical, deterministic terms or in terms of human decisions and cultural history. This is the essential historiographical concern underlying Illuminato Peri's book. His survey of medieval Sicily begins with and concentrates heavily upon Is-

lamic-Norman Sicily, a land with a reputation for fertility and wealth. His main theme, as he describes the port cities, the *casali* or agricultural settlements, the rivers, and the architectural styles, is that Sicilian prosperity in the Norman period rested upon a delicate equilibrium, one not only economic but cultural and political as well: between port cities and agricultural hinterland, Christians and Muslims, followers of Greek rites and Latin rites, and between the ruling dynasty and the conquered population. Moreover, Peri maintains that Norman Sicily's prosperity was not self-generating but that the *congiuntura* between economy and society depended upon immigration from continental Italy.

According to Peri the turning point for medieval Sicily and the beginning of its decline came under the Swabians. Behind the traditional Burckhardtian image of Sicily as the most politically progressive of Italian states in the thirteenth century, Peri finds conservative economic and fiscal policies, which, he claims, destroyed the fragile balance of Sicilian prosperity. Peri acknowledges the achievements—the legal compilations, the scientific treatises, the poetry—of the “functionaries” class under the Swabians, but clearly for him these contributions cannot compensate for Sicily's continued dependence upon an agricultural-pastoral economy and its failure to develop the cities, the merchants, and the artisans characteristic of central and northern Italy. For Peri the decline of medieval Sicily was not simply determined by geography but resulted from geographical limitations and the destructive policies of Henry VI and Frederick II. For example, the Swabians, in not recognizing the vital importance of immigration for Sicilian prosperity, pursued policies that reduced even further the dwindling flow of immigrants from the mainland. The regressive policies of the Swabians were continued and intensified by the Angevins; and, in addition, Sicily was swept by disasters such as the contracted navigability of rivers in the southeast, famines, epidemics, and wars. The Angevin government responded with policies calculated not to alleviate the conditions of alternating periods of abundance and famine but to profit fiscally from them.

Peri's interpretation is a deeply pessimistic one, and unfortunately this viewpoint leads him to see decline even when the evidence is tenuous or negative at best. For example, Peri finds “stagnation” in early thirteenth-century Sicilian agriculture, even though he acknowledges the relatively high yields of Sicilian grain in comparison with other regions. His evidence for agricultural “stasis” is his perception of a lack of technological innovation in thirteenth-century Sicilian agriculture; but, since he does not compare the level of Sicilian technology with that of other regions, his evidence has limited significance. The book has other weaknesses. There are

too may brief, discontinuous chapters, and too frequently one is buried under a tediously detailed treatment of an isolated document. In general, however, the book contributes a basic and needed revision of medieval Sicilian history from the viewpoint of the new *storia strutturale*.

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PETRE DIACONU. *Les Coumans au Bas-Danube aux XI^e et XII^e siècles*. Translated from Rumanian by RADU CRETEANU. (Bibliotheca Historica Romaniae, Études, number 56.) Bucharest: Editura Academiei Republicii Socialiste România. 1978. Pp. 158. 8.25 L.

The Cumans, an important Turkic confederation of the trans-Danubian, Ukrainian, and western Central Asian steppelands in the two centuries preceding the Mongol invasions, remain one of the least studied groups of Eurasian nomads. Their role in the history of the Rus' principalities, Hungary, Bulgaria, Rumania, and Byzantium was profound and lasting. Regrettably, this slim volume adds very little to the literature on Cuman history. Petre Diaconu has very narrowly defined his topic, often forgetting that these nomads interacted with a number of peoples and territories simultaneously. Consequently, the larger perspective frequently fades from view.

Eschewing political history (for which he directs the reader to the old, but still useful studies of Rasovskii and Zlatarski), Diaconu promises a more detailed treatment of questions of historical geography, numismatics, and sigillography. Had this promise been fulfilled, the book might have been regarded as an important contribution. Unfortunately, it is a series of brief excursions summarizing previous research and occasionally buttressing it with addenda from recent Rumanian archeological investigations.

The Rumanian territories are extremely rich in Turkic (most probably Cuman) toponymy. This material is discussed in only one brief, and amazingly unsophisticated, chapter. Diaconu is apparently ignorant of the modes of toponymic analysis used by Fuad Köprülü to determine the settlement patterns of the Oghuz Turkic tribes in Anatolia or the more recent study of Hungarian tribal settlement patterns done by Györffy (*Archivum Eurasiae Medii Aevi*, 1 [1975]). He ignores, on the whole, the more than century-old tradition of Cuman studies in Hungary (for example, the recent work of Rásonyi, Pálóczi-Horvath, and Mándoky). The studies of Rumanian Turkologists specializing in Cuman philology (for example, Drimba) are also absent.

The occasionally proffered etymology of one or another alleged Cuman term or place name displays an almost saintly innocence of Turkic and related fields. This is very unfortunate, for a careful toponymic analysis might have revealed to the author the extent of the Cuman occupation of the eastern Rumanian territories.

Much of this study centers on the Byzantine sources for the Cuman Balkan raids of 1094, 1114, and 1122–23, some minor points of which are argued in great detail. Diaconu is probably right (pp. 67–70) in his identification of the "Scythians" of the 1122 events with the Cumans rather than with the Pechenegs, as has hitherto been generally accepted. His attempt to equate *Blökummannaland* of the *Heimskringla*, an Icelandic source for the 1122 raid, with *Vlachia* is typical, however, of his philological acumen. Diaconu makes a valiant effort to prove (without adducing much in the way of evidence, either literary or archeological) that Oltenia, Muntenia, and much of Moldavia were under either Cuman or Byzantine rule during this period. This allows him to challenge the claims of some Soviet historians that these or parts of these territories fell under the sway of the western Rus' (Halych) princes. Diaconu, unaware that we may now distinguish between a number of politicogeographical units within the Cuman union, makes no attempt to differentiate among the various Cumans involved in the activities of Ivan Rostislavich "Berladnik" in the "Danubian towns" or among the steppe allies of the Asenids of Bulgaria. This is a serious methodological flaw.

Perhaps the most important part of this work is the annex (pp. 134–38), which describes, with maps, the distribution of Byzantine coin finds on the lower Danube. In all other respects, this is a remarkably unsophisticated and superficial work.

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MODERN EUROPE

PETER BURKE, editor. *The New Cambridge Modern History*. Volume 13, *Companion Volume*. New York: Cambridge University Press. 1979. Pp. vi, 378. \$29.95.

The idea of adding a collection of thematic essays to a prestigious series that emphasizes narrative history is an imaginative one, and it has on the whole paid off. The essays deal with a number of major features of European history since 1500—some emphasize Western history, others cover Europe more generally—including warfare, bureaucracy, secularization, environment, peasantry, and demography. Inevitably, one wonders about the exclusion of certain

topics; the subject of urban social structures and mentalities is largely omitted, for example, as is any real effort to characterize European diplomacy. The volume leans toward intellectual history—indeed, a final essay on Western civilization for the past 2,500 years conventionally defines great ideas as the essence of Westernism, which I think overstates the case.

The inevitable unevenness of the essays should not detract from the utility of the volume. With very few exceptions (I found the essay on warfare simply too brief, that on social science a bit shallow), the essays are at least well-informed and thorough summaries, and at best they are really stimulating. My preferences included the essay on environment, necessarily somewhat tentative in periodization but at least serving as an early benchmark in the field; that on the scientific revolution, which, adopting a social context and relating seventeenth-century developments to an older intellectual framework, suggests a redating of what was really revolutionary about modern science; and Peter Burke's intelligent statement of the complexities of charting secularization.

The essays are not interconnected, and in certain cases some exchange would have facilitated not only coordination but accuracy. Thus, the final piece reflects no understanding of the kind of change in popular mentality that Burke and Le Roy Ladurie discuss. The timidity of the demographic essay is also striking: descriptive changes are duly noted but discussion of causation, which Americanists increasingly seek in aspects of *mentalités*, is sedulously avoided.

The volume as a whole was supposed to be informed with a consideration of whether the French and Industrial Revolutions constituted a key break in the phenomena being considered. The editor was properly concerned to use the essays to escape the focus on short-run change, inevitable in the preceding narrative volumes, and to deal with major change and the equally important theme of continuity. Some of the essays add little to fairly well-accepted periodization schemas; thus, Le Roy Ladurie accepts, perhaps a bit facilely, the idea of the late nineteenth century as a key break for the peasantry. On the whole, the essays seem more at home with early modern than with modern history, and few come to grips with the problem of deciding whether there has been major change *since* the Industrial Revolution. Few indeed try to place the twentieth century in any serious analytical context.

The final essay, more daring than the rest but also on balance less useful, does venture a bit of prediction, noting that we may be heading back toward a more medieval civilization in the pendular swing of Western culture from real Westernism (ancient, modern) to non-Western intrusions (Middle

Ages, the future). As is suggested by the inclusion of this rather playful avoidance of the very issue of modern periodization with which some of the other essays grapple, the volume comes to no overall conclusion, even about the criteria that should be used to measure major periodization. One remains with some very interesting individual essays, whose linkage, however, must be sought elsewhere.

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DOROTHY KOENIGSBERGER. *Renaissance Man and Creative Thinking: A History of Concepts of Harmony, 1400–1700*. Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. xiii, 282. \$25.00.

Dorothy Koenigsberger attempts a fresh look at the Renaissance notion of harmony, studying it less as an explicit theory than as a series of related assumptions underlying the various facets of the period. To achieve her goal the author analyzes the works of Leon Battista Alberti, Leonardo da Vinci, and Nicholas of Cusa. After establishing the links among these writers, Koenigsberger then takes up the practice and theory of music in the Renaissance and concludes by discussing the Renaissance interest in interdisciplinary studies.

The thesis of the book is hard to state simply. The author argues that the notion of analogy is central to Renaissance harmony, that Renaissance analogy differs from medieval analogy in being less hierarchical, more grounded in actual experience, and more convinced of the natural sympathy among the parts of reality. Alongside this main thesis are several subsidiary ones. The author feels that Nicholas of Cusa exercised a decisive influence by framing artists' notions of Platonism in the early fifteenth century, that magic ran counter to the harmony her subjects sought, and that the universality implied by analogical thinking led to the interdisciplinary nature of Renaissance scholarship.

Several factors make the argument difficult to follow. The author too frequently concludes a detailed discussion of a text with a commonplace that adds little to our understanding, and she devotes far too much space to explaining what she is not saying. In addition, the book seems unnecessarily disorganized and hastily written. A chapter on Nicholas of Cusa is inserted without sufficient justification between two chapters on Leonardo. To make matters worse the first chapter on Leonardo refers to the second as if it were to follow immediately. Obviously the chapter on Cusa was inserted as an afterthought. Finally, the editor should have corrected the grammar of the book, which exhibits lack of parallel structure, dangling participles, and colloquialisms that distract the reader from the book's thesis.

Apart from these issues of organization, style, and grammar, there are some important conceptual problems. First, the basic contrast between Renaissance and medieval analogy rests on an overly simple view of medieval thought. The author usually refers to Boethius as her exemplar, although at one point she makes good use of Bonaventure. Dante, much more important to Renaissance thinkers themselves, is barely mentioned, and the author seems unaware of Mazzeo's excellent treatment of medieval analogy in Dante's work. Had Dante been taken into account, the picture of Renaissance analogy presented in the book would be far more sophisticated. Second, while it is refreshing to view Alberti and Leonardo outside the context of Florentine intellectual life, the author needs to consider more explicitly the importance of that tradition, to which only a brief section is devoted. Finally, the author, in trying to probe the unspoken assumptions that underlie theories, has ignored many important models. In particular her attempt to show the implicit contributions of the Platonic Academy to science could have benefited greatly from a reading of Thomas Kuhn.

This is an ambitious work on an important subject that unfortunately does not quite deliver what it promises.

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M. S. ANDERSON. *Historians and Eighteenth-Century Europe, 1715-1789*. Oxford: Clarendon Press. 1979. Pp. vi, 251. \$26.00.

Historians do not usually write book-length historiographical essays. M. S. Anderson's history of eighteenth-century bibliography will, therefore, prove to be an extremely useful teaching aid for graduate students and specialists alike. Basic trends in French, German, and British historiography are delineated, with the French and British ones receiving the most adequate treatment. The most attention is devoted to changing views of the French Revolution and the *ancien régime*, from the contemporary reaction to it to the modern *Annales* school. French historians have shifted their focus from studying the revolution in Paris to emphasizing regional history. The classical historians are also given an excellent treatment: Michelet, Taine, Tocqueville, and Jaurès in particular. Useful also is the picture of the rise of quantification studies and the importance of the work of Ernst Labrousse in this context. Anderson supports the French claim that the "vitality" of their social and economic history has no equal in the world today.

Narrative and diplomatic history is downgraded, although some rather obscure eighteenth-century

works are mentioned. The recent German emphasis on "modernization studies" (Koselleck, for example) is not mentioned, and the excellent German schools of regional history are completely omitted. Yet the short sketch of Frederick the Great's biographers and those of Joseph II is extremely well done. Less satisfactory for the practicing Germanist is the author's argument that the concept of enlightened despotism turns out to be meaningless. Certainly German regional history in this area is as rich as the French, though less well known in the English-speaking world. What was new and different about the theory and practice of government under enlightened despotism has been worked out by several contemporary North American historians whose work has not been included: H. Liebel, H. C. Johnson, F. Szabo, and especially Leonard Krieger of Chicago. The important work of Walther Hubatsch, who has produced an entire school of young German historians working in this era, is also ignored. Overdrawn is the author's picture of Frederick II and Joseph II as rulers who were merely continuing age-old policies. No one who is familiar with Continental archives would find such a view to be at all tenable. Once confronted with the mounds of documentary material dealing with the reforms and innovations effected by these rulers, one cannot believe that enlightened despotism is reducible to "a set of theories and aspirations."

Most interesting as well is the author's survey of the changing picture of George III. His contemporaries viewed him favorably, but the nineteenth-century Whigs attacked him for seeking to undermine the constitution. Lewis Namier's methodological revolution in this century then brought in the new emphasis on sociological analysis, which was applied to biography. The circle comes full, then, and the old interest in political history reappears in its new social guise.

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LUCIANO ALLEGRA and ANGELO TORRE. *La nascita della storia sociale in Francia: Dalla Comune alle "Annales."* (Fondazione Luigi Einaudi, "Studi," number 22.) Turin: Fondazione Luigi Einaudi. 1977. Pp. 355. L. 7,000.

French scholars have recently complained that the *Annales* historians are much praised in Italy but not sufficiently imitated. Italian historians may have neglected to give the *Annales* this ultimate flattery, but they are paying critical attention to French historiography. Luciano Allegra and Angelo Torre's splendid account of the origins of French social history is the most important of current reflections on French historical discourse. Torre is responsible for

the first part, "Il superamento del positivismo," and Allegra for the second "Una metodologia per le *Annales*."

Allegra describes the essential characteristics of the *Annales* methodology as a commitment "to posing problems and formulating hypotheses" (p. 273). This is the path Torre and Allegra follow in tracing the development of French social history. Modern French history is seen as beginning with Numa-Denis Fustel de Coulange's *Histoire des institutions politiques de l'ancienne France* (1875). In that work, Torre observes, Fustel de Coulange asserted that institutions are always "the expression of the cohesion among customs, laws, material interests, the way of thinking and the 'spirit' of the generations that govern" (p. 31). The subsequent development of French social history did not follow a linear course from Fustel de Coulange's directions. Constitutional, political, and legal historians contested the validity of the more socially oriented history that was responding to the progress of socialism, sociology, and the materialist philosophy of history.

The fruitful dialogue between history and sociology in the *Année sociologique* further extended historians' appreciation of the nature of society, but it did not decide what labor was specific to history and what was particular to sociology. Henri Berr's *Revue de synthèse historique*, while encouraging regional history, convinced historians to incorporate in their work the new findings and concepts of geography, linguistics, ethnography, and studies of popular culture. Torre's analysis does not treat, however, only those who later became great stars in the French historical firmament; he concretely and informatively cites hundreds of historians who added incrementally to the growth of historical method and practice. Both authors repeatedly stress the chronic struggle waged against political and event-centered history. The evidence they collect suggests, however, that the traditional historians were overwhelmed by the new wave of social history already in motion before World War I.

The postwar years offered the opportunity to exploit and expand the theory, method, and example that would give social history its hegemony in France. Allegra explicates the secrets of this success in the second part of the book. The confrontation with communism, the impact of Maurice Halbwachs, and the decisive work of the *Revue d'histoire économique et sociale* are incisively examined. The *Annales*'s programmatic force, from its founding in 1929 until the present, was possible because of all that was accomplished in the previous fifty years of historical controversy and inquiry.

Today, the *Annales* is self-consciously experiencing earnest doubts about the unity of its purpose. This penetrating study, accenting the labyrinthian ways of historical discourse, illuminates and makes in-

telligible social history's complicated past, and it also makes explicit the generic problems likely to shape this history's present and future contradictions and traumas.

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CHRISTOPHER DUFFY. *Siege Warfare: The Fortress in the Early Modern World, 1495-1660*. Boston: Routledge and Kegan Paul. 1979. Pp. xii, 289. \$22.50.

Christopher Duffy is the author of a number of well-received volumes on the military history of the eighteenth century and the Napoleonic era—*The Wild Goose and the Eagle* (1964), *Borodino and the War of 1812* (1973), *The Army of Frederick the Great* (1974), *The Army of Maria Theresa* (1977), and *Austerlitz* (1977). All—institutional history, battle accounts, or biography—were marked by clear, lively writing, good use of sources, and excellent illustrative material, as well as a certain dry humor. Perhaps inevitable for a military historian of this period, in which warfare was so often conducted within the parameters of a fortress environment, Duffy also developed an interest in fortifications, and the result was *Fire and Stone: The Science of Fortress Warfare, 1660-1860* (1975).

The present volume is a companion book to *Fire and Stone* that examines the centuries during which fortress warfare gradually emerged in the forms that would prevail in Europe until the mid-nineteenth century. It displays all the characteristics of Duffy's previous books—extensive learning, clear style, well-chosen and well-executed illustrations, substantial documentation, and a judicious bibliography.

As his point of departure, the author has chosen 1494, when a French army under Charles VIII, equipped with a strong and effective artillery, burst into Italy and rapidly shattered its complex and delicately balanced political system. Castles and fortresses with which the Italian rulers previously had protected their possessions quickly were subdued by the new weapons. As Guicciardini, the great historian of this period, wrote, sieges that previously had taken months now took days or even hours. The events of 1494 forced a basic re-evaluation of fortification systems.

The Italian response was a new type of fortification to replace the medieval castle, which had repelled attack primarily by physical obstacles. The new model, based on a bastion trace subjecting the attacker to an effective cross fire, provided an answer by turning walls and ramparts from mere obstacles into fire bases. Concentrating his fire in turn, the attacker made progress in battering breaches

into the new ramparts, while the defense replied by adding outworks and arming fortresses with more far-reaching artillery. Italian, French, and Dutch schools of fortification and siegecraft developed, ultimately culminating in the work of the great Vauban, master of fortress design and siege warfare.

But this book is not concerned primarily with the technicalities of fortress design, armament, or even siege warfare, though it does cover these, too. It is a survey rather than a monograph, and its main purpose is to analyze warfare in a period and in an environment ever more dominated by sophisticated and costly fortifications. It embraces military as well as social and economic dimensions and does not restrict itself to Western Europe, but also deals with Eastern Europe, the Ottoman Empire, and the Far East, although here, especially in the dominion of the Turk, the art of fortification and siege warfare long remained rudimentary.

Not unlike logistics, a subject often neglected in conventional writings on campaigns, fortifications provided the basic framework for most of the wars during this period, dominating strategy and requiring, at least in Europe, the vast and cumbersome siege trains that slowed down campaigns and made logistics so all important. Beyond that, the development of siege warfare, as Duffy points out, required the centralization of resources and gave an impetus to the rise of the modern bureaucratic states. Scholars, buffs, and plain historians will find this an interesting and useful book.

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ERIC J. LEED, *No Man's Land: Combat & Identity in World War I*. New York: Cambridge University Press. 1979. Pp. xii, 257. \$14.95.

Most studies of World War I, like those of other wars, are "history from above" in treating causes, course, and consequences. What little we sense of the individual's experience derives largely from specific novels, poems, and memoirs rather than analytical "histories from below." Leed's work responds to this need and will be compared to Paul Fussell's *The Great War and Modern Memory* (1975), a comparison worthy of both authors and instructive to readers. Whereas Fussell focuses on literary and linguistic responses, Leed is concerned with psychological results. By reaching many of the same conclusions from different starting points, the two books validate and complement each other. Taken together, they deserve to be regarded as the seminal works on the individual's experience of war, its implications for Western society, and the nature of modernity.

Leed's main concern is the transformation of per-

sonality by war, in particular, the "cultural repertoire of meaning drawn upon by participants to define felt alterations in themselves" (p. ix). More generally, he seeks to "isolate and define the way in which an historical event of the first magnitude contributed to the character and definition of modernity" (p. ix). He concludes that "the war experience is an ultimate confirmation of the power of men to ascribe meaning and pattern to a world, even when the world seemed to resist all patterning" (p. x).

The war's structure and apparent autonomy produced in the individual soldier a sense of discontinuity of self and identity. There emerged what Leed denotes as "liminal man," a marginal character caught in the no man's land of the book's title between front and home, severed from society by mobilization and unable to reintegrate after demobilization. Adjustment to war was complicated by the intellectual baggage from the prewar period, above all, romantic perceptions of war as escape from modernity and purifier of bourgeois society. This vision, apparently realized in the class-negating "community of August" 1914, proved ephemeral: idealistic middle-class volunteers were frequently despised by working-class draftees, and utopian expectations were shattered by the grotesque realities of war—above all, the dominion of technology over men and the dilemma of trench warfare that victory required attack but defense was superior.

In response Leed perceives a defensive personality which sought to survive rather than win and directed hatred more against the war, home, and superiors than the enemy. One method of survival was fantasies and myth. Leed concurs with Fussell's comment: "That such a myth-ridden world could take shape in the midst of a war representing a triumph of modern industrialization, materialism and mechanism is an anomaly worth considering" (p. 115). Leed sees this anomaly explained by psychic need, myths being an "attempt to close the gap between the surprising realities of life and initial expectations" (p. 116). He describes a variety of fragmentary but widely shared visions of release—particularly the dream of flying—and nightmares of interment in a war underground (Fussell calls it "the troglodyte world") and Jünger's myth of war as a beneficent machine. The other "exit from the labyrinth" was neurosis, "a flight from an intolerable, destructive reality through illness" (p. 164). It was complicated by what Leed calls "the politics of neurosis," official rejection because of the threat to discipline and thus prosecution of war. A further difficulty was the minimal success of treatment through discipline, moralizing, and psychoanalysis. Yet neuroses were pervasive—some claimed univer-

sal—among front soldiers and not only did not disappear after the war but increased.

Veterans found it generally awkward, frequently impossible, to reintegrate into civilian society because of what Leed calls the collapse of "the economy of sacrifice": wartime recognition of the soldier's sacrifice of self was replaced by commercial values and the profiteer became the new model. The veteran was consequently confronted with the hypothetical choice of renouncing his vision to rejoin society or retaining his vision and remaining an alien in his own country. In reality, however, he had no choice since he had so internalized the war in the form of private myths and neuroses.

Yet these myths and neuroses did not remain private but helped to shape the interwar period. The First World War was not only a "nodal point in the history of industrial civilization" (p. 193) because material realities and traditional mentalities collided but also "the first holocaust" (p. 213) repeated in the Second World War. Leed thus makes it clear that war affected and was affected by modern culture. Indeed, war may be the dominant myth of modernity.

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ANTONELLO VENTURI. *Rivoluzionari russi in Italia, 1917-1921*. (Biblioteca di Storia Contemporanea. I Fatti e le Idee, Saggi e Biografie, number 424.) Milan: Feltrinelli. 1979. Pp. 265. L. 8,000.

Socialists who yelled "Abasso la guerra!" outside the gates of Fiat-Centro in March of 1917 began to hear not an echo but the response "Fare come in Russia!" Of course, no one in Turin or anywhere else in Italy knew just what that meant, for the news from Petrograd was spotty and its import uncertain. In the first few months the task of explaining Russian developments to readers of the leftist press was—regrettably—left to émigrés, always notoriously unreliable when it comes to explaining contemporary developments in the homeland.

It is customary for historians to speak of the "tragedy of Italian socialism," uncommon to find them examining the Russian role in it. Three recent works have helped reduce our ignorance. Stefano Caretti's *La rivoluzione russa e il socialismo italiano* (1974) and Helmut König's *Lenin und der italienische Sozialismus* (1967) are both more meticulously researched than is Antonello Venturi's book, and some readers will no doubt prefer their more cautious judgments. But the three studies complement each other more than they overlap, and Venturi's, the newest, can be judged strictly on its own merits.

Venturi divides his book into four excessively long chapters, and it is easy to question his priorities. Far too much space (sixty-five pages) goes to V. V. Sukhomlin, a Socialist Revolutionary and political naïf who merely happened to be hanging around the offices of *Avanti!* when the February Revolution erupted in Russia. Sukhomlin understood that event imperfectly, and, in any event, he soon left for Petrograd. Michael Vodovosov, his undistinguished successor at the Socialist newspaper, sought to interpret the October Revolution; his method consisted largely of quoting Lenin. A more substantial figure was Nicholas Liubarsky, who had to deal with issues that were of overwhelming importance for Italian Socialists: Comintern membership and the question of the *consigli di fabbrica*. He also had the task of trying to bring Giacinto Menotti Serrati and Antonio Gramsci into the Soviet fold.

These latter issues and personalities receive Venturi's close attention in the last, and best, third of this uneven work. He gives a good account of the important strike of April 1920 in Turin, and he deals sympathetically and intelligently with the sad Serrati mission to Russia. American readers will also be interested in Venturi's discussion of Marc Slonim's adventures in Italy.

WOODFORD MCCLELLAN
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R. T. THOMAS. *Britain and Vichy: The Dilemma of Anglo-French Relations, 1940-42*. (The Making of the 20th Century.) New York: St. Martin's Press. 1979. Pp. vii, 230. \$18.95.

"It is difficult to discern a clear pattern in Britain's policy towards Vichy, or even make, in regard to that policy, a single statement which does not require endless qualifications" (pp. 178-79). The tone is almost regretful, yet in describing the confused and undirected efforts of the British government, R. T. Thomas has produced a highly coherent and well-written study.

The collapse of France in 1940 with the subsequent rise to power of Marshal Pétain presented Britain, as the author shows, with a situation for which it had no previous experience. Vichy was hardly a sovereign state, but it still had a fleet and colonies that the British wanted to keep out of the hands of the Germans. They also hoped to make Pétain and his cronies less collaborationist and eventually to bring France back into the war on their side.

The times were hardly propitious for normalizing relations, however, and shortly after the 1940 armistice, Britain's justifiable fears about the future use

of the French fleet led to the attack at Mers-el-Kebir, which Thomas recognizes as inevitable but calls a blunder that "poisoned all future dealings between London and Vichy" (p. 46).

The book, in addition to discussing London's Vichy connection, also deals with the two other branches of British foreign policy: assisting the Free French movement in hopes that General de Gaulle would be able to rally the French Empire and negotiating with General Weygand, Vichy delegate general in North Africa, to try to coax North Africa into the war on the Allied side. "British policy vacillated between these three alternatives without finding a solution in exclusive pursuit of any of them," Thomas observes (p. 55).

Britain and Vichy falls into two parts: Britain's Vichy policy and Britain's response to the Vichy policy of the United States. Thomas shows that although the Americans shared Britain's overall aims, they were more eager to strike a deal with Vichy and were much more antipathetic to de Gaulle, so much so in fact that London feared that Roosevelt intended to make the Vichy regime the basis of the postwar government of France. Furthermore, while the British, after the defeat of Hitler, wanted to restore France to her status as a great power, the Americans wanted to keep the country weak and subservient. That American interests were not served in this way was due less to British objection than to the evolution of forces over which neither country had any control, for example, the murder of Admiral Darlan and the political skill of de Gaulle himself.

Both the British and Americans pursued policies that they hoped would better help them fight the war against Germany, but, as Thomas's study shows, the best course of action during a time of war might have the worst possible consequences after that war is over.

Britain and Vichy is a well-written study by one who has complete mastery of his resource materials. In handling well a difficult subject Thomas has maintained the high standards of the admirable "Making of the 20th Century" series in which his volume appears.

W. LAIRD KLEINE-AHLBRANDT
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LILLY MARCOU. *L'Internationale après Staline*. Paris: Bernard Grasset. 1979. Pp. 316.

When the fall of Khrushchev shook their doctrinal certainty, the Communist parties, as Lilly Marcou sees them, were "confrontés à une réalité diverse, diffuse, glissante, imprévisible" (p. 181). Most people find reality as insecure as this all of the time,

but Communists are different, and the historian has turned poet to capture their pathos. Other illuminations of this kind guide the reader through a labyrinthine passage of a dozen years in the history of world communism. The subject is the progression of international conferences, the "International" after the three Internationals, that tried to maintain unity following the death of Stalin and the disappearance of the Cominform. It is important, but only the touch of the poet keeps it from being profoundly dull.

The conferences declined from a tour de force of unity in 1957 to an admission of disunity in 1969. The Soviet Union was struggling against the spirit of national autonomy, in particular that spirit as an instrument of China's competitive claim upon world Communist leadership. At the first conference, China was supporting Russia; at the second, in 1960, it was reducing "le rôle dirigeant du Parti communiste de l'Union soviétique à une rôle historique et honorifique, plus que politique" (pp. 111-12); at the third and last conference, China was the enemy. To the author this split helped provide the conditions for the appearance of Eurocommunism, with its gentler, more reasonably hopeful promise.

One might object to the rigor of Lilly Marcou's self-discipline in keeping to the main issues and their protagonists. Her characters, Khrushchev, Brezhnev, Mao, Togliatti, Hoxha, Thorez, and others, are as select and lordly as the dramatis personae of a Racine play, and she might have inquired further into their social objectives and relation to the silent masses.

In her scholarly restraint, the author is more objective than her intent, which was to proceed, on the recommendation of Claude Lévi-Strauss, from a "lecture de gauche . . . avec l'âme de la pensée dialectique" (p. 14). Supplementing exhaustive research among the documents with interviews of living, talking participants, she has discovered as many of the essential facts as possible and let them tell the story without tenderness for her own *gauchisme*.

Properly seeking more enlightenment from history, Lilly Marcou goes back a century to examine Karl Marx's management of the First International. She finds in it a model resolution of the conflict between centralism and autonomy (p. 23). This encourages her hopefulness about the effects of the weakening of Soviet international control and the concomitant rise of Eurocommunism. But her ability to distinguish factuality from myth-making, evident throughout this study, lets her see the "force mythique" in Marx's International (p. 14). That mythical element has hidden the centralizing principles and iron will of Marx the leader, who broke up the organization rather than lose control of it to

anarchists and other elements demanding autonomy. Plus ça change. . . .

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JOHN LUKACS. *1945: Year Zero*. New York: Doubleday. 1978. Pp. 322. \$8.95.

For John Lukacs, 1914 was the beginning of the end and 1945 the end of the end of the old era. A European-centered world was finally destroyed as barbarous Oriental hordes moved in from the East and well-intentioned, but naive, Americans marched in, a little too slowly, from the West. Lukacs's chatty series of essays is always literate, often provocative, and sometimes wrong. He devotes separate chapters to the main personalities of 1945—Hitler, a vulgar, brutal, shrewd nationalist; Churchill, who was almost always correct except when it came to his understanding of American policy; Roosevelt, weakened but with his Wilsonian illusions still intact; Stalin, a scheming, suspicious nationalist; and Truman, "a national blessing" (p. 134) who quickly grew with his job. He offers also an analysis of American opinion in his "Year Zero" and concludes with a moving memoir of Hungary in 1945 as the Nazis withdrew and the Russians took over.

Aside from the chapter on public opinion, Lukacs presents little evidence that he has spent much time with primary sources or even recent monographs. Further, his interpretations are not new. He covered the same ground in his *A History of the Cold War* (1961). According to Lukacs, the Cold War did not begin because of calculated Soviet or Russian imperialism. Stalin moved into a vacuum, and when the Americans did not react early on, as Churchill hoped they would, the Russian leader naturally made the most of the opportunity. If only a line had been drawn in 1942, the Iron Curtain would have clanked down much further east.

Yet this book is worth reading for the variety of penetrating insights Lukacs presents and for his ability to capture in a few phrases the dominant characteristics of an individual or a nation. Perhaps he overemphasizes the ethnic factor, whether it is the Oriental nature of the Russians or the multinational character of the United States. Characteristically, he was disturbed to find that Americans considered Hungary an Eastern European and not a Central European state.

Lukacs can be nasty, as in his gratuitous slur to countryman György Lukács (p. 297n) or in his contention that Communists in Hungary were generally ugly. Moreover, some of his notions are bizarre as in his claim that little has changed since 1945

with Norman Mailer still our "principal literary figure" (p. 242 n). He is surely off base in his impressionistic survey of magazine opinion, which is not informed by the latest scholarship. Even if we have to put up with an occasional bit of Nabokovian word play, however, it is worth waiting for such lines as these that describe Roosevelt: "When an American seductionist goes to the Caucasus he'd better bring plenty of money" (p. 93). *1945*, as with much of Lukacs's work, makes for interesting, if exasperating, reading. He has much to say that merits attention from those who seek to explain the origins of the Cold War.

MELVIN SMALL
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ALAN KREIDER. *English Chantries: The Road to Dissolution*. (Harvard Historical Studies, number 97.) Cambridge: Harvard University Press. 1979. Pp. xi, 279. \$20.00.

It was long believed that the effect of the Henrician and Edwardian Reformation at the parish level would always be beyond our ken due to lack of source material, but recently a new generation of historians has been showing what can be done, largely by using records that have long been known and used in other contexts. In the days when the dissolution of the monasteries loomed large, with interest centering on the dispersal of their great estates, on crown finance and administration, and on local power politics, the chantries with their tiny endowments seemed small beer indeed. But for the ordinary early-Tudor parishioner chantry priests were more familiar figures than abbots and their disappearance more noticeable on the local scene. Following in the footsteps of Christopher Kitching, Alan Kreider has provided the first treatment of the subject on a large scale. Eschewing the actual suppression of these essentially "intercessory institutions," the pensioning of their priests, and the sale of their property, he concentrates instead on attempting to discover what part chantries played during their two centuries of active life and on the run up to their dissolution.

Making full use of the chantry certificates and a great many other sources, Kreider is able to answer more fully than will ever be possible for monasteries the question as to the function of chantries in local society and hence the extent to which they were missed. Although they were established essentially to pray for the dead, Kreider finds that their priests performed a variety of other functions, acting as schoolmasters, auxiliary parish clergy, and dispensers of poor relief. He then proceeds to devote two chapters to the national debate concerning the

existence of purgatory, on the outcome of which the future of the chantries depended, and the book ends with detailed studies of the acts of 1545 and 1547. Although this is familiar ground and there are no startling revelations, it has never been set down so clearly or interpreted so effectively.

This is above all a very honest book. The author makes clear that his detailed findings relate only to four counties. It is to be hoped that similar studies will follow for other counties and that Kreider himself will give us another volume on the suppression and its consequences. He may well discover that the commissioners for the suppression, on whose findings he has so heavily relied, were not entirely objective in their assessments of the indispensability of the chantry personnel. If they followed the traditions established by the augmentations men who dissolved the monasteries, they followed a convenient rule of thumb: to disturb vested interests as little as possible, especially those covered by written agreements. As Kreider recognizes, speed was essential to the success of the operation, even if this entailed some loss of profit to the crown in the short run.

JOYCE YOUNG
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W. J. SHEILS. *The Puritans in the Diocese of Peterborough, 1558-1610*. (Publications of the Northamptonshire Record Society, number 30.) Northampton: The Society. 1979. Pp. xi, 166. £7.50.

W. J. Sheils has added to the production of local Puritan geographical studies, in this case the diocese of Peterborough (coterminous with the counties of Northampton and Rutland) for the period of 1558-1610. His approach is to follow the outlines of the national history of Puritanism, primarily as laid out by Patrick Collinson in *The Elizabethan Puritan Movement* (1967), and then to examine the local manifestations of Puritanism. The choice of the Peterborough diocese for a new examination of Puritanism at the grass-roots level proves to be a good one because of the fertile growth experienced by Puritanism in the area. Among the notable Puritans with Peterborough connections were John Penry, who married into a Northampton family, Robert Browne, the original Brownist, William Hackett, the idealistic revolutionary, Peter Wentworth, the parliamentarian, Sir Walter Mildmay, and the Cecil family, to name a few. Browne settled at Thorpe Achurch in the diocese after returning from Dutch exile and renouncing his separatism. Even thereafter, he revealed several Nonconformist tendencies. During the enforcement of Bancroft's canons in 1604, sixteen clergy were deprived, the highest number of any diocese. One of the North-

ampton preachers described Puritans as "the hotter sort of Protestants," and Peterborough was well supplied with them.

Peterborough Puritanism, as described by Sheils, parallels quite closely the national trends. In the early Elizabethan years, the Puritan clergy operated through area exercises, fast days, and powerful preaching. From 1587 to 1591, the Puritan-minded clergy organized themselves into three classes, thus attempting "presbyterianism within episcopacy." Following the collapse of the classis movement, Sheils sees in the 1590s and beyond a shift to family and parished Puritanism with the more modest goals of "non-separating congregationalism" (p. 68). He gives a valuable analytical as well as descriptive study of the deprivations resulting from the 1604 canons. Sheils calculates that 20 to 28 percent of the Peterborough clergy were actively opposed to the canons, about 12 percent were called before local courts as Nonconformists or involved in other Puritan activity against the canons, and 6 percent (16 clergy out of about 278) were actually deprived. Although the book cuts off at 1610, Sheils projects a few clues about the durability of the Puritan tradition over the next fifty years. Puritanism was still deeply entrenched at the time of the Restoration, and after 1660 the clergy from sixty-five parishes were silenced and removed.

This is a solidly researched book that throws considerable light on the local workings of Puritanism. Much of the book is descriptive and analytical on chronological lines; the last several chapters, however, contain some statistical and map analyses on clergy and gentry. Here and there the reader wishes for more. At one point a bare mention is made of possible radical influences from Dutch refugees at Stamford, but no follow-up is given. The topic of the social and economic program of Puritanism is briefly treated; an extension of this area would be welcome.

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SYBIL M. JACK. *Trade and Industry in Tudor and Stuart England*. (Historical Problems, Studies and Documents, number 27.) Winchester, Mass.: George Allen and Unwin. 1977. Pp. 200. Cloth \$29.95, paper \$12.50.

More than two-thirds of this book is a consideration of John U. Nef's long-dead thesis that an industrial revolution occurred in sixteenth- and seventeenth-century England, alleged by the publishers to be still controversial. The remainder (one-half according to the publishers who seem to be as incapable of counting as of judging historical controversies) con-

sists of thirty-four documents intended to illustrate the discussion. Sybil M. Jack's principles of selecting and editing these sources seem eccentric. One "document," number 25, headed "The Tanner's Costs," is not a single document at all but extracts from two; and it is not a statement of costs in one tanning firm but consists of records of bark sales to a tanner in Nottingham and of sales of fells and hides to a fellmonger and a tanner in Litchfield. Presumably, Jack is trying to illustrate a point about industrial costs made in the discussion, but in doing so she forces her sources into a Procrustean bed.

The main part of the book is interesting, less for what it tells us about Tudor and Stuart industry than for the methodological issues it raises. Jack states that "economic historians have always been principally concerned with growth and development or their absence" (p. 15). She assumes that sixteenth- and seventeenth-century industry must be discussed in the context of economic growth, and there has been no greater growth monger than Nef. But it is untrue that economic historians have *always* studied growth and questionable whether historians of the preindustrial period ought to be primarily concerned with it at all. Preindustrial economies almost by definition lack economic growth. Several years ago Postan suggested that historians of periods before the eighteenth century should be more concerned with the functioning of economies than with changes occurring over time.

Jack is of course aware that Nef's arguments were based upon atypical industries and exaggerated examples. She tries, therefore, to broaden the approach by examining sectorial shifts on the ground that economic growth involves the movement of resources from agriculture and personal services into industry and trade. One sympathizes with her willingness to use economic tools to examine economic history, but the concept of sectors, like that of growth, has been developed to describe modern, industry-based, capital-intensive economies. Its insensitive application to the early modern English economy, with its low levels of specialization, produces a good deal of convoluted debate leading to the banal conclusion that "significant change may have been structural." This statement is elaborated by claiming that "what was occurring concerned the changes which are necessary before revolution can come rather than revolution itself" (p. 115). If this means that the developments of the sixteenth and seventeenth centuries preceded those of the eighteenth and nineteenth, it is true but trite; but if it means that the Nef-type industrial revolution was a prerequisite for industrial changes in later periods, the statement is both logically and evidentially unsound.

It is a pity that Jack decided to cast her examina-

tion of Tudor and Stuart industry in the form she has, for the subject is important. She manages to convey quite a lot of useful information in her pursuit of the chimera of growth; but her discussion is confused by her conceptual apparatus and also by stylistic obscurities and numerous nonsequiturs that litter her text.

L. A. CLARKSON

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A. B. WORDEN, editor. *Edmund Ludlow: A Voyage from the Watch Tower*. Part 5, 1660-1662. (Camden Fourth Series, number 21.) London: Royal Historical Society. 1978. Pp. x, 370. £8.00.

As a result of A. B. Worden's edition of part 5 of *A Voyage from the Watch Tower*, the 1698-99 edition of Ludlow's *Memoirs*, one of the central texts of the Whig canon, can never again be read as it has been in the past. This newly discovered and original version of Ludlow's memoirs, of which part 5 is only a portion and which unfortunately covers only the post-1660 period of Ludlow's life, reappeared in 1970 and is now housed in the Bodleian. Students of the Restoration and of English republicanism in its pre-1660 form will be greatly assisted by Worden's editorial labors. He has done an admirable job of comparing the new text with the old and of identifying the somewhat significant alterations wrought upon the original manuscript by the editor of the 1698-99 text. Worden concludes that this hitherto unknown, although suspected, editor was John Toland (1670-1722). Although the evidence is by no means conclusive, I concur in his judgment.

In his lengthy introduction Worden marshals evidence for that judgment; he also attempts to place the appearance of Ludlow's *Memoirs* and Toland's editorial liberties in the context of the political issues of the late 1690s. His analysis of radical Whiggery in that period occasions the only reservations that must be voiced about what is, in every other way, a superb edition with a most useful introduction. Worden relates and indeed endorses much of the slanderous musing that surrounded the career of Toland, an infamous freethinker, republican, and, to use his own term, religious "pantheist." To fall prey to this propaganda would be to assume, as did some contemporaries, that Toland's religiosity was fraudulent and to fail therefore to see the relationship among his religious beliefs, his political projects, and his literary endeavors.

In his interpretation of Toland's context and motivation it would appear that Worden has allowed himself to be misled by the clergy. Of course they thought that they knew a great deal about this "incendiary," as the secretary of state called him. After

all, letters addressed to Toland turned up in the library of the Archbishop of Canterbury, Thomas Tenison, with the inscription "Letter to Mr Toland seized in Ireland" written across them. (Worden fails to note that Toland's own access to other people's mail was part of a two-way traffic.) But the historian should tread carefully here; the observations of contemporary enemies should be weighed against a careful reading of Toland's published and unpublished writings, especially his religious writings, since his consistency and originality primarily lie there. Toland's religiosity is of vital importance to any interpretation of his reasons for altering Ludlow's original text. As Worden rightly notes, Toland's doctoring was most ruthlessly surgical on those passages that reveal Ludlow's intense and radical Protestantism and, in particular, his millenarianism. In explicating Toland's alterations it is not sufficient to postulate that they are "a powerful reminder of the changes in belief and feeling which affected English society" in the later seventeenth century (pp. 51-52). Those changes, because they were incorporated into the political vision of the radical Whigs, require explication. And, once understood, they also provide the key to some of Toland's most historically important activities on both sides of the channel as well as to his profoundly historical cast of mind.

Having chosen to see Toland as somewhat disturbed (it is no longer fashionable to equate heresy with just plain sinfulness), Worden fails to understand that for Toland, as for Collins, the creation of a new natural religion, devoid of traditional Christian metaphysics and its priestly overseers, lay at the heart of his version of what should be the new Whig order in Church and state. And up until at least 1701 Toland was a valued member of "the same college of politicians," as Lord Methuen called it, which met among other places at the Grecian. No evidence from the 1690s suggests a separate "Roman" Whig faction, however closely allied with Toland's "Calves-Head" group, as Worden would have it. Rather, the evidence, including the Trenchard-Simpson correspondence at the Spencer Library (University of Kansas), which Worden understandably appears not to have seen, suggests no discernible distinction between these "politicians" (as one letter there speaks of Toland and Tindal) in the "college" and the crowd at the Grecian Tavern. Likewise, because Worden underestimates Toland he imagines that he was trying to rally the country—that is, Roman Whigs—to Calves-Head republicanism but inadvertently succeeded in transforming the latter into its muted version that became in the course of the eighteenth century the credo of the "country." It seems much more probable that this astute politician was intending some-

thing rather closer to what Worden describes as having happened.

In assessing the variety of motivations available to Toland as editor, Worden fails to grasp that he was also quite simply a historian with strongly antiquarian tendencies. In a letter to "a fellow collegian" written toward the end of his life, Toland saw himself as the successor of Anthony à Wood—an odd comment, given their political antipathies but perfectly understandable once Toland's significant historical and antiquarian research, particularly into the history of religions, is clearly interpreted and not ignored for the sake of pursuing his "purely" political activities.

Finally, it is mistaken to imagine the country Whigs, wedded to their estates in deepest wherever, as possessing "broader" horizons and greater intellectual depth than freethinkers like Toland. He and Collins were profoundly important religious thinkers whose forays onto the Continent implanted their ideas among circles of French Protestant refugees living in the Netherlands, some of whom went on to become leaders within European freemasonry. Although Ludlow requested that his text be edited to suit a new age, doubtless he would have strenuously objected to Toland's effort to make it speak not of "Christ ruleing" but of virtues and ethics derived from stoicism and intended for adoption in a visionary republic of pantheists. Toland's republicanism was no less firmly rooted in religion than Ludlow's.

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JAMES DALY. *Sir Robert Filmer and English Political Thought*. Toronto: University of Toronto Press. 1979. Pp. xiv, 212. \$22.50.

Those of you who learned basic seventeenth-century English political thought more than twenty years ago must now certainly retrain yourselves. It used to be simple Whig versus Tory, royalist versus parliamentary, absolutism versus liberty paradigms, easy to trace from Gooch or Allen. It was Hobbes, bad; Locke, good; and the test was simple Victorian liberalism. Not any more. From Pocock and Greenleaf to Kenyon and Schochet, all we have learned is that Stuart political morality, if it existed, had its own orientations that cannot be slighted merely because we fail to grasp their urgency.

We once knew Sir Robert Filmer as a quixotic Civil War royalist, author of *Patriarcha* (posthumously published in 1680), the absolutist absolutely vanquished by Locke's first treatise. But his

rehabilitation as a forceful theorist, first begun by Peter Laslett in 1949, more recently enhanced by Gordon Schochet and J. P. Kenyon, has been completed in this superb study by James Daly of McMaster University. In a style lucid, at times puckish, Daly takes us through a fresh version of Filmer, not so much by way of textual analysis (Daly virtually admits that Filmer was blindly single purposed) but by placing him in a world of wide-ranging possibilities and complex intellectual incentives. In an adroit presentation of Filmer's ideological alternatives, Daly presents in his second chapter alone a virtual compendium of seventeenth-century political ideas.

Among the author's numerous Filmerian attributions, we should now include Filmer as having played a part in the demolition of the *meum-et-tuum* world of the Elizabethans, which placed the state in cozy seclusion above society's petty preoccupations with private liberties. The state's totalist right might preempt anything in the private sector. Political adiaphora, the "things indifferent" of Hooker and Locke, were to Filmer without meaning. It was not only that patriarchal authority had been conferred on the state—the most familiar of Filmer's positions—but that the state was possessed of an omnipotent will, always capable of being translated into pure power. Daly's chapter on Filmer's royalist connections, admittedly his weakest, stresses the royalists' avoidance of his works, even during the exclusionist controversy. What we have in the bulk of the study, however, is not only a thorough delineation of mid-Stuart political ideas, one to be read as prelude to J. P. Kenyon's *Revolution Principles* (1977), which covers the ground after 1688, but also a study of man's relation to the state and to his own institutional being.

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BERNARD CAPP. *English Almanacs, 1500–1800: Astrology and the Popular Press*. Ithaca, N.Y.: Cornell University Press. 1979. Pp. 452. \$35.00.

Astrology, as Keith Thomas demonstrated in his *Religion and the Decline of Magic* (1971) and Bernard Capp confirms in this informative book, was a pervasive and flourishing cultural reality in Tudor and Stuart England. "It formed," says Capp, "a part of the dominant pattern of beliefs, though one which slowly declined and which coexisted very uneasily with other hostile elements" (p. 283). The almanacs of the period, in which astrology enjoyed a prominent position, sold up to four hundred thousand copies a year (in the later seventeenth century) and were undoubtedly consulted by a wide range of

readers. Astrology was of interest to both educated and poorly lettered people; it was a major component of popular literature; and the almanacs themselves—the central sources for Capp's enquiry—touched upon an impressive diversity of human concerns, concerns both for the workings of the heavens and for affairs on earth.

Capp's most important contribution lies in his detailed description and analysis of this (often colorful) diversity of concerns. Almanacs included political and religious prognostications and interpretive commentaries on public affairs. Their authors sometimes engaged in social criticism, and they commonly polemicized against corrupt clergymen, Puritans, and Roman Catholics. Astrology, in fact, was an important ideological weapon in the dissensions and intrigues of the period: it was employed to disarm and abuse enemies and to legitimize interests and objectives to which particular groups were already committed. It was also implicated in some of the millenarian thinking of these years.

Almanacs offered certain educational benefits as well. They helped to popularize natural science in a very elementary way: astrologers saw themselves as natural scientists, not as practitioners of the occult, and there was undoubtedly a substantial overlap between astrology and the more recent scientific knowledge. Almanacs served as calendars, helping people to discern with greater subtlety and discrimination the passage of time, and they provided some basic information on human history. They also offered to their readers a considerable volume of miscellaneous information of practical value—information about roads, fairs, farming, weights and measures, and the like. For many readers they were an accessible source of useful knowledge, some of it astrological (and meant to be used for practical effect) and some of it eminently mundane and unexotic. Almanacs, as Capp shows, cannot be categorized easily, for they mixed old beliefs (in magic) with new beliefs (in mechanical philosophy), they entertained with crude sensationalism and instructed with sensible advice, and they included both messianic prophecies and straightforward huckstering and special pleading. Taken as a whole, almanacs impinged upon, and drew their sustenance from, a very wide range of the experiences of the period.

Capp uses his evidence well. He provides an admirable summary and explication of the almanacs' contents, he tries to understand their character and functions within the context of the broader society, and he outlines the changes they underwent in the eighteenth century (which were partly a consequence of the declining confidence in the efficacy of astrological prognostications). He also pays close

attention to the publishing history of almanacs and the careers of their makers. His book is clearly organized and rich in detail, and it succeeds in casting new light on many well-cultivated, and a few less familiar, fields of early modern English history.

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PIERS MACKESY. *The Coward of Minden: The Affair of Lord George Sackville*. New York: St. Martin's Press. 1979. Pp. 279. \$17.95.

For the want of a cavalry charge at Minden on August 1, 1759, Prince Ferdinand's allied army lost the chance to turn a French retreat into a rout. Blame fell on Lord George Sackville, the commander of the British and Hanoverian squadrons, who, despite tradition and the title of Piers Mackesy's book, was hardly a coward. A court martial, however, found him guilty of disobedience and declared him unfit to serve in any military capacity. He recovered from his disgrace in George III's reign; by 1775, having changed his name to Germain, he became the American secretary and, according to Mackesy, bore with courage and intelligence the head load of the war.

Mackesy writes to explain Sackville's action on August first. His narrative agrees substantially with most modern authorities. Sackville was part of a complicated interplay of his own ambitions and England's factional politics, domestic and foreign. Following a quarrel with Ferdinand, Sackville found cause to question in the heat of battle what he believed to be confusing and conflicting orders—to advance to the left through woods he believed impassable and against an enemy he could not see but understood was to the front. His hesitation was also justified, holds Mackesy, because the prince, plagued by doubts, did not tell his subordinates his battle plans. (That caricature, along with many other assertions, needs a critical reading, because a 1761 letter, quoted on page 51 to prove Ferdinand's shortcomings, actually describes those of his nephew, Prince Charles.) As for the Sackville-Ferdinand quarrel, Mackesy offers no new insight or evidence. Nor is he able to penetrate the mystery of why Sackville apparently let pique keep him from reconnoitering the ground or obeying the orders that would have brought him the glory sought by every British officer in Germany. Possibly Lord George was in a state of mind similar to that of Cornwallis, whose refusal in 1780–81 to understand Clinton's clear orders lead to defeat at Yorktown.

Mackesy does, however, present the novel thesis that Sackville's homosexual orientation impaired his relations with others. The evidence, apart from patronage of two suspected homosexuals in the

1770s, comes from rumors, satirical poems, and gossip. Because Sackville's own correspondence file, "Minden Papers," has been missing for over one hundred and fifty years, the truth on many items may never be known. If Mackesy's suggestion is correct, however, it does not fit well with his desire to refute Sir John Fortescue's view that Sackville was a "deplorable" man.

An evaluation of Mackesy's extensive use of primary and secondary materials is impossible for anyone not intimately familiar with the sources because the author excluded reference notes to save space. Thus, *The Coward of Minden*, which might have discouraged further study for years, may in fact stimulate a new interest in Lord George.

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RALPH DAVIS. *The Industrial Revolution and British Overseas Trade*. Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. 135. Cloth \$18.00, paper \$9.00.

The subject of overseas trade as an influential factor in the social and economic growth and development of Great Britain and, in particular, the Industrial Revolution has long been debated in historical literature. This is not an unexpected result given the abundance of extant source materials. Nevertheless, overseas commerce has not been emphasized compared to other causal factors. One of the reasons for this has been the suspicious attitude that scholars have brought to the critical sources upon which depends the proper assessment of the value that overseas trade had on the British economy. Questions involving the balance of trade and payments, for example, have been thought unanswerable because the customs data in the reports of the inspector-general of imports and exports were recorded in "official values" rather than "current real values."

Among all the other questions that the late Ralph Davis and his associates have chosen to address, the computation of current real values for the period 1784–1856 may be considered one of their most important contributions. Great Britain's imports from the region "Northern Europe," for example, amounted to slightly more than an annual average of 10 percent of its total world imports in "official values" pounds sterling for the periods 1784–86 and 1794–96. Davis's computed current real values are, however, approximately 16 and 18 percent, respectively. This means that "Northern Europe" was extraordinarily important to the economic growth and development of Great Britain not only in terms of the enormous volume of imported raw and manufactured materials but also in terms of value.

Davis is not as precise in identifying other crucial

areas of scholarly concern. This less-than-comprehensive treatment may very well be owing to the magnitude of the problems identified in the title of the book and the author's death before a more thorough examination of the problems could be concluded. The reader, therefore, is presented with brief summaries of the components of the import, export, and re-export trade of Great Britain that highlight only a handful of composite commodities. Geographical areas are grouped together for the most part by region and not by country. Much of the chronology is devoted to the first half of the nineteenth century. Although there are adequate citations, there is no bibliography and only an incomplete explanation of the methods and sources used to generate the computed current real values. Finally, there seems to be an ambivalence in concluding on the overall influence of overseas trade on the British Industrial Revolution.

Despite these obvious and understandable shortcomings, Davis's study is a much-valued contribution. It is to be hoped that Davis's associates will continue to contribute more of their findings in article or book form or, at least, make them readily available to scholars who are still trying to unravel the complexities of Britain's international commerce.

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JAMES J. SACK. *The Grenvillites, 1801-29: Party Politics and Factionalism in the Age of Pitt and Liverpool*. Urbana: University of Illinois Press. 1979. Pp. xx, 244. \$12.50.

English politics in the early nineteenth century has hardly received the attention lavished upon the subject for the decades preceding the French Revolution or following Catholic Emancipation. Fluctuating between Namierite connection and an inchoate two-party system, politics in these years may alternately and sometimes simultaneously be considered in terms of Whig and Tory, court and country, or the machinations of Foxites, Pittites, Portland Whigs, Addingtonians, Grenvillites, and Canningites, associating in uneasy and usually fleeting alliances. This first full-length study of the Grenvillites, an important if elusive connection, is based on a wealth of manuscript material and is a welcome addition to the scholarship of the period.

James J. Sack distinguishes two groups among the Grenvillites, the family network and the broader parliamentary party. The former was directed from the family's country house at Stowe by the ambitious first Marquis of Buckingham until his death in 1813, when he was succeeded by his more ambitious son, Earl Temple, later first Duke

of Buckingham and Chandos. The family's influence in Bucks and Cornwall virtually assured their control of six parliamentary constituencies during the first marquis's lifetime, a nucleus enlarged by a train of nephews, in-laws, and hangers-on. In an excellent chapter, the author examines Buckingham's effective, if dubious, use of patronage and verifies the Grenvillites' reputation for venality.

The Grenvillite parliamentary group included the family connection, substantially augmented by politicians, particularly some prominent peers, who associated with Buckingham's younger brother, Lord Grenville. Having served as Pitt's foreign secretary from 1791 until the ministry's resignation in 1801, Grenville opposed the Addington and Pitt governments in the succeeding five years and became the reluctant leader of an influential parliamentary group in opposition. Sack calculates that in 1808, when strongest, the Grenvillite party consisted of twenty-four peers and twenty-two commoners. The bulk of the study is devoted to the forging of the Grenville-Foxite alliance, its brief Talents' Ministry of 1806-07, and the surprising endurance of that alliance in opposition until the schism of 1817 and Grenville's withdrawal from politics, which marked the effective end of the Grenvillite party. In two subsequent chapters, the author discusses the erosion of the remaining Grenvillite faction under the erratic Duke of Buckingham.

Sack excels in tracing the intricate maneuvering of both family and party and their ambiguous relationships to one another. But he does not overcome the tendency of this type of study to diminish political principle. While he repeatedly claims that many were attracted to Grenville's leadership because of ideological considerations, the ideology of Lord Grenville, much less that of the Grenvillite party, remains obscure. The reader does not learn why Grenville supported Catholic Emancipation. The Talents' Ministry's "one outstanding credit" (p. 95), the abolition of the slave trade, is given one line. Sack's analysis of the 1817 rift would have been enriched had he given greater consideration to conflicting ideas of liberty that obtained among Whigs and Grenvillites. Vestiges of the doctoral dissertation are evident in the excessive citations. Nevertheless, the study is useful and workmanlike and deserves the attention of specialists in the period.

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MICHAEL GLOVER. *A Very Slippery Fellow: The Life of Sir Robert Wilson, 1777-1849*. New York: Oxford University Press. 1978. Pp. xiii, 224. \$14.50.

Although not a major figure in the age of Pitt, Castlereagh, and Canning, Sir Robert Wilson deserves

another good biography. Based on Wilson's voluminous papers in the British Library and published works, Michael Glover's study supplements Giovanni Costigan's solid life of Wilson (1932).

Wilson was an erratic paradox. He fought against Napoleon for two decades only to become a partisan of the Napoleonic legend; he defended (in turgid prose) the autocracy of Alexander I and championed reform of Parliament. An incurable romantic, Wilson was never at a loss in self-advertisement, especially in exaggerating his military service and establishing himself as a military expert. His presence at Eylau, Friedland, and especially in the Peninsular campaign enabled Wilson to assert his claim as an authority on military affairs, even though Wellington denounced him as a disgrace to the British officer corps.

Returning to England in 1809, Wilson courted the Tory government for preferment while intriguing with the Whig opposition to make himself "an alternative hero to Wellington" (p. 82). As chief military adviser to the opposition, Wilson's gloomy prophecies on the Peninsular War were used by the Whigs against the government. But, as usual, his prognostications were consistently wrong and "classics of misinterpretation" (p. 83).

In 1812 Wilson was again in Russia and soon a spokesman for dissident Russian generals who demanded that the tsar dismiss his commander-in-chief, Kutuzov. Again, Wilson's wrongheadedness was astounding. A day after he asserted that Austria would not join the fray against Napoleon, Austria declared war against France. Similarly, Wilson foretold certain disaster for the allies at Leipzig, only to be confounded by Napoleon's defeat.

Surviving a disgraceful fiasco during the allied occupation of Paris, Wilson secured election to Parliament as a Radical in 1818, only to achieve a reputation as a parliamentary bore. Even more alarming to his parliamentary colleagues was Wilson's attachment to the Bonapartist cause, but this was counterbalanced by his advocacy of the cause of Queen Caroline against his *bête noire*, George IV.

Wilson was dismissed from the army and contemplated going to South America to lead the rebels against Spain. Deflected from his purpose by his wife's illness, Wilson went to Spain to help the Liberals resist French intervention. He set an example of bravery in a hopeless situation and returned to England one step ahead of Ferdinand VII's police. Wilson was now something of a hero and, veering away from his Radical friends, launched a campaign to recover his military rank. It was only after the death of George IV in 1830 that Robert Peel persuaded Wellington to permit restoration of Wilson's rank in the army. Having ruined his political career by equivocating during the Reform Bill de-

bates, Wilson gradually retired from politics. Melting with age, he now sought only a comfortable berth, which Peel provided by appointing Wilson Governor and Commander-in-Chief of Gibraltar in 1841. His six years on the Rock were quiet and "a fitting end to a long and turbulent life" (p. 196).

Glover's biography of Wilson is lucid, interesting, and enhanced by a useful bibliography and index. Unfortunately, it is ill served by an inconveniently placed footnote apparatus.

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DAVID ROBERTS. *Paternalism in Early Victorian England*. New Brunswick, N.J.: Rutgers University Press. 1979. Pp. x, 337, \$22.50.

It comes as something of a shock to realize that David Roberts's major study of early Victorian administrative history, *Victorian Origins of the British Welfare State*, was published as long ago as 1960. Now we have its successor—the book about which his friends have enquired and to which they have looked forward for some time. Roberts has returned to one of the themes mentioned, but not explored, in his earlier book. As so often in the writing of history, one good book has grown out of another. The idea of paternalism is familiar enough to students of the period. What Roberts has done is to take this concept and examine it in its various ramifications, particularly during the 1840s.

Paternalism in Early Victorian England is divided into three parts. Part 1 looks at the theory of paternalism as formulated by intellectuals. Between 1827, when Kenelm Digby published his *Broad Stone of Honour*, and 1847, the date of Arthur Helps's *Friends in Council*, more than thirty books espousing paternalist social ideas were published. In addition, novels, pamphlets, and articles championed the same principles. Roberts examines the reasons for this revival of older ideas and shows how they were disseminated by the Tory reviews.

The second part, "Paternalism at the Grass Roots," contains a valuable case study of Sussex, using local newspapers and poor law papers. The roles of landlords, clergy, and "captains of industry" are detailed in subsequent chapters. In part 3 Roberts analyzes the impact of paternalist ideas on politics and legislation. He shows that "since paternalism was a cluster of attitudes and ideas that revolved around differing attachments to private property, the Church of England, and various institutions of government" (p. 211) it produced several varieties of paternalist M.P.'s. These ranged from romantics like the Young Englanders to Peelites, Church of Englandites, and country squires. Paternalism as a literary and political force reached its peak of influ-

ence in 1844, argues Roberts. Thereafter it came to seem increasingly irrelevant. In the bourgeois England of post-1848 it appealed neither to the confident middle class nor to the radical, urban working men.

It is, of course, perfectly logical to study paternalism in the first instance through the paternalists themselves. This Roberts does and does excellently. The question for the historian, however, is whether this is enough. At times one almost feels that the author has become a prisoner of his own paternalists (having lived with them no doubt, off and on, for so long) and has lost the power to see another side of things. Roberts recognizes that deference is the obverse side of paternalism; but we hear almost nothing about deference from those who had to defer (or did not). History from below is notoriously difficult to write; but surely something needs to be said about paternalism from the receiving end? Although there are brief references to poor law riots, incendiarianism, and Captain Swing, the full significance of these breaches in the wall of deference is not grasped. Nor is there any appreciation of urban radicalism in this period. In the summer of 1839 the Chartist *Northern Star* was selling fifty thousand copies—the largest circulation ever for a radical newspaper—and radical ideas penetrated into strongholds of northern paternalism. Roberts is too good a historian not to see the gap between the ideal of paternalism and the reality. But somehow the significance of the obverse dimension seems to have escaped him.

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JAMES H. TREBLE. *Urban Poverty in Britain, 1830-1914*. New York: St. Martin's Press. 1979. Pp. 216. \$20.00.

James H. Treble has provided us with a useful study of poverty in nineteenth-century Britain, but he blazes few new trails. He has combed the main printed sources most diligently and has incorporated some fresh material, particularly from Scotland, but his method is largely descriptive. In chapters 1 and 2, the best in the book, he brings together impressive quantities of evidence on movements in wage rates; he also provides a most skillful account of the importance of casual labor and structural underemployment to the problem of poverty. He proceeds by weight of example, which can make for dull reading on occasion, and he makes little attempt to compute his evidence and enter the minefield of debate on the extent to which poverty had diminished before the great upsurge of interest in the "social question" in the 1880s. Aware as one is of Treble's great knowledge about poverty and unemployment across the trades and throughout

Britain (though the mining districts receive relatively light treatment), it is disappointing to find a conclusion no sharper than this: "most of the available indices, qualitative as well as quantitative, suggest that primary poverty was more deeply entrenched and of wider dimensions and that the amount of secondary poverty was on no less a scale than by the time Booth and Rowntree had turned their attention to what, for them, was the burning social issue of the day" (p. 188). This sentence might also serve to demonstrate Treble's less-than-economical writing style.

It is also a drawback for those seeking raw dialectical meat that the author devotes relatively little space to the anterior causes of poverty. He is obviously right to stress casualty and underemployment but we are left uncertain why casualty was so prevalent in so many industries. The British economy was still immature in many respects, and entrepreneurs, probably to a greater extent than they realized, needed a large pool of underemployed labor on which they could draw at need (and at low wages) to trade competitively. We need to know more about the kind of economy that produced this kind of demoralizing poverty. Or, to take another of many possible lines, does Treble think that social historians have allowed the pendulum to swing too far in apportioning blame for the extent of poverty? Many Victorians could not see beyond individual improvidence; but do writers like Stedman Jones, in his generally excellent *Outcast London* (1971), play down the extent to which the poor made the worst of the unenviable circumstances they inherited? It is not necessary to swallow the individualist ethic of the Charity Organization Society to concede the basic point that far too many heads of household spent far too much time and money in the beer shop and visited the results of their improvident habits on their wives and children. Nor is it plausible to argue that the vortex of deprivation drew menfolk inexorably thither, since thousands avoided the snare.

The later chapters of the book contain little that is new. Readers will learn little about the operation of the poor law that they have not already gleaned from Michael Rose or from Derek Fraser's excellent symposium, *The New Poor Law in the Nineteenth Century* (1976). Nor does the material on diet take us far beyond John Burnett's immensely valuable *Plenty & Want* (1966). One wonders how far historians of late nineteenth- and early twentieth-century patterns of consumption can proceed without resort to oral history to flesh out skeletal sources.

I would like to end with two minor, but not insignificant, gripes. The book has been inadequately proofread. Some comical misprints survive; some 1946s should be 1846s; and, surely, proofreaders should not permit a misspelling of the author's

name to stand (p. 200, n. 192). Also, the publishers have resorted to the dubious, but increasingly common, economy of omitting a bibliography and making footnotes (collected at the back, of course) serve as "Sources and References." This is not good enough, and chasing a full reference back down a long chain containing *opere cit.* is far too time consuming as well. Authors inevitably use more works and collections than they have space to cite directly; other scholars often wish to know which.

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DAVID ROBERTSON. *Sir Charles Eastlake and the Victorian Art World*. Princeton: Princeton University Press. 1978. Pp. xvii, 468. \$50.00.

The two pre-eminent figures in the "Victorian Art World," as David Robertson reasonably suggests, were John Ruskin and Sir Charles Eastlake. Whereas Ruskin stood outside the artistic establishment of Victorian England, Eastlake was at its very heart. The first part of this book traces Eastlake's achievements as a member and then president of the Royal Academy, as keeper and then director of the National Gallery, and as secretary of the Fine Arts Commission responsible for the interior decoration of the new Houses of Parliament. The operations of these official bodies are thoroughly researched and briskly described.

The greatest value of this book (and nearly half its considerable bulk) lies in its appendixes, which qualify it as a work of reference that students of the period cannot fail to find useful. Appendix D comprises a detailed catalogue of acquisitions made by the National Gallery from its inception until Eastlake's death in 1865; Appendix E provides a breathless, seventy-page summary of the principal pictures shown at the Royal Academy from 1850 to 1865, with excerpts from contemporary reviews. The bibliography of Victorian source materials is the most comprehensive yet compiled.

Eastlake's own paintings are little regarded today, but in cataloguing his exhibited pictures Robertson is unnecessarily apologetic about the space thus allotted to a minor artist. Eastlake's Italianate portraits and religious tableaux of 1835-45 were extremely well received by critics and patrons alike; his skill in evoking a formula exactly suited to the taste of the day is not to be underrated. In general, the author refrains from drawing conclusions from the facts he has so admirably marshaled. Eastlake's scholarly interest in the *quattrocentisti*, for example, and his desire to obtain their works for the National Gallery are clearly demonstrated; equally clear is the gallery's repeated failure to obtain such works while Eastlake held the post of keeper. Was

he unwilling, or unable, to communicate his enthusiasm and expertise to the trustees? One would like to know more of Eastlake as a personality. His marriage to Elizabeth Rigby—a marriage that, according to Samuel Rogers, astonished all Europe—is another topic into which the present book should provoke research. It is tempting to assume that the articulate, astringent Lady Eastlake, outspoken critic of *Jane Eyre* and *Modern Painters*, exerted a powerful influence on her less forceful husband. Although there is little in this study (or in Marion Lochhead's biography of Lady Eastlake) to confirm or refute our suspicions, illumination is surely to be found in the copious correspondence cited in the bibliography.

This book, however, is not offered primarily as a biography; Robertson describes it more accurately as "a piece of cultural history with a biographical centre." In documenting the National Gallery's purchases and omissions, the activities of the leading artists, and the violent controversies in the press and in Parliament, he has effectively illustrated the changing tastes of the eventful early years of Victoria's reign. Above all, he has opened up the field for future studies. Eastlake gave to his own magnum opus the typically self-effacing title of "Materials for a History of Oil Painting"; the present volume, which follows in the same traditions of scholarship and modesty, might as justly be entitled "Materials for a History of Victorian Art."

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LYNN HOLLEN LEES. *Exiles of Erin: Irish Migrants in Victorian London*. Ithaca, N.Y.: Cornell University Press. 1979. Pp. 276. \$16.50.

Exiles of Erin is an excellent synthesis of social-scientific theory, historical experience, and quantitative techniques. Lynn Hollen Lees reviews the complete experience of the Irish migrants who settled in London during the 1850s and 1860s. She examines their origins, the characteristics of their migration, the occupations they took up, the areas in which they lived, and their subsequent demographic, cultural, and political behavior. Lees has the happy knack of being able to write with the charm and style of the traditional historian while unobtrusively lacing her text with quantitative insights, the value of which even the most antinumerate among us must recognize and appreciate.

By 1861 there were 107,000 Irish men, women, and children living in London. Add to them the English-born children of Irish parents and one has a community of some 178,000. The migrants came

not from the most poverty-stricken and densely populated parts of Ireland but from relatively prosperous areas, where traditional economic and social structures were already crumbling. Almost half came as family groups straight to London. Once there, they were to be found throughout the city, not locked in an urban ghetto. Yet, while living close to the English, they remained apart, tending to concentrate in certain streets or courts, moving frequently (half the inhabitants of one court in 1861 were said to have moved in a month) though rarely going far. "At all ages and all stages of the life cycle" they held occupations demanding the lowest skills.

Contrary to the literary evidence the London Irish quickly adjusted their demographic behavior to that of the host population. Their age at marriage was soon virtually identical to that of the London population generally; so was their family and household size. As to the composition of the latter, "more lodgers but fewer servants and kin distinguished Irish from English." London did not destroy the Catholicism of the Irish. Admittedly church attendance was low in the 1850s, but this was in part due to the shortage of priests and churches. And in rural Ireland at this time church attendance was similarly low. As the nineteenth century advanced, however, church attendance increased, though it never reached in London the near universal attendance record of Ireland in 1900. Politically, the London Irish were soon extensively involved in Irish nationalist politics. By the end of the nineteenth century, however, they could be "mobilized in causes other than the defence of Ireland or the pope," although, according to Lees, "their activity in English-led, English-based political movements remained limited in London before 1914."

This brief summary of some of Lees's findings does scant justice to the wealth of detail that enlivens her narrative. In *Exiles of Erin* she adds greatly to our understanding of the first major migration of modern times. In doing so, she modifies existing theory, thereby helping anthropologists and sociologists to understand more fully the mass migrations that are such a feature of our own times.

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K. R. M. SHORT. *The Dynamite War: Irish-American Bombers in Victorian Britain*. Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. 278. \$13.50.

This thoroughly researched, detailed, and sound account of the bombing campaign carried out by terrorist groups emerging from the fragmented Fenian movement demands the attention both of specialists

in Irish history and experts concerned with terrorists. It can also be read as entertainment and will be mined by writers of historical fiction.

K. R. M. Short of Westminster College, Oxford, drew on all relevant police records in London and Dublin and, by special permission, had access to Scotland Yard and Home Office papers not due for release until the 1980s. With the aid of these, he weaves together the various threads of a complicated story into a coherent and interesting narrative.

After the failure of the Irish rising in 1867 and the Canadian raid of 1870, terrorism was the only card left to play by those Fenians who rejected legal and constitutional protest. Although agrarian secret societies and the Molly Maguires employed terrorism for economic protest, it had never been approved by Irish republicans from the time of Wolfe Tone to that of James Stephens.

The physical force wing of the Irish nationalists turned to bombing after spending several years in efforts to construct a submarine, which provided a useful contribution to naval architecture but not an effective commerce raider. With the adoption of a bombing campaign, the Clan na Gael and the less responsible Skirmishers of O'Donovan Rossa presented a major problem for the British police from 1881 until 1887. At that time, dynamite was a recent invention, opening new possibilities for terrorism that were being exploited by the Narodniki in contemporary Russia and would be exploited later by anarchists in Europe and the Industrial Workers of the World in America.

Short finds that the Clan na Gael, though not always the Skirmishers, were sensitive to public opinion. This limited what they could do, but the author believes that the Clan na Gael left the field undefeated, because the police were unable to develop a foolproof defense against bombing. The police, too, worked within the limitations imposed by political supervision and budget, but they enjoyed a large measure of success in uncovering diverse plots and placing obstacles in the way of conspiracy.

The efforts of the author to place the "Dynamite War" in its setting are generally effective but are marred by some astonishing errors of fact. On page 13 he writes that the rebellion of 1848 was a symbol of resistance during the potato famine. This would have been difficult as the worst years of the famine were 1845-46. On page 22 we are told that "the Canadian forces conclusively defeated the Fenians at the battle of Ridgeway," when, in fact, the Canadian militia was driven from the field by a Fenian bayonet charge. Such blemishes suggest that the author is more at home in the history of terrorism than in Irish and American history.

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A. J. A. MORRIS. *C. P. Trevelyan, 1870-1958: Portrait of a Radical*. Reprint. New York: St. Martin's Press. 1979. Pp. 193. \$19.95.

Convinced that Sir Edward Grey had lied about the extent of Britain's obligations to France, C. P. Trevelyan resigned from the Liberal government at the outbreak of World War I. His denunciation of British intervention proved to be a decisive moment in the march that would transform the Liberal Imperialist of the 1890s into the dedicated socialist of the 1920s.

Making very effective use of the Trevelyan family papers, A. J. A. Morris has analyzed the career of a politician who never achieved the highest offices but whose life illuminates the response of a part of the British political elite to the dissolution of the aristocratic-bourgeois synthesis at the turn of the century, the trauma of World War I, and the Great Depression. Occasionally, an excessive reliance on the Trevelyan papers gives this important short biography a too narrow focus.

Grand-nephew of Thomas Babington Macaulay and son of a prominent Gladstonian politician, Whig and Liberal blood flowed richly through the veins of C. P. Trevelyan. His youngest brother, George, would be regarded by many as the last of the great Whig historians. Born to privilege, the Trevelyan family believed that the advantages conferred upon them could only be justified by service to the nation. While this view can support a cruel paternalism, it led C. P. Trevelyan in a wholly different direction. Privilege—his own included—must go; equal opportunity for all must rise in its place.

Elected to the Commons in 1899, Trevelyan became Under Secretary at the Board of Education in 1908. Belonging to the radical section of the Liberal Party, he enthusiastically supported the social legislation of the pre-1914 years. Among the few to resign over opposition to the war, Trevelyan joined with E. D. Morel in forming the Union of Democratic Control, the most effective opponent of Britain's traditional foreign policy objectives during the conflict. His growing association with radicals, socialists, and trade unionists finally convinced Trevelyan that the qualitative changes required to create a more just and equitable society would never be realized by the Liberal Party.

Retaining many of the fine values of traditional liberalism, Trevelyan joined the Labour Party in 1918. While his commitment to specific socialist policies was often vague, he was quite precise about the need to expand educational opportunity for every citizen. He was a natural choice for President of the Board of Education in the first Labour cabinet in 1924 and again in 1929. Anticipating that Labour would act more courageously when it was

the largest party, Trevelyan was bitterly disappointed over the tepid support for his educational proposals. He resigned early in 1931, lamenting the failure of MacDonald to prescribe socialist medicine for the economic depression rather than vainly attempting to revive stale nostrums.

Defeated in the 1931 general election, he declined a new constituency on the grounds that room must be made for youth. Unfortunately, Morris gives only six pages to the last twenty-seven years of Trevelyan's life. It would have been beneficial to read more of Trevelyan's reaction to Labour's massive victory in 1945. One wonders if the old "radical" judged that Clement Attlee's government had moved Britain irreversibly in the direction of a just society or whether, once again, he found timidity and hesitancy winning out over socialist commitment.

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GISELA C. LEBZELTER. *Political Anti-Semitism in England, 1918-1939*. New York: Holmes and Meier. 1978. Pp. ix, 222. \$26.50.

The limited impact of modern anti-Semitism on political life in England stands in sharp contrast to its influence on the Continent. At the end of the nineteenth century, when anti-Semitic groups in Austria and Germany were achieving some success at the polls and the Dreyfus Affair was agitating France, the Jewish question in England failed to excite widespread popular interest and remained on the periphery of parliamentary politics. Only in the interwar years did it invade the national political arena, and even then its impact was circumscribed. Political anti-Semitism in England, nevertheless, remains an absorbing subject. Understanding its failure there and its success elsewhere makes apparent the essential differences between the social and political development of England and the Continent. Gisela C. Lebzelter's broadly conceived study of the eruption of political anti-Semitism in the 1920s and 1930s not only describes the nature and impact of the major anti-Semitic groups of the time and assesses the responses of Anglo-Jewry, the government, and the radical left but also addresses this larger question of why anti-Semitism never deeply penetrated English political life.

Lebzelter demonstrates that the failure of political anti-Semitism in England can hardly be due to the absence of radical anti-Semitic ideas. Publicists like Henry Hamilton Beamish, Arnold Spencer Leese, and William Joyce depicted the Jewish menace in terms as crude as those employed in Ger-

many. Their Manichean, racially oriented view of an inevitable life-and-death struggle between Jews and Aryans led them to advocate extreme solutions to the Jewish question, including, in the case of Leese, mass extermination.

The weakness of radical anti-Semitism, according to Lebzelter, can be attributed to the character of English political culture and the course of Anglo-Jewish history. There was no historical tradition of political anti-Semitism for anti-Jewish agitators to exploit. The acculturation and integration of Anglo-Jewry in the eighteenth and nineteenth centuries had proceeded far more smoothly than on the Continent. Emancipation never emerged as an important political issue nor did public Jew-baiting ever become respectable or fashionable. Moreover, the Jews were not the only distinctive minority in England. Other religious and ethnic minorities served as lightning rods for the hostility of the majority, while British cultural and racial superiority could be defined in opposition to nonwhite peoples under British colonial rule.

Thus, when Mosley's Fascists attacked Jews verbally and physically, they challenged mainstream English political culture, which condemned the use of physical violence and celebrated the virtues of legality, restraint, and compromise. Most Englishmen, Lebzelter points out, did not reject anti-Jewish prejudice as such, and, indeed, were tolerant of social and cultural expressions of anti-Semitism. What they rejected was the public exploitation of racial hatred for political ends. The government acted to curb the Fascists only when it realized that Jew-baiting was a threat to traditional political values. Some of these observations have been made previously, particularly by Robert Benewick, but Lebzelter's analysis is the most comprehensive to date.

This work will also serve as a valuable corrective to the treatment of Fascist anti-Semitism in Robert Skidelsky's biography of Mosley. There Skidelsky maintains that English Jewry's bitter opposition to British Fascism and German Nazism was largely to blame for the centrality of anti-Semitism in the Fascist program and that Mosley's proposals for the mass deportation of Anglo-Jewry were merely off-hand remarks, largely irrelevant to his program. Lebzelter demolishes this apology, pointing out that for Jews to oppose Hitler, even as early as 1933, was not unreasonable and that Jewish participation in anti-Fascist ranks did not justify Mosley's exploitation of racial prejudice. Finally, she establishes that Mosley's own "final solution" (a phrase he used repeatedly)—forcible expulsion—was not out of character with the substance of his attacks on English Jewry and, thus, should not be dismissed as an unfortunate consequence of heated elec-

tioning. In this and on other issues, Lebzelter's appraisal is judicious and balanced.

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F. H. HINSLEY. *British Intelligence in the Second World War: Its Influence on Strategy and Operations*. Volume 1. Assisted by E. E. THOMAS *et al.* New York: Cambridge University Press. 1979. Pp. xiii, 601. \$24.95.

This is the first volume of the British official history of the influence of intelligence upon the conduct of the Second World War. It shows the general state of the various intelligence agencies at the outbreak of the conflict and traces their development through the Norwegian campaign, the Battle of France, the Battle of Britain, the initial fluctuations of the Desert War in the Middle East, the campaign in Greece, and, as its final episode, the German attack upon Russia in June 1941. In the sense that the authors have had much fuller access to sources—some of which are still too sensitive to be revealed—than any previous writer in the field, the book may be regarded as the first authoritative study of its kind. In the extent to which it is comprehensive, covering as it does all kinds of intelligence from photographic reconnaissance to cipher breaking, it is entitled to the same description.

The chief conclusion to be drawn is that intelligence in general, and the new inferences to be drawn about it in the light of all this new information, exercised only a minor, and certainly not a decisive, effect upon the first two years of the war. It is true that Enigma gave Wavell very good information about Rommel's next moves in May 1941, but this did not make much difference to what happened. On a smaller scale, it is true that intelligence revealed the threat to *Glorious* in the Norwegian battle, but it did not prevent the sinking of the aircraft carrier. This conclusion may surprise some students of the war, especially those who are carried away by the academic hysteria that greeted the publication of the spate of popular histories following Donald McLachlan's initial incursion into naval intelligence. There is, after all, no need to rewrite all war history as some had averred; there is only the need to write much of it in better and clearer terms.

Unfortunately, this last injunction must apply to this volume. The style of writing often falls below an acceptable and sometimes an understandable level. Jargon proliferates and the standard of scholarship is disappointing. Vast numbers of footnotes fail to indicate the nature of the sources, which is one of the essential ingredients of a source reference,

and some are misplaced in the text. There are even footnotes on footnotes, which is a habit that should be stamped out now before it becomes fashionable. The index is pathetic and the list of abbreviations, of which unnecessary numbers are employed, often leaves the reader blinded with science. These scholarly blemishes greatly mar the beginning of a most important control upon the history of the Second World War.

NOBLE FRANKLAND
Eynsham, Oxford

VERNON BOGDANOR. *Devolution*. (OPUS.) New York: Oxford University Press. 1979. Pp. 246. Cloth \$14.95, paper \$5.95.

This book constitutes the most intelligent statement on devolution to appear in the wake of the legislative measures promulgated by the government during the last Parliament. Unlike other studies, it is historical, comprehensive, and interpretive. Vernon Bogdanor examines successively attempts to devolve British governmental power from Gladstone's home rule bill in 1886 through the creation of Northern Ireland (the only area to experience devolution) in 1921 to the rise of Scottish and Welsh national movements in more recent years. But the emphasis of his study is on the present and the likely effect a creation of assemblies on the Celtic fringe will have on British national institutions and way of life. An important theory emerging from his analysis is that much of the current interest in devolution is a reaction to technocracy, corporatism, and other centralizing forces in the modern state.

Despite the attractiveness of devolution as an alternative to impersonal government and as a means to satisfy nationalist demands, there is much in Bogdanor's study to suggest that its application would be problematical at best. The creation of peripheral assemblies would ultimately raise questions about Britain's unitary government and the supremacy of Parliament. A reason why devolution has remained a "damp squib" since the time of Lloyd George has been the apathy of England, which contains 85 percent of the population, yet it would inevitably have far-reaching consequences on that country. Interestingly, the recent Labour government disregarded this fact and acted contrary to its centralist credo by fostering a legislative program that would sustain the support of the devolutionary parties in Parliament. Experience in Northern Ireland has shown, discounting sectarian strife, the advantages of devolving such areas as agriculture and industry, yet these are the very subjects not included in the latest proposals. Furthermore, there is no assurance that the addition of a regional tier (pursuant to the creation of a top layer by Britain's

entry into the European Economic Community) will make government any less remote from the people; it might even have the opposite effect. But the most serious flaw in the late government's devolution package, according to the author, is that it violates one of the canons of good government by separating spending power from the power to tax. This deficiency might provide an opening for more pernicious centrifugal movements and lead to eventual separation. But it is Bogdanor's view, inspired by Burkean and Gladstonian notions of nationality, that devolution need not lead to disunion and could very well result in closer union between England and its fringe areas. Furthermore, the recent legislation, however misguided, illogical, and dangerous, is consistent with Britain's tradition of carrying out radical changes in the operation of government while maintaining the essential form and spirit of its ancient constitution.

There is much in this study that is hypothetical, but it is based upon much solid reasoning and research. Clearly it distinguishes the author as an authority on devolution and supersedes Reginald Coupland's reliable *Welsh and Scottish Nationalism* (1954). But it seems unlikely, given the outcome of the devolution referenda in March 1979 and especially the general election in May, that the subject will attract great interest in the immediate future.

JOHN D. FAIR
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Montgomery*

PAUL BEW. *Land and the National Question in Ireland, 1858-82*. Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. 307. \$15.50.

In this impressive, detailed analysis of the Land League, Paul Bew claims that Barbara Solow's *The Land Question and the Irish Economy, 1870-1903* (1971) exaggerates the improved condition of rural Ireland from the early 1850s to the late 1870s. Increased grazing and continued emigration meant more cows and fewer people. In Connacht, pre-Famine patterns persisted: small farms, a surplus population in relation to economic opportunity, and strong reliance on the potato. When explaining the attraction of the Land League, Bew argues that James S. Donnelly, Jr.'s *The Land and People of Nineteenth Century Cork* (1975) and Joseph Lee's *The Modernization of Irish Society, 1848-1918* (1973) place too much emphasis on rising expectations as a factor in late nineteenth-century agrarian radicalism, and he insists that old nationalist aspirations and agrarian grievances remained as motivating forces. Bew makes an important point concerning the effect of Irish-America on rural consciousness in Ireland. Contact with American values through exiles gave the Irish

masses a political sophistication far beyond their economic and social situation.

Bew affirms Thomas N. Brown's *Irish-American Nationalism* (1966) thesis that the New York-designed "New Departure" strategy of 1879 was the beginning of the Land League. The New Departure projected a war on landlordism as a technique for mobilizing and radicalizing the Irish peasantry as a prelude to revolution. Although Parnell declined an invitation to serve as a constitutional front for Irish-American Republicans, he first embraced and then took command of the Land League, believing that a settlement of the agrarian issue would convert the Anglo-Irish Protestant gentry to nationalism. Although journalists emphasized boycotting, the most effective Land League action was "rent by bayonet": tenants supported by Irish-American money holding out to the last minute before paying rent, always negotiating reductions.

In his major thesis, Bew rejects the historical consensus concerning Land League harmony. There was a basic conflict of interests between its original constituency—the small farmers of the west—and the large farmers of Leinster and Munster who joined in 1880. The latter forced acceptance of the "Three F's" Land Act of 1881, a settlement that offered less than Parnell's hope for peasant proprietorship or Michael Davitt's goal of land nationalization after the achievement of an Irish state. Although it diminished landlordism, the Land Act left the problems of Connacht unsolved, perpetuating the specter of emigration.

Although he does not always write clearly and is too impressed with the views of Marx and Engels on Ireland, Bew has produced a well-researched, intelligent, and important book about a crucial phase of Irish history. He tells a great deal about the ability of Parnell to enlist all segments of Irish nationalism despite the vagueness of his intentions and the complicated interrelationships between the land and the national questions.

LAWRENCE J. MCCAFFREY
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P. W. J. RILEY. *The Union of England and Scotland: A Study in Anglo-Scottish Politics of the Eighteenth Century*. Manchester: Manchester University Press; distributed by Rowman and Littlefield, Totowa, N.J. 1979. Pp. xvi, 351. \$25.00.

People have long debated whether the 1707 incorporating union of England and Scotland has served well the interests of either nation. They will doubtless continue to do so. In the wake of P. W. J. Riley's new study, however, it is no longer possible to see the 1707 act as the culmination of a "natural"

historical development and its architects as men of vision.

As Riley details at length, the union resulted from concerns narrowly parochial and utterly cynical, which displayed slight regard for the needs of England and none at all for those of Scotland. Overwhelmingly, Scottish politicians in the early eighteenth century saw but one decisive objective: securing office and patronage for themselves and their allies while denying both to their rivals. To that end they were prepared to promote in the Scottish Parliament virtually any policy adopted by the court in London. Such politicians went into opposition not to vindicate Scottish patriotism or "country" principles but to create sufficient nuisance to compel the government to take them on board. Virtually no one challenged this arrangement or its central assumption, the English domination of Scottish policy, and Riley speculates that greater concern for Scotland's welfare would have developed only if the realm had remained a separate kingdom in the seventeenth century. But, whatever the reason, real power lay in London, and few Scottish parliamentarians ever imagined themselves challenging that fact: they sought office and its spoils rather than responsibility.

Nevertheless, the cutthroat competition among rival factions—Riley calls it a "jungle" (p. 6)—frequently made Scotland's governance a very difficult matter. The English court and Parliament displayed no greater high-mindedness, but the death of Anne's sole surviving heir made the problem of managing Scotland a matter of acute (if temporary) importance. Although few politicians would have stated it so starkly, Scotland in the early eighteenth century faced the grim alternatives of management or military conquest. Effective management required union, itself the product of a highly complex and fortuitous alliance of English politicians and mutually hostile Scottish groupings. The politics of union were utterly sordid, but on reflection Riley finds the alternative vastly more horrifying.

Within its terms Riley's analysis is unimpeachable, and his work visibly demolishes the traditional view, associated particularly with G. M. Trevelyan and G. S. Pryde, which portrays the union as inevitable and "natural." At the same time Riley's work vindicates at least broadly William Ferguson's recent study of Anglo-Scottish relations and the union.

The Union of England and Scotland runs into difficulty, however, when it encounters the realm of ideas. Fletcher of Saltoun fits uncomfortably within this matrix, and Riley is constrained to stress his uniqueness without ever doing justice to his ideas and the culture from which they derived. But the problem runs deeper than this. However obsessed men may be with patronage, however venal their

preoccupations, however pervasive a spoils system may be, then as now any such structure of politics will inherently require an accompanying structure of ideology. Mayor Daley and Paul Powell, no less than Queensberry and Argyll, inhabited a decidedly ideological world, and that world must be approached through more than the cackling malice of personal rivalry. It is now time to get beyond the poverty of Namierism and the politics of place to the increasingly classicized ideas that ever more powerfully informed even the most venal aspirations of the early eighteenth-century world. Riley has presented us with an able and detailed study but one greatly limited by its severely traditional assumptions.

ARTHUR H. WILLIAMSON
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KENNETH J. LOGUE. *Popular Disturbances in Scotland, 1780-1815*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. 278. \$18.50.

This is a detailed study based mainly on thorough research on the voluminous Scottish judicial papers. It contains accounts of all the disturbances that the author has been able to trace in this period, namely forty-two food riots, about forty antimilitia riots (all in August and September 1797), four others against recruitment, four against Highland clearances, seven political disturbances, six industrial ones, three over church appointments, four over turnpike toll-bars, and four others. As the first to attempt such an account, this is a useful work, especially the chapter on food riots (mostly over oatmeal) that confirms conclusions by students of other food riots about their relation to prices and the distribution network, lack of violence and pillage, and type of people involved.

Very much of the book consists of accounts of disturbance after disturbance and is sometimes heavy going. The author's constant attempts to go beyond narrative are hampered by the nature of his sources. Because his information is often patchy, he is forced to speculate. Newspapers are of little use to him.

Apart from the two chapters on food and militia riots, most of the book consists of disjointed episodes with little in common and gives no sense of continuity in attitudes and actions. One might well question whether "disturbances" really form a subject at all. The book studies "those disturbances which resulted in at least one person being charged with 'mobbing and rioting' as defined by the official prosecutor, either the local Procurator-Fiscal or an official in the Crown Office in Edinburgh. This has been done in order to have some sort of external criterion of what was or was not a popular distur-

bance" (p. 17). This seems an arbitrary definition, dependent not on the action itself but on the response of authorities, and means that the disturbances have no common unity but are a number of isolated events. The fact that he deals with industrial disturbances but not other industrial disputes underlines the fact that certain kinds of disturbance have more in common with other kinds of action than with other disturbances.

The value of the book lies in its account of individual disturbances. The concluding chapter makes some general conclusions, including that, of the mere 450 participants whose occupations are known, 65 percent were "skilled manual workers." The author has read widely on popular disturbances in general, but his lack of information hampers positive comparisons, and he does rather uncritically use terms like "pre-industrial," "watershed," "forward-looking," and "progressive" and assumes that modernization means a decline in violence.

IORWERTH PROTHERO
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IAN DONNACHIE. *A History of the Brewing Industry in Scotland*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xi, 287. \$34.50.

Scotland's brewing industry has long been the poor relation of the much more famous whisky trade. Ian Donnachie sets out to rescue the brewing industry from its historical oblivion. To what extent does he succeed? In the first seven chapters, which cover the period ca. 1650 to 1850 and deal with the origins of the industry, its expansion during the eighteenth century, cyclical fluctuations, the connection with agrarian improvement, capital investment, markets, and management and labor, Donnachie does very well indeed. He successfully surmounts one of the major difficulties of research into the industry—the absence of business papers—by a very thorough search of legal records, especially Court of Session sequestration papers. These provide considerable detail about individual brewing enterprises and flesh out the bare bones of the more readily available aggregate statistics. Such detail is most valuable when it is used in the chapters on agriculture and brewing and on entrepreneurship and capital to tackle important questions about Scotland's economic history. Thus future textbook writers will have little excuse for ignoring the contribution of brewery demand to agriculture, though they may still wonder exactly how much of the demand was satisfied from East Anglia rather than East Lothian and whether relations between brewers and farmers were quite so harmonious as this study implies

given the preponderance of barley prices in brewers' costs. They may have rather greater reservations about an estimate of capital formation that aggregates insurance valuations for the years 1793-1815 without any recognition of the price changes during the period and that assumes (without any attempt at justification) that "the average brewer undervalued his fixed assets in much the same way as cotton mill masters appear to have done" (p. 77). An appendix devoted to the methodology of capital formation estimates in the brewing industry would have been more useful than the list of valuations that is included.

The other five chapters deal with the industry from 1850 to 1977, with the majority of the discussion devoted to technological change, marketing (with a good account of the distinctive Scottish licensing system), and the brewing boom of the 1890s. The final chapter surveys the industry since 1914. Tracing the application of scientific methods in industry is never an easy task, but Donnachie's account seems a good deal more optimistic than earlier work by Sigsworth and by comparison with contemporary activity in the whisky industry. The use of substitutes for barley is attributed to science but arguably had rather more to do with the removal of protection for agriculture. The absence of business papers makes these chapters less satisfactory, for as the industry has become dominated by major firms important areas of business behavior such as pricing policy and competitive relationships become increasingly relevant to the overall structural change that Donnachie describes. Thus collusive behavior is not mentioned despite the existence of trade associations among both producers and retailers and (in 1969) a reference to the Monopolies Commission. Nevertheless, much of the book makes a useful contribution to Scottish industrial history.

RON WEIR
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IAN CARTER. *Farmlife in Northeast Scotland, 1840-1914: The Poor Man's Country*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xiv, 258. \$28.75.

Extensive research in secondary literature has gone into this account of rural society in northeast Scotland. Despite the bucolic title, however, the text rapidly turns into a polemic on the "breaking of a peasant society." Ian Carter examines the relationships of farmers and workers in the different circumstances of the "Golden Age" and the "Great Depression." I have space for discussion of only two major points, the first of which concerns whether there was a distinct "peasantry" that late and whether it really was "broken." Peasants are defined as producing subsistence needs and entering

the market only to be able to pay rent and taxes. "Capitalist" farmers are defined as producing for the market. Of course, they lived partly off the farm, so that, in reality, there were "capitalist" farmers who had large holdings and whose family requirements were only a small fraction of total output, while "peasants" were those whose family consumption accounted for a large percentage of their total output. In short, a cline is indicated, not a categorical distinction that will bear sharp argument about class oppositions. The "breaking" of the farming ladder seems on the evidence presented to have been rather its voluntary dissolution by emigration toward higher incomes in the towns and colonies.

The author states that the northeastern Scottish peasantry "had not died without a fight" (p. 162), but he seems to strain to explain away the fact that the fighting did not follow class lines. He falls back on the assertion that the peasants did not mobilize support for their political interests because they were not smart enough to realize what these were and so let the "capitalist" farmers (mis)represent them. One becomes sceptical when Carter counts hired farm servants as peasants, duped by "capitalist" farmers into acting as their "shock troops."

The book is peppered with antipathies toward economists, rural sociologists, and above all University of Edinburgh historians. These last are accused of virtual conspiracy to present the polarized farmer/laborer society of the East Lothians as applying to all Lowland Scotland. More widely, according to Carter, this model has had the effect of making empirical investigation of regional variation in agricultural history appear unnecessary and has been used to justify conservative agrarian policies as far afield as the Third World. One cannot subscribe to that. British agricultural historians study little else except regional variations. W. W. Rostow lately came nearer the mark in accusing them of hiding behind Stephen Potter's ploy, "it's different in the South." Fifteen or twenty years ago studies of Norfolk and Lincolnshire were overinfluential in English, perhaps British, agricultural history, and I was myself a critic of the resultant anatopisms. The cause, however, was nothing more sinister than the tendency of historians to follow the fashion and a mildly insensitive extrapolation from the known to the unknown. Subsequent revisionism, a sign of agricultural history's maturing, has converted the real problem into the opposite one of how to synthesize.

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FRANÇOIS CARON. *An Economic History of Modern France*. Translated by BARBARA BRAY. (Columbia Economic

History of the Modern World.) New York: Columbia University Press. 1979. Pp. 384. \$16.50.

François Caron divides his economic history of France into two parts: the first deals with the nineteenth century (1815–1914) and the second, and slightly longer, part with the years since 1914. This is the first work in English to give detailed treatment to the course of French economic history from 1914 until the mid-1970s. Other distinguishing features of this work are Caron's accent on growth and his de-emphasis of noneconomic factors, such as social attitudes. His generally positive judgments on the performance of the French economy during the nineteenth century and also during the 1920s are a reaction against the preoccupation of the last generation of Anglo-American scholars with the *mentalité* of the French businessman and with economic retardation. He discounts the view that the rapid economic growth of the 1950s and the 1960s was a sudden rupture with past stagnation, constituting a French "economic miracle." Instead, Caron views this growth as a logical prolongation of the industrial growth of the period 1896–1929, and he therefore calls into question the whole gamut of social and economic explanations that have been employed to explain French stagnation.

This work is less a narrative of events than an analysis of how and why the French economy performed as it did. Caron integrates the results of recent scholarship, including that of the French school of *histoire quantitative*, and succeeds admirably in presenting a balanced view of French economic growth. Avoiding a strict chronological approach, he analyzes separately such topics as population and the working force, productivity, banks, capital formation, foreign trade, agriculture, and industry. Some traditional subjects (for example, public finance) are neglected; there is little on tariff history but more on the balance of payments.

One central theme throughout is the role of the state. Even in the nineteenth century "the neutrality of the state was never anything but a theory" (p. 44) taught by liberal academics. The state's aims were contradictory in that it attempted to promote growth but avoid the social dislocations associated with it, to promote competition but not upset the status quo, and recently to encourage concentration, so French enterprises could compete with foreign multinationals, while favoring small- and medium-sized businesses over giant enterprise. Caron sees the present equilibrium between the public and private sectors as fragile, "and a slight modification strengthening the former would definitely turn France into a bureaucratic system" (p. 365).

In this new—there has been no French edition—and important work, Caron has been ill served by his translator and editor. The translator frequently

stumbles over the highly technical vocabulary. Her renderings of the French are often awkward and unclear and sometimes completely misleading. These, and the numerous errors in spelling and fact, should have been corrected during editing. Despite these defects, this is a welcome publication that deserves to reach a wider audience than its hard-cover format will permit. There are a large number of works on the economic history of modern Britain available in inexpensive editions but not one on France. This is unfortunate because, for those interested in the problems of development and models of growth, as much, if not more, may be learned from the French experience as from that of England.

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PIERRE CAYEZ. *Métiers jacquard et hauts fourneaux aux origines de l'industrie lyonnaise*. Lyon: Presses Universitaires de Lyon. 1978. Pp. 464.

"Jacquard looms and blast furnaces"—it is hard to think of a less revealing title for this history of Lyon industry in the first century of the Industrial Revolution (actually 1780–1860). To be sure, silk weaving and metallurgy figure prominently, representing the traditional and the newer branches of industry respectively. Yet the emphasis has not been on technology, either for the bourgeois of Lyon then or for their current chronicler. Production in Lyon was dominated by commerce, later also by finance, with the silk merchant-manufacturer providing the model. In turn, the present study defines its scope in terms of an economic region, a space over which Lyon capital held sway. Pierre Cayez is far more concerned with decision making and market dominance, with the organizational and financial history of industry, than he is with technological or even purely economic aspects.

In other ways as well this is a somewhat curious book. The reason for its publication in present form is unclear. The volume represents the first half of a characteristically massive *thèse*, shorn of its scholarly apparatus but seemingly not otherwise modified. Some sentences sound almost absurd in the absence of footnotes, so clearly were they written as simple introductions to infrapaginal reviews of literature. A prefatory note informs us that the entire thesis is being reproduced by the University of Lille III; research libraries would gain from holding out for the integral version. Yet this volume will not appeal greatly to a wider audience either. The problem is not so much with the mass of factual material presented as with the insufficient intellectual inducement the reader is offered to work through that mass.

For the most part, this is very traditional industry history, more descriptive than analytical. It is of interest for two reasons. One is the large amount of primary material Cayez presents and has, presumably, unearthed (in the absence of footnotes one cannot be sure). He is cautious and critical regarding his sources and data, but 143 tables and 100 appended pages of charts, graphs, and maps cannot help but inform. The second merit of Cayez's work is that he opens the door on a number of important problems regarding industrialization in an urban region. Among them are the interaction of the urban economy with its regional and wider environment, particularly the extent to which the urban center imposes or fails to impose its own dynamic. Even more timely is the attention focused by the case of Lyon on the somewhat neglected urban aspects of protoindustrialization. Cities served as more than centers of shipping and commerce, foci of chiefly rural development. They and their immediate suburbs housed much production, often directly complementary to that of the surrounding rural areas. Finally, there is the crucial question of the transition from protoindustrial production to modern factory industry. The relations between the traditional and the newer industries, particularly at the level of capital and entrepreneurship, are Cayez's principal concerns.

The problem with the book is that exposition overwhelms analysis. The big questions are raised but only by way of introduction and summary. The data are not marshaled in support of contending hypotheses; rather, they parade by in tidy ranks. The reader is left to figure out for himself to what extent Cayez's research has shaken or reaffirmed accepted interpretations, cleared up or further complicated ongoing controversies. All too often, poor data (and some imprecision in presentation) leave no great sense of loss or disappointment: when no questions are asked, how can one fault the answers?

Yet, if any single city can serve as the microcosm of the French economy in the nineteenth century, surely Lyon is that city. For all its faults, Cayez's work does show us the often painful early steps and missteps in Lyon's evolution as a hinge between north and south in the economic geography of France.

PAUL M. HOHENBERG
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CARL J. EKBERG. *The Failure of Louis XIV's Dutch War*. Chapel Hill: University of North Carolina Press. 1979. Pp. xix, 240. \$20.00.

Although France emerged from the Dutch War as the most feared state in Europe, the war had not gone according to French plans. What had begun

as a quick strike against the Dutch lasted for six years and led to the first European coalition to oppose Louis XIV. To understand more fully what went wrong, Carl J. Ekberg has made a detailed study of the months between the French failure to achieve total military victory over the Dutch in the campaign of 1672 and the withdrawal of French armies from Dutch territory in October 1673. It is a story of military and diplomatic failure and of how a limited war was transformed into a general European conflict. There are chapters dealing with various aspects of those months: the motives and person of the king, the conduct of the war, the failure of the Cologne conference, the widening of the war, and the growing isolation of France. By using memoirs of the period as well as the French war and foreign affairs archives to good effect, Ekberg has described the government's confusion and the king's inability to articulate a policy. Equally convincing is his picture of the king's excessive reliance on Louvois, frequently to the exclusion of his other ministers, and of Louvois's inadequacy once the neatly planned strategy of 1672 fell apart.

Ekberg is unrelenting in his indictment of Louis XIV, whom he sees, at least in these years, as governed by a consuming, highly personal, and puerile thirst for glory that led him to pursue a course of action contrary to the interests of the French state. Urged on by Louvois, Louis's egotism led him to ignore his other advisors who, according to Ekberg, were opposed to the war and its conduct and who advocated moderation in the tradition of Cardinal Mazarin. Stimulating as his assessment is, however, it has some serious problems. As his own evidence indicates, the documents do not support a case for opposition to royal policies within the government, except for Pomponne. Finally, we should not overlook the powerful element of continuity between Louis's reign and the past. However unwise, immoderate, even monomaniacal we find Louis XIV's behavior, his goals—given the greatly enhanced power of France—remained those of Richelieu and Mazarin.

RICHARD PLACE
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ROBERT SOUCY. *Fascist Intellectual: Drieu La Rochelle*. Berkeley and Los Angeles: University of California Press. 1979. Pp. x, 451. \$25.00.

Pierre Drieu La Rochelle was born in 1893 and died by suicide in 1945, having contemplated and attempted it on several earlier occasions. Between 1917 and his death he wrote some thirty books and pamphlets and two (unsuccessful) plays. Most of his novels were heavily autobiographical; two of them,

Reveuse bourgeoisie (1934) and *Gilles* (1939) are illuminating documents—the one on the bourgeois family (his own), the other on the development of a fascist intellectual (himself). I have never enjoyed his writing nor understood the widespread interest he generated in his lifetime and since. Perhaps, as with Faust's vision on the Brocken, many found in him an image of their own failed aspirations. Historians may see him as a convenient kaleidoscope, reflecting a *cursus* common to his generation: cult of Barrès and of self, surrealism; revolt against decadence, fascism (or communism, or a bit of both), suicide (or execution, or exile), and eventual institutionalization, in a variety of combinations. Drieu represents a failure carried to extremes that few of his companions—not Aragon, not Malraux—achieved yet typical enough to warrant generalization.

What could be more typical than this willowy, flanneled Anglomane, this flabby admirer of toughness, who inflated his schoolboy rebellion into intellectual arguments, projected his failings on others, railed at the decadence he brilliantly represented, glorified sports at which he was no good, rejected the materialism his hedonism required, turned his yearning for virility into a cult of what he lacked and his resentments into pranks, pamphlets, and spiteful creeds?

The procrastinating narcissist wanted to be a man of action. Political action was the noblest sport—more satisfying than his avid brothel-creeping. So Drieu magnified his egoism into elitist nationalism, flirted with royalism, coquetted with revolutionism, fell for fascism and clung to it because of (false) *noblesse oblige*, betrayed all his loves beginning with himself. Yet this spoiled child was clear-sighted enough to see his failures—of talent, affection, and commitment. He judged himself a weakling and a coward, knew himself to be as *taré* as the worst bourgeois. He was, Soucy quotes him, preoccupied with the fundamental problems of “action, sex and death” (pp. 231, 351). He found his action in sex, he took death for action. Every other page he wrote reflects the desperate realization that civilization was equivalent to impotence. For him. His hard, gem-like flame burnt down into mediocrity. Unlike many contemporaries, he knew it. At least, he said it. His published and unpublished writings are a continual denunciation of himself. We need not wonder at his destructiveness and his self-destruction.

Robert Soucy's book treats this complex figure with hard-headed sympathy. He gives us the rake's progress of an intellectual philanderer, whose fascism may indeed have been merely “the exasperation of a lazy mind” (p. 273). There may be other intellectual (and psychological) roads to fascism. But, if fascist intellectuals are those who cannot live

up to the world and to themselves, Soucy provides an awful warning for nonelite elitists.

EUGEN WEBER
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JOSÉ MARAVALL. *Dictatorship and Political Dissent: Workers and Students in Franco's Spain*. New York: St. Martin's Press. 1978. Pp. 199. \$22.00.

Sociology in general, and especially political sociology, is a subject likely to encounter a multitude of obstacles within the repressive framework of a dictatorship. In fact, as far as Spain is concerned, it would not be preposterous to affirm that the knowledge yielded by the analysis of workers' and students' protests could be paralleled—admittedly in a more limited and elitist sphere—by a study of the research and writings of the generation of post-Civil War social scientists. Among them, José Maravall stands out in the specific fields of working-class problems and labor relations. His long dedication to these questions guarantees the value of his new contribution.

The book has as its central issue the why and how of political dissent under a capitalist dictatorship. Its case study is Franco's Spain, the dissenting voices those of the workers and the university students. The answer is partly to be found in the internal contradictions of a dictatorial system that, having started in the forties with a blend of political autocracy and economic autarchy, was later forced to accept more liberal and open policies to achieve growth and development. But, at the same time, it tried to preserve its authoritarian institutions. The tensions thus generated set the stage for the dissent of workers and students.

Dissent was partly determined by the dictatorship, but it also had its own causes and forces. Franco's regime was a kind of historical parenthesis between the Second Republic and the newly born democracy. Maravall searches for the connections between the protest of workers and students under Franco and the ideologies and organizations of the Republican period. He finds lines of continuity at all levels: professions, geographical areas, communities, even families—at least as far as the working class is concerned, for here group coherence and continuity is much greater than in the short experience of university life. But dissent in both worlds is closely related to the activities of left-wing political parties—PCE (Communists) and PSOE (Socialists) in particular. This leads to the conclusion that the politicization of dissent under a dictatorship is almost inevitable. Sometimes it will be subversive and clandestine, at other times it will infiltrate official

structures. Both strategies were tried in Spain without conclusive evidence in favor of either.

The lack of freedom under a dictatorship makes the study of any form of opposition rather difficult. Sources are always limited. Maravall adds to those easily available—the press, Ministry of Labor reports—some less well known—documents of illegal trade and students unions—and some totally original—personal interviews with workers' and students' leaders. The conclusions he reaches must be taken as indicative rather than definitive. There is a great deal of subjectivity in the supporting evidence of some protagonists. Maravall poses a lot of challenging questions and answers some of them. The book as a whole throws much light on some obscure areas of Franco's Spain. The possibility of extending the conclusions to other situations may, however, be limited given the exceptional nature of Franco's regime.

JOSÉ AMODIA
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JAMES D. TRACY. *The Politics of Erasmus: A Pacifist Intellectual and His Political Milieu*. Toronto: University of Toronto Press. 1978. Pp. 216. \$15.00.

No more than Shakespeare, Erasmus was a forgotten man during the centuries after his death. Although he lacked the advantage enjoyed by Luther and Calvin of having church historians dedicated to perpetuating his memory, he has always commanded the attention of scholars. The stream of publications rose enormously during the quinquicentennial celebration of his birth and has flowed on unabated, with more yet to come, as the magnificent new editions of his works progress in the Netherlands (in Latin) and in Toronto (in English). One of the most learned of Erasmus scholars is James D. Tracy, whose first book, *Erasmus: The Growth of a Mind*, provided a full chronological account of Erasmus's intellectual development. In his present volume Tracy focuses upon the realistic dimension of Erasmus's political thought.

Nearly all previous studies of Erasmus's political thought have worked with his treatises such as the *Panegyricus*, *Querela pacis*, and the *Institutio principis christiani*, they have dealt with perennial issues in political philosophy and have emphasized his moralism in contrast to the realism of contemporaries such as Machiavelli or even More. Like Preserved Smith, whose *Key to the Colloquies* sought to unlock the secret of the accretions and alterations of the text by identifying Erasmus's life circumstances at the time, Tracy explores correspondence and other documents to assess Erasmus's interest in and knowledge of actual military and political develop-

ments in the Netherlands as well as of diplomacy in high places in which influential friends of his were involved. A few examples of the concreteness added to the picture of Erasmus's thought by this detailed approach will be suggestive. In the *Querela pacis* Erasmus described mercenaries as the "vile excrement of criminality holding life less dear than a small bit of profit." Erasmus had a personal encounter with the notorious Black Band that explains the rancor he expressed. Erasmus's strong prejudices against the Habsburgs led him to blame them unfairly for many ills and his interpretation of the Guelders war was wildly wrong. But in both of these cases of bad political judgment Erasmus was reflecting views that had wide currency among his countrymen. Erasmus's observations on fiscal practices and political and military events are sufficiently accurate, when checked against other contemporary sources, to suggest that his letters are probably trustworthy even when there is no corroborating evidence. He saw quite clearly that the power of the great nobles and territorial princes was growing and that, as he wrote to Spalatin, "the remnants of ancient democracy" were passing from view.

Tracy concludes that Erasmus was well informed in politics and that he saw the state not just as a welfare institution but as a power that should defend the people, by force if necessary. He further shows that Erasmus was not just a cosmopolitan intellectual ("Where you fare well, there is your fatherland") but was for a time allied with the "national" party supportive of a specific *patria* and that he was not a mere moralist blind to power and hence unable to grasp the mainspring of relations between states. One wonders how the author justifies calling Erasmus a pacifist, given his recognition of the necessity of power and arms, if pacifism means opposition to war or to the use of military force for any purpose. This volume takes its place with other excellent recent Erasmus studies such as those of Marjorie O'Rourke Boyle, Franz Bierlaire, and Georges Chantraine.

LEWIS W. SPITZ
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HERBERT H. ROWEN. *John de Witt, Grand Pensionary of Holland, 1625-1672*. Princeton: Princeton University Press. 1978. Pp. xiii, 949. \$40.00.

John De Witt has not been the subject of a full-scale biography since the 1880s: James Geddes's *History of the Administration of John de Witt* appeared in 1880 and an English translation of Antonin Lefèvre-Pontalis's *John de Witt* in 1885. Perhaps both of these works were prompted by the assurance,

proclaimed in 1872 by the distinguished Dutch statesman and historian J. R. Thorbecke, that anyone who gave us "a life of De Witt worthy of the man" would earn himself "a place among historians of all time" (p. vii). Geddes, however, never published more than his first volume. He may have been discouraged by what Robert Fruin said in his review: "To write a history of De Witt's times which satisfies the requirements of historical art as well as those of historical science is a very heavy task, especially for a foreigner" (p. vii).

How well has Herbert H. Rowen acquitted himself of this "very heavy task?" Let us first consider "the requirements of historical science." As councilor pensionary of Holland (1653-72), De Witt directed the foreign policy of the United Provinces; so his biographer must be a master of their relations with the France of Louis XIV and the England of Cromwell and of Charles II. De Witt was more than a modern "secretary of state," for there was no *stadholder*, president, or other monarch for him to report to; he was responsible to two collective masters: the States of Holland and the States General of the United Provinces. But the man who handled their incoming and outgoing correspondence also recommended policy, not only foreign but also financial, military, and naval policy and much else. In theory responsible to the States, in practice he was their manager. Rowen knows how the Dutch Republic worked, and he shows how De Witt gained control of the various aspects of government in chapters entitled "The Craft of Politics," "The Master of Patronage," "The Manager of State Finances," and "Diplomacy: Craft and Art." Readers may feel that the exposition of the Dutch constitutional system is not as clear as they would like it to be, but Rowen would reply that the unclarity is not in the eye of the expositor but in the system itself, as anyone who has tried to describe it knows.

Rowen has gone through the letters and papers of De Witt, and of the States of Holland and of the States General; the narrative rests upon the sources, and, indeed, it sometimes seems that nothing has been left out. There are 890 pages of text in 41 chapters. The list of printed sources runs to nine pages, not counting pamphlets and newspapers, which account for eight more. Then there are fifteen pages listing articles and books bearing on the subject. The requirements of historical science have been fulfilled.

What about the "requirements of historical art?" Rowen has the talent to write a charming chapter on life in The Hague, a moving chapter on De Witt's relations with his wife and children, a gripping account of the events leading up to his lynching in 1672. Art, too, has gone into the narrative of De Witt's role in naval strategy against England

and in military strategy against the invasion of Louis XIV. For Rowen, this has been a labor of love, and those who read the 890 pages will have gained a truer and more complete picture of De Witt than has ever before been presented.

One may nevertheless complain that it is not easy to find things in this book; it is indexed for names but not for subjects (of what use is a half column of references to the States General, with no sub-headings?). There are no maps, even to illustrate the descriptions of naval and military battles. There are no reproductions of portrait or bust, though these are described in detail. Perhaps the press is to blame for some of these curious omissions. This reader's principal suggestion, though, is that serious consideration be given to bringing out a book of one-third the length. Having set forth the record here with a Netherlandish love of detail, Rowen should now feel free, like Rembrandt, to paint a portrait that highlights the main features of his important subject.

GORDON GRIFFITHS
University of Washington

ANDRÉ CORDEWIENER. *Organisations politiques et milieux de presse en régime censitaire: L'Expérience liégeoise de 1830 à 1848*. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège, number 220.) Paris: Société d'Édition "Les Belles Lettres." 1978. Pp. 504, 19. fr. 150.

This remarkable regional study of Liège is concerned with synthesizing materials about "creative politics" in the initial years of Belgian independence as well as the formative role of the local press. André Cordewiener has not only extended the writings of P. Harsin and R. Demoulin on the instrumental part played by this industrial region in national politics, he has also documented more fully the causes for the breakdown of *unionisme* (and political romanticism) as the *liégeois* liberals and Catholics moved from cooperation to conflict. In a major historiographical sense, he has supplemented the efforts of H. Haag, A. Simon, and C. Lebas on political groups on the national level with a local study that is rich and exhaustive in its detail and documentation. Along with E. Witte in his seminal work *Politieke machtsstrijd in en om de voornaamste Belgische steden, 1830-1840* (1973), Cordewiener depicts and analyzes the bitter and prolonged postrevolution origins of the church-state issue by concentrating on the development and use of the press by the two major political organizations.

The work is more extensive and helpful on the liberal political groupings and opinions, especially their diversity, their increasing anticlericalism, and

the central force of freemasonry both *in* and *outside* their political organizations and the press. The regrouping of political forces is portrayed, with emphasis on a tight analysis of the composition, degree of cohesion, and points of discord of the liberals. This "party" factionalism and reconstruction in the forties demonstrates the importance of the schism between the "doctrinaire" and "progressive" variants of liberalism and its resolution. It furthermore allows the author to explain how and why moderation and caution won over the democratic ambitions of the more social "radicals and Progressives" in the intramural battle of liberals and how and why the reunited neoliberals chose the path of warfare against the Catholics and the church. Skillfully and sharply, the author distinguishes between politics centering on elections and their issues (which involved only a minority of participants because of the highly restrictive suffrage) and politics reflected by the press (which in these economic boom times had not only wide currency and appeal but also extensive influence). Cordewiener insists that both the division and redefinition of liberal positions in the forties was largely carried out in the *Journal de Liège*, *Liberal liégeoise*, *L'Espoir*, and *L'Industrie*, a process that, significantly, was not duplicated in the defensive Catholic press. His landmark history of this domestic political rearrangement depends upon a close study of the internal realignment of the Orangists, the episcopates' recondemnation of the freemasons, and the 1842-45 debates over education and freedom of the press.

One might question whether Cordewiener has proved that Liège moved Belgium in the direction of materialist, pragmatic but nonradical brand of liberalism. His work, however, illustrates that the demise of mixed ministries and coalition governments resulted in large part from left-wing liberal notions about separating state from church. The ecclesiastical control of education, as perceived by the Walloon bourgeoisie of the eastern industrial regions, was one major area of needed reform in addition to the abolition of the stamp duty on papers and more public assistance for new industries. The author also points to the diverse liberal newspapers as the "motor force" in disseminating these new ideas and promulgating the issues that resulted in not just a local victory but also national power for the liberals in 1846-48. Finally, the author has illuminated the crucial relationship of the press to embryonic parties in a very specific socioeconomic context and at a particular time when attracting broader support and building a definitive political program became a necessity.

This impressive scholarly work is not only based on a depth and range of documentary evidence that includes brochures, thirty newspapers, and state, lo-

cal, and private papers but also contains several significant appendixes of party and press persons of note.

PIERRE-HENRI LAURENT
Tufts University

T. K. DERRY. *A History of Scandinavia: Norway, Sweden, Denmark, Finland and Iceland*. Minneapolis: University of Minnesota Press, in association with the American-Scandinavian Foundation. 1979. Pp. x, 447. \$19.75.

Those who feel the area we know as Scandinavia should be treated in a regional as well as in a national context have long awaited a good overview of the history of the five Nordic countries. They will welcome T. K. Derry's well-balanced treatment of the history of Scandinavian society from the pre-Viking period down to the Swedish electorate's (at least temporary) rejection of Social Democratic government in 1976. Devoting generous attention to cultural, economic, and social aspects of Scandinavian developments, Derry has admirably avoided reciting the details of dynastic and military entanglements that could easily be the death of an attempt at comparative history spanning such a long period and so many nation-states. Commendable, too, given the wealth of literature in English on the Viking era, is the author's concentration on the period since the late Middle Ages and the appearance of the nation-state in the north.

It is no easy task to weave the fabric of a "history of Scandinavia" from the research and writing of Nordic historians. Historians in all five countries dwell to a surprising and anachronistic extent upon their respective national societies (although these efforts are often methodologically or theoretically sophisticated), and broad comparative efforts have been limited to an abortive study of abandoned farms in the late Middle Ages, a recent study of Scandinavian responses to the Great Depression and a current study of the relationship between central governments and local communities in the five countries during the mid-eighteenth century. For this reason, Derry's constant efforts to place Scandinavia—taken as a whole—into the broader framework of European society are most refreshing. This may be illustrated by his consideration of the collective economic output of the twenty-two million Scandinavians alive in 1973 with that of other Western societies, including mention of grain production nearly equaling that of Great Britain and of a merchant marine six million tons larger than that of the British and more than twice the size of the American. Yet Derry is prudently aware that he must treat the region as one composed of five sepa-

rate societies, although they are linked together in interesting ways by the Nordic Council and related institutions.

The book is burdened with few inaccuracies, although the Finns may be irritated by the fact that the Finnish parliament, the Eduskunta, is consistently referred to as the "Elskunta," and readers referring to the end maps will despair at finding Sigtuna miles from Lake Mälaren and Elsinore dismayingly distant from its strategic location on the sound within sight of the Swedish town of Helsingborg. Without question, however, T. K. Derry's history of Scandinavia provides the student, the generalist, and the layman with a reliable, up-to-date, and highly readable introduction to the history of a region all too often ignored in Anglo-American treatments of early modern and, especially, modern Europe.

MICHAEL F. METCALF
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KEKKE STADIN. *Småstäder, småborgare och stora samhällsförändringar: Borgarnas sociala struktur i Arboga, Enköping och Västervik under perioden efter 1680* [Small Towns, the Petite Bourgeoisie, and the Great Social Transformation: Bourgeois Social Structure in Arboga, Enköping, and Västervik during the Period after 1680]. Summary in English. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensis, number 105.) Uppsala: Uppsala University; distributed by Almqvist and Wiksell International, Stockholm. 1979. Pp. 180. 66.50 KR.

Most social and economic research concerning late seventeenth-century Sweden has dealt with prominent and very prosperous elites. Kekke Stadin, however, has added significantly to our knowledge of this period through his study of the businessmen in three medium-sized towns from 1680 to 1715. The author examines this group to see if they experienced the same social and economic transformations then beginning in larger Swedish and European commercial centers. He looks particularly for evidence of capital accumulation and investment and for changes in the petite bourgeoisie's occupational structure and political and social influence.

Stadin chose his three towns on the basis of differing economic functions. Arboga was a commercial intermediary between the Bergslagen mine owners and Stockholm exporters. Enköping's economy was based upon the sale of foodstuffs to Stockholm. Västervik, unlike the others, sold its wood products, tar, and iron abroad, as well as to Stockholm. From his study, Stadin shows that an economic elite, smaller in number and more prosper-

ous than the upper ranks of the bourgeoisie had been before, began to emerge in the three towns. These merchants also tended to be established in the most progressive sectors of the towns' economies and to possess a greater number of employees than had been common previously.

Success was associated to a significant extent with the ability to adapt to new economic circumstances. Arboga's commerce declined severely when Bergslagen mine owners established direct commercial ties with Stockholm. Only small foundry owners were able to prosper because of the demand for their services fostered particularly by the Great Northern War. Västervik wholesalers were adversely affected by the establishment of a tar monopoly in Stockholm but adjusted by investing their capital in ship construction and ownership. When Enköping's fishing industry died out, the town's commercial farmers acquired even greater importance than they had possessed before. Stadin also shows that this emerging elite tended to secure a tighter grip upon the elective offices of local government and to strive toward greater social exclusiveness through restrictive marriage patterns.

Stadin presents an informative examination of the social and economic transformation of the petite bourgeoisie in these Swedish towns and establishes a pattern that he argues was true for other towns as well. The author uses his limited primary sources well and is obviously acquainted with the pertinent secondary materials in his field. Stadin establishes a reasonable definition of the group he studies without becoming entrapped in an excessive discourse on dogma and terminology. The book possesses a sufficient number of clear and explanatory tables and diagrams. It is blessed with a nine-page English summary that presents the essence of his project very well with only a handful of minor misspellings.

LELAND B. SATHER
Weber State College

ÅKE LINDSTRÖM. *Bruksarbetarfackföreningar: Metalls avdelningar vid bruken i östra Västmanlands län före 1911* [Swedish Ironworkers' Trade Unions: The Branches of the Swedish Engineering Union at the Ironworks in Eastern Västmanland before 1911]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensis, number 106.) Uppsala: Uppsala University; distributed by Almqvist and Wiksell International, Stockholm. 1979. Pp. 146. 58 KR.

The Swedes have achieved significant team-work in historical training and research. The well-known emigration project and the study of the family at Uppsala and World War II research at Stockholm have attracted twenty or more students each and

have provided group financial support. The team approach animates a theoretical framework within which individual researchers may investigate their segments of the subject and enjoy cooperation with other scholars. Unlike the seminar, this group activity usually extends over several years. The present study is the fourteenth publication within the project "The Functions of Swedish Class Society: Popular Movements," others being concerned with topics such as rural workers in Uppland, sports as a folk movement, political activities of popular movements in Gävle in the 1880s, strike-breaking and the freedom of labor, and conflicts within the Communist Party of Sweden. A variety of monographic material is thus handled within a program that has some cohesiveness of concept and methodology.

Åke Lindström's dissertation shows how labor's attitude toward unionization developed during the critical period 1900-10 in the rural ironworks of Västmanland in central Sweden, with special attention to Skultuna. Centuries-old patriarchal ironworks (*bruk*) had acquired a close-knit social and economic structure, a vertical solidarity embracing owner-manager, artisans, helpers, and families. Toward the end of the nineteenth century, new machinery, increased production, hiring of more workers from outside, and spreading labor consciousness led to changing attitudes of both owners and workers. Change was slower in rural *bruk* than in cities, as confirmed in related studies in this series. Change came most slowly in the smaller *bruk*, where relationships of owner and worker were more intimate. In eight of the smallest *bruk* in Västmanland no unions were established by 1910. But many *bruk* were becoming consolidated, so that nationally the number declined from 381 to 140 (1870-1913). Frequently the patriarch-owner was displaced by a newcomer superintendent (*disponent*). Gradually, a horizontal solidarity of workers replaced the older vertical solidarity of the *bruk*.

Differences are pointed out between classes of workers: for example, the smiths formed a self-conscious elite among the ironworkers, and because of pride and privilege they were most hesitant to join the union of metalworkers; they clung to older methods of work and reacted against innovative techniques as well as against innovative trade associations. Unionization came largely through quiet personal persuasion without mass meetings or confrontation. And superintendents reacted with restraint—a few firings, some thwarted attempts to charge rent for previously free housing, and little more. A new day had arrived and was accepted. In 1900 only 6 percent of Västmanland's ironworkers were organized; in 1910 the figure was 54 percent.

The eight-page English summary is excellent, the 112-page Swedish text is well organized, although

unnecessarily repetitious. Here in a microcosm of time and place is a valuable account of the Swedish shift from patriarchy to unionism.

FRANKLIN D. SCOTT
Honold Library,
Claremont

ROBERT SCHWEITZER. *Autonomie und Autokratie: Die Stellung des Grossfürstentums Finnland im russischen Reich in der zweiten Hälfte des 19. Jahrhunderts (1863-1899)*. (Marburger Abhandlungen zur Geschichte und Kultur Osteuropas, number 19.) Giessen: Wilhelm Schmitz Verlag. 1978. Pp. vii, 395, DM 80.

If one of the most important functions of the historian is to call into question long-held, comfortable assumptions and to reveal the complexities of persons, events, and institutions, the author of the present volume deserves unstinted praise for his remarkable erudition and analytical skill. Using a wealth of archival materials in the Soviet Union and Finland and scholarly and not-so-scholarly studies in six languages, Robert Schweitzer has produced a work of first-rate importance for understanding the relationship between tsarist Russia and its non-Russian subjects.

The subject of attempted Russification in the western borderlands from Alexander II to the First World War has been the subject of plausible generalizations for many years. To test such generalizations the author uses as a case study Finland, which, as a Grand Duchy, had achieved by the late nineteenth century by far the highest level of self-government of any of the Romanov dominions. He has explored some of these generalizations with a thoroughness that even the ablest Finnish scholars have not equaled and has attacked some assumptions with considerable success.

It has been customary, for instance, to attribute the tougher policy of St. Petersburg toward Finland in the last two decades of the nineteenth century to the increasingly reactionary atmosphere of the imperial government and the fear that a liberal infection might spread from the Grand Duchy. The author disputes this interpretation: "Finland's autonomy was not directly dependent upon the degree of liberalism in Russian internal policy, and the Finnish constitution was not such a tempting example for the Russian opposition that Alexander III would have had to liquidate it for the safety of the autocracy. The change of rulers brought, it is true, a personal shift in policy toward Finland, but no new basic program" (p. 9). Indeed, as Schweitzer illustrates by numerous examples, Alexander III was not consistently, or even basically, a foe of the Finnish constitutional system. Insofar as it was con-

venient, he was willing to let sleeping dogs lie and to disregard autocratic dogma with respect to the Grand Duchy. Circumstances, however, forced a confrontation that the tsar would have preferred to avoid.

The nature and scope of Finland's "constitution" in the nineteenth century has always been wrapped in ambiguity. As one minister state secretary for Finland put it in the middle of the century, "The Finnish constitution is like the illicit relationship of a married man. Everyone knows about it; everyone accepts it; but the less it is talked about the more happily the partners can get along together." Schweitzer does not cite this dictum, but he probably would agree with it. One of the most striking facts to emerge from these pages is the incessant talking about the Russo-Finnish relationship that went on from the mid-nineteenth century onward, as well as the growing acerbity of the dialogue. As long as Finland's position remained hazy and Russian policy toward Finland was determined by quiet personal contacts in St. Petersburg, things went reasonably well. For various reasons, however—including pressure from Russian jurists and military men—the imperial government found it necessary in the last quarter of the century to reach a more exact formulation of the Grand Duchy's position. "From the political point of view," the author writes, "Finland's status had never had any other basis than the will of the tsar. Alexander III, however, was the first to regard himself as compelled to determine this juridically" (p. 288). Out of this apparent necessity arose conflicts of opinion that eventually became irreconcilable.

The author traces exhaustively—even exhaustingly—the complexities of the Finnish problem from the mid-nineteenth century to the eve of the February Manifesto of 1899, which inaugurated open resistance on the part of Finns. In this detailed analysis no one, it seems clear, could possibly have done a better job than Schweitzer has. It would seem churlish, then, to complain of omissions in this remarkable exegesis. One should note, however, that Schweitzer's study remains deliberately on the theoretical, juridical, and political level. It is a little like some old-fashioned diplomatic history in that it describes the claims, the negotiations, and the recriminations but does not consider some of the pressures on both sides that pushed the participants into their theoretical positions. One of these pressures was the concern of Russian military men about the vulnerability of St. Petersburg; unsure of Finnish loyalty, they were determined that Finland should not become the springboard for an attack on the capital. Another was the slow but sweeping transformation taking place in Finnish society, which, among other things, increased the self-assertiveness of the Finnish elites and, simultaneously, their fear

that an erosion of their constitutional status might lead to a loss of their social privileges.

Such considerations, however, are for another book. Within the limits he has set for himself, Schweitzer has produced a work than can truly be called definitive.

C. LEONARD LUNDIN
Indiana University,
Bloomington

HEIKKI RANTATUPA. *Elintarvikehuolto ja -säännöstely Suomessa vuosina, 1914–1921* [The Provisioning and Rationing of Food in Finland, 1914–21]. Summary in French. (*Studia Historica Jyväskyläensia*, number 17). Jyväskylä: Jyväskylän Yliopisto. 1979. Pp. 276.

The circumstances surrounding Finland's declaration of independence in 1917 and the brief intense civil war of 1918 are gradually acquiring historical documentation. To cite only a few examples, there are the studies of the international diplomatic context by Yrjö Nurmio and Tuomo Polvinen, the research by Jaakko Paavolainen on Red and White terrorist atrocities in 1918 and Vilho Rasila's attempt to correlate socioeconomic variables with political behavior at the village level. What scholars such as these have contributed, beyond their data, is a conceptual framework facilitating the examination of highly charged events in a relatively objective comparative framework.

Heikki Rantatupa's doctoral dissertation is also based on this approach. His research question is straightforward: How did Finland's governmental authorities attempt to provide food for Finnish civilians from 1914 to 1921, and how did their plans work out in practice? The core of his data comes from the surviving archives of successive central food administration agencies, including Oskari Tokoi's socialist commissariat during the spring of 1918. His methodology is descriptive, though incorporating statistics wherever the uneven records permit such treatment. His results account for variations both over time and in geographical regions, often illustrated by maps.

As of 1914, Finland imported 60 percent of its food grain supplies, mainly from Russia, to feed its 3.3 million people. Through 1916 Russian supplies were available and only price controls were applied in Finland. From early 1917 Russian imports dwindled, and from the summer of 1917 to the end of 1918 Finns lived almost entirely on domestic food stocks, with minimal supplements from Russia and later from Germany. Food rationing and compulsory crop sales to government agencies were legislated in the spring of 1917 and remained in force until the spring of 1920.

Under the consumption norms set in 1917, farm-

ing areas in south and west Finland were producing a surplus, and the task of central authorities was to transfer this surplus to two deficit groups, the 30 percent of the population living in urban areas and the residents of rural eastern and northern Finnish provinces not normally self-sufficient, especially in grains. The cities had first access to whatever imports could be secured, but in the countryside the central authorities were unable to bring about any substantial redistribution of food supplies. In fact, though not in law, food in rural areas was controlled by local governments with local needs in mind, whether it was protecting a surplus or seeking to make up a deficit. The most acute periods of malnutrition came in mid-1918, after the civil war and before the autumn harvest. Generally, the food policies of Red and White governments did not differ sharply, save that military needs were given higher official priority on the White side, and the Reds had larger urban populations to feed. Only access to American grain supplies after January 1919 finally lifted the threat of starvation.

Rantatupa does not directly assess the broader significance of food supply policies for other events during these years, yet some implications are obvious. No central agency in Finland possessed administrative tools strong enough to force local governments to comply with national policy. This local nature of actual food management, especially in the countryside, needs to be appreciated in estimating the role of food shortages in precipitating the civil war. At the level of international diplomacy the desperate need for food imports from Russia, Germany, and the Allies has already been shown to be important. Rantatupa's careful account only underlines this.

The French summary is useful not only for those unable to read Finnish but also for others who may lose the thread of argument from time to time in the details of the original text. Indexes of personal and place names are included.

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ULRICH MARWEDEL. *Carl von Clausewitz: Persönlichkeit und Wirkungsgeschichte seines Werkes bis 1918*. (Militär-geschichtlichen Studien, number 25.) Boppard a. R.: Harald Boldt Verlag. 1978. Pp. viii, 296.

This book is primarily valuable for demonstrating conclusively several points so familiar to modern Clausewitz scholars that no one has bothered to discuss them in detail. It title promises more than the author delivers. Clausewitz's influence outside of Germany is discussed in a twenty-five page appendix—an arguable distortion of emphasis. The bulk of Ulrich Marwedel's book considers the evaluation

of Clausewitz's career and work by German contemporaries and successors.

Within these limits Marwedel provides a useful summary of Clausewitz's professional and intellectual activity. It compensates in brevity and readability for a certain lack of depth; *Carl von Clausewitz* does not compare to Peter Paret's *Clausewitz and the State* (1976) in its analysis of Clausewitz's thought. Marwedel's strength instead lies in his treatment of Clausewitz's reception. Particularly after temporarily entering Russian service, Clausewitz acquired an enduring reputation for unreliability. His contributions to Prussia's military reforms were overlooked. He never received assignments that might have established him as a major field commander. Instead, almost by default, he emerged as Prussia's and Germany's great theorist of war. Unfortunately, Clausewitz's intellectual heritage was more praised than comprehended. Marwedel correctly describes this as reflecting a general failure to understand and work through the dialectical method essential to Clausewitz's thought. The men of a realistic, practical age rejected philosophic underpinnings and instead sought concrete rules of behavior. For them Clausewitz became a source of formulas and precepts, a recipe book for victory—if only one could understand the recipes.

The result of this approach was the scrambling or distorting of almost every one of the master's essential points. Even Clausewitz's stressing of moral and psychological factors led to an emphasis on cultivating inherited traits of character, as opposed to developing the intellect and the personality. Absolute war and the battle of annihilation became shibboleths; arguments for limiting and controlling war were ignored or explained away. Even on a practical level Clausewitz's epigoni so eagerly absorbed his criticisms of defensive warfare that they ignored the possible impact of an improving technology on the conduct of operations. The Wars of Liberation became a military model, an endless source of first principles. The result was a mind-set equally hostile to history and the dialectic. Clausewitz, Marwedel argues convincingly, was not an inspiration for German theories of war. He became instead a source of quotations for preconceptions—a pattern shaken, but not destroyed, by the events of World War I.

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WARREN B. MORRIS, JR. *The Road To Olmütz: The Career of Joseph Maria von Radowitz*. New York: Revisionist Press. 1976. Pp. v, 185. \$44.95.

This political biography of Joseph Maria von Radowitz provides a brief overview of the career of one of the more remarkable German statesmen of the

nineteenth century. Although it is based primarily on published sources and adds little to what is already known about Radowitz, it helps fill a void in the literature by giving the English-speaking reader a scholarly introduction to the life and work of the man who was the principal author of the Prussian Union Project of 1849–50.

A devout Roman Catholic whose family had moved from Hungary to Saxony in the eighteenth century, Radowitz had a career that began with a position in the military academy of Electoral Hesse and ended with a brief but significant tenure as foreign minister of Prussia. He first distinguished himself as an artillery expert and became chief of the Prussian general staff of the artillery at the age of thirty-three. Subsequently, he was the Prussian representative to the military commission of the German Confederation, Prussian minister to Baden, and leader of the conservative faction in the Frankfurt Parliament in 1848. A moderate conservative, Radowitz's political goal was to substitute a tight federation of the German states under Prussian domination for the Austrian-led Confederation of 1815.

The author provides some sound judgments on Radowitz's career. His rapid rise in the Prussian service is properly attributed to luck, unusually good connections (especially his friendship with Frederick William IV), and a capacity for hard work. His failure is explained as the result of his inability as an outsider to gain support within Prussia and his difficulty as a moderate in attracting either the liberals or the conservatives in Germany to his program. But, while the book is certainly adequate on Radowitz, it fails to provide much depth or perspective on the German question. The author's point of view is one of uncritical admiration for Radowitz, and, like the *kleindeutsch* historians of the past, he assumes that unification under Prussia was the best solution to the German problem. He gives Radowitz proper credit as an unheralded forerunner of the German Empire, but he fails to give him his due as a militarist, German chauvinist, and anti-Semite—the darker side of that tradition. While well organized and written in a clear style, the book has numerous minor errors, such as confusing the Austrian statesmen Friedrich and Leo Thun-Hohenstein.

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REINHARD SPREE. *Wachstumstrends und Konjunkturzyklen in der deutschen Wirtschaft von 1820 bis 1913: Quantitativer Rahmen für eine Konjunkturgeschichte des 19. Jahrhunderts*. Assisted by MICHAEL TYBUS. Göttingen:

Vandenhoeck and Ruprecht. 1978. Pp. 215. DM 26.

Reinhard Spree's book is a major contribution to German economic history. And, since recent political historiography has often drawn on business fluctuations for explanatory background Spree's findings will surely attract the curiosity of this wider audience as well.

The author wants to construct a quantitative framework for a history of business or growth cycles in nineteenth-century Germany. He uses eighteen time series with yearly data from 1820 to 1913, including variables on population growth, bankruptcies, the capital market, agricultural and industrial prices and production, and industrial productivity, profits, and investment, most of which bear on key industrial sectors like cotton, coal, and iron. In analyzing these time series both traditional and new methods are applied: Spree has chosen polynomial functions of the second to the fourth degree to fit the optimal time trend of each time series, with the residuals taken to detect peak and trough years. Their clustering in certain three-year periods allows Spree to identify business cycles of the economy as a whole. In order to solve the same problem through a different approach, an (American) National Bureau of Economic Research-type diffusion index is constructed, with each indication given the same weight. In a separate chapter Michael Tybus presents a spectral analysis of the variables. Thirteen out of the eighteen indicators reveal Juglar cycles between seven and nine years, some also show Kuznets (sixteen to twenty-five years) and Kitchin (three to five years) cycles, whereas Spiethoff's and Schumpeter's findings of long swings, *Wechselspannen* or Kondratieffs respectively, are rejected. Spree admits, however, that the existence of the "great depression" cannot be ruled out altogether, since years of expansion only narrowly outnumbered those of contraction. Although the author denies long swings the quality of a sound (empirically verified and theoretically deduced) concept, he nevertheless insinuates different stages in German economic growth: early industrialization, take-off (like Rostow from 1850 to the early 1870s), and high industrialization. These growth phases are often referred to when explaining structural breaks in the cyclical behavior of the variables.

Spree also tries to determine causal relationships between the variables by correlating the residuals with different lags. There is no indication of a correlation between population growth and the business cycle, for example, nor is there any evidence of a relationship between agricultural cycles and those in nonagricultural output. As Spree himself suggests, it might complement his study to analyze in

greater detail cyclical fluctuations within certain sectors and their relevance to the economy as a whole as well as to scrutinize the degree of market integration, especially before the 1850s.

In order to apply aggregate indicators, the regional business fluctuations ought to bear sufficient congruity. But during half of the period in question "Germany" was not a political entity and was just on its way to becoming an economically integrated state. On this ground two indicators may be questioned. In the first place, Scottish pig iron prices reflected British rather than German business cycles. Thus, Spree cannot trace any response of German iron production to the movements of these world market prices until well into the 1840s (pp. 132 ff.). In the second place, this independence was due to the structural change in German pig iron production (an indicator also used)—the transition from charcoal to coke pig iron. An aggregate measurement, however, veils the differences in the demand-supply conditions for these two different goods produced in different regions. Hence, these indicators are hardly feasible for tracing overall German business fluctuations before 1850.

It is a pity that in this book Spree refrains from tying himself down to a clear-cut, peak-to-peak and trough-to-trough dating of the detected cycles. On this the reader has to see Spree's recent article in *Geschichte und Gesellschaft* 5 [1979]: 237. But, in spite of these minor objections, Spree's book marks an important step toward a better understanding of nineteenth-century German economic growth and its fluctuations.

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WOLFGANG HOTH. *Die Industrialisierung einer rheinischen Gewerbestadt:—Dargestellt am Beispiel Wuppertal*. (Schriften zur Rheinisch-Westfälischen Wirtschaftsgeschichte, number 28). Cologne: Rheinisch-Westfälischen Wirtschaftsarchiv zu Köln. 1975. Pp. 283.

This book is a useful addition to the still-growing literature on German industrialization. Like much of that literature it is a local study, focusing on the industrialization of the Wupper valley in the Rhineland and deals mainly with the nineteenth century. Its usefulness derives from the importance of its subject and the well-organized presentation of much information about that subject. The Wupper valley—the *Wuppertal*—is historically important as one of the earliest centers of industrial growth in Germany. It had already achieved considerable renown as a center of textile exports by the beginning of the nineteenth century, and its continued growth

had spin-off effects elsewhere in Germany, especially in the neighboring Ruhr area. It is not too far-fetched to see the Ruhr's development in part as a function of demands (for coal, for example) and capital and entrepreneurial skills originating in the more advanced *Wuppertal*. As Wolfgang Hoth reminds us, this was a good-sized region: for example, the twin cities of Elberfeld and Barmen, which comprised most of the region, had in 1858 a population of around 100,000, in 1880 of 190,000, and in 1900 of roughly 300,000 (making it at that last date the eighth largest urban center in Germany). Its development, therefore, was of some consequence and deserves more attention in discussions of German industrialization than it has generally received (Wolfgang Köllmann's fine book on Barmen, *Sozialgeschichte der Stadt Barmen*, scarcely mentioned Elberfeld).

Readers interested in the *Wuppertal* as a case study in industrialization will be grateful to Hoth for his well-ordered presentation of relevant facts. The book has two parts. Part 1 discusses prerequisites or essential factors of industrialization such as transportation facilities, supplies of labor and capital, entrepreneurial leadership, and so on. Hoth goes down his list, identifying times and/or instances in which a particular factor became scarce and/or critical and then discusses the Wupper response. Part 2 then presents a more chronological narrative of *Wuppertal* industrialization. This section offers a 5-stage periodization based mainly on the criteria, degree of mechanization of production and sectoral distribution of the labor force. It begins with the transition from preindustrial to early industrial conditions (1780–1820), ends with "mature industrialization" or *Hochindustrialisierung* (1882–1907), and thus appears to reject—for the *Wuppertal*'s industrialization—the watershed notion of development attached to concepts like "industrial revolution" or "take-off." Perhaps aggregate interpretations of Germany's industrialization that stress discontinuity require revision, after all.

A "useful" book is not necessarily a flawless one. I should like to mention two weaknesses that ought to interest American readers in particular. First, it is a rather parochial book, containing virtually no comparative references to analyses of industrialization in other countries or, indeed, to non-German sources at all. It fails, for example, to mention the important and accessible work on *Wuppertal* by the late Herbert Kisch, who did draw parallels between industrialization in the Wupper valley and industrialization elsewhere in the world. Second, it has no theoretical framework—neither of the general Marxian nor the specific neoclassical variety—and resorts at best to *ad hoc* theorizing. This limitation creates difficulties for readers who want to draw

theoretically oriented conclusions. The treatment of labor supply, for example, moves quickly from an interpretation stressing the positive effects of abundance to one stressing the virtues of scarcity (for labor-saving technical progress) without providing any systematic evidence that enables readers to decide the issue themselves (pp. 79–81). The same thing is true in the discussion of migration (pp. 72–73). It is quite likely that no one theory will do justice to these facts, but, by allowing the facts to dominate, Hoth makes generalization difficult and the reader's job slow going.

RICHARD H. TILLY
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ROLF WILHELM. *Das Verhältnis der süddeutschen Staaten zum Norddeutschen Bund (1867–1870)*. (Historische Studien, number 431.) Husum: Matthiesen Verlag. 1978. Pp. 192. DM 48.

In view of German historiography's turn from great personalities and great-power politics to domestic issues, Rolf Wilhelm's book at first glance may appear out of step if not old-fashioned. Is there still more to be said about German unification between 1867 and 1870? Indeed, the topics on which Wilhelm focuses are familiar: military reorganization in the south along Prussian lines after 1866, the reform of the *Zollverein* in 1867, and the project of a South German Confederation. Wilhelm's argument and the material he marshals, however, make a significant contribution to the question of whether a German national state under Prussia was primarily a consequence of Bismarck's last war. He extends Otto Becker's thesis, found in his posthumous *Bismarcks Ringen um Deutschlands Gestaltung* (1958), that German unification was well under way before the Franco-Prussian War. Wilhelm sees Prussia's relationship with the south as important as that with the great powers.

Wilhelm devotes more than one-third of the book to the south's military reorganization following the Peace of Prague. This reorganization occurred most energetically in Baden. The first state to seek a military convention with Prussia, Baden in 1867 made the Prussian military plenipotentiary in Karlsruhe its minister of war and in the following year appointed Prussian officers to key army positions. Reforms in Bavaria and Württemberg, although not going as far in undermining their states' rights, followed similar lines.

Political attempts to carry out the promise in Article IV of the Peace of Prague of a South German Confederation are also well known, but Wilhelm adds more weight to his argument through the use of south German archival material; he includes twelve pages of key documents. Popular opposition,

whether democratic or ultramontane, to Prussian domination was very strong—it swept the field in the elections to the newly formed customs union parliament in 1868. This fact, however, also explains the reluctance of the mutually jealous southern dynasties to establish a united southern parliament under the control of oppositional democrats and clericals. Thus, the Bavarian Prime Minister Hohenlohe's projected South German Confederation was, as the Württemberg Varnbüler realized, impossible within the framework of monarchy.

The force of military assimilation, southern pressures for economic unity (which Wilhelm correctly notes have not yet been properly studied), and the internal contradictions of any monarchical "third Germany" were moving southern Germany slowly into the arms of the North German Confederation. It is in this context that Wilhelm sets Bismarck's diplomacy of not forcing "unripe fruit" and his *Kaiserplan*. The latter, designed to outflank the nationalists and the particularists, offered both unity and an apparent return to the old Reich, not through Prussian conquest but by Hohenzollern adoption of a German imperial crown. When this project ran into opposition, especially from London and Munich, the vacancy on the Spanish throne provided an opportunity for Bismarck to nudge the inevitable unification along. Thus, Wilhelm ends on two points: after 1866 there was, in the long run, no alternative for the southern states to a national state along the lines Bismarck provided; and the controversy over the Spanish-throne candidature needs to be re-examined in this light.

Not everyone will be persuaded; nor are these ideas new. Wilhelm plays down opposition within the south, and his Bismarck is more patient, even-handed, and pacific than that of most historians. But this book shows that there is still life in the old questions of diplomatic and political history.

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MOSCHE ZIMMERMANN. *Hamburgischer Patriotismus und deutscher Nationalismus: Die Emanzipation der Juden in Hamburg, 1830–1865*. (Hamburger Beiträge zur Geschichte der Deutschen Juden, number 6.) Hamburg: Hans Christians Verlag. 1979. Pp. 266.

It is difficult to disagree with Mosche Zimmermann's insistence that Jewish history ought to be considered as "an important, integral aspect of the historical totality . . . that is closely linked to all areas of history and not isolated from history in general" (p. 9); and, on the whole, he is to be commended for his efforts to locate the experience of

Hamburg's Jews within both an urban and a national context for the middle third of the nineteenth century. This period witnessed profoundly important developments at three levels: legal emancipation for Hamburg's large and prosperous Jewish community, reform of the city's constitution, and essential determination of the future shape of a united Germany.

The author states his basic question as follows: "How relevant were considerations and activities related to German unification on the one hand and local patriotism and local interests on the other hand for the relationship between Jews and non-Jews in Hamburg?" (p. 11). He answers this question in two ways. In the opening chapters he emphasizes repeatedly that both spokesmen for and opponents of Jewish emancipation generally argued in terms of the well-being of the city; the former contended that economic discrimination deprived Hamburg of the best energies of some of its most capable inhabitants, whereas the latter sought to defend the interests of the city's lower middle classes. In the middle and later parts of the book, Zimmermann focuses less on the reasons men gave for their views than on the means thought necessary to achieve the ends desired by Jewish leaders. Could emancipation be carried out through gradual reform in Hamburg, or would it require revolutionary changes at the national level? Gabriel Riesser, the most characteristic individual treated in the book, reflected the views of many other Jewish liberals when he turned in despair to national politics during the 1840s, only to retreat once more to the local level after 1849, when legal equality was largely achieved within the city. The completion of Jewish emancipation in the 1860s subsequently confirmed the Jews' patriotic support for their Hanseatic homeland.

Although the author successfully makes his case, one may still ask how important a case it is. The central question has the advantage of opening up a wide frame of reference, but it does not lead to conclusions that strike this reviewer as particularly surprising. It is also regrettable that he does not say anything about localism versus nationalism among Jews in other parts of Germany. Finally, although this is not really a criticism of Zimmermann's book, he does not offer a general account of the Jewish experience in Hamburg itself. For that, one must still turn to the works of Helga Krohn: *Die Juden in Hamburg, 1800-1850: Ihre soziale, kulturelle, und politische Entwicklung während der Emanzipationszeit* (1967); and *Die Juden in Hamburg: Die politische, soziale, und kulturelle Entwicklung einer jüdischen Grossstadtgemeinde nach der Emanzipation, 1848-1918* (1974).

ANDREW LEES
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MARION A. KAPLAN. *The Jewish Feminist Movement in Germany: The Campaigns of the Jüdischer Frauenbund, 1904-1938*. (Contributions in Women's Studies, number 8.) Westport, Conn.: Greenwood Press. 1979. Pp. xi, viii, 229. \$17.50.

The Jewish Women's League (JFB), founded in Germany in 1904 and numbering fifty thousand women by 1925, aimed to maintain the traditions of the Jewish religion and the solidarity of the Jewish community and concentrated on charitable activities among German Jews. Although it belonged to the umbrella organization of German feminism, the *Bund Deutscher Frauenvereine* (BDF), the JFB, as Marion A. Kaplan notes, was isolated socially from German feminism because of its emphasis on "Jewish-mindedness" (p. 21). "The self-conscious feminism of the BDF . . . discomfited Jewish women" (p. 83), and, while the BDF concentrated on "improved educational, career and political opportunities for women," the JFB "bore a comfortable resemblance to traditional Jewish charities" (p. 83). In view of this it seems something of a misnomer to describe the JFB as a "feminist organization" (p. 136) and to talk of "its normally keen feminist analysis" (p. 201). JFB leaders insisted that "we are not the so-called emancipated women" (p. 75); and, if they campaigned for participation in the decision-making bodies of Jewish welfare organizations and encouraged women to gain an independent voice in Jewish life, they did not do so out of any conscious intention to use conventional female activities as a means of gaining equality, as Kaplan realizes (p. 75). Feminism, at its most minimal definition, involved some kind of ideological commitment to female equality (however conceived), and this the JFB did not have. Kaplan conveys the impression that it actively endorsed wider feminist goals such as the vote (pp. 58, 76), but she presents no evidence that it ever demanded anything more than women's suffrage on local and Jewish welfare and educational bodies. Ultimately, she is forced to admit that "the JFB sometimes lost track of its feminism while it absorbed itself in social work" (p. 74). It would have been less confusing to the reader had she dropped the term "feminist" altogether.

Despite this rather strained attempt to give the JFB feminist credentials, however, Kaplan's book, based mainly on the JFB's periodical, is clearly written, well organized, honest, and perceptive. Kaplan is good on Jewish women's ambivalent attitude to assimilation into German society and on the problems they encountered within the Jewish community. She is aware of the JFB's overwhelmingly middle-class composition and has some pertinent criticisms of its campaign to solve the "servant problem" through "home economics training" (pp. 179-81). On the JFB's major campaign against

"white slavery," however, I cannot help feeling that she has taken too much of the evidence at face value and that she has missed an important opportunity in failing to explore the psychological implications of the fact that the leading JFB campaigner in this respect, Bertha Pappenheim, first gained notoriety in another sphere as Freud's "Anna O." Her account of German feminism in general, moreover, is open to question: she is wrong, for example, in portraying the JFB as the most important religious organization in the BDF (p. 152)—the Protestant women's society was far more significant—and in saying that the female suffrage society neglected the vote in its early days (p. 64). Finally, the book's abrupt ending is unfortunate, to say the least.

RICHARD J. EVANS
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FRITZ FISCHER. *Bündnis der Eliten: Zur Kontinuität der Machtstrukturen in Deutschland, 1871-1945*. Düsseldorf: Droste Verlag. 1979. Pp. 112. DM 19.80.

This book is an expanded version of a lecture that Fritz Fischer delivered to the German Historical Association in October 1978. The title conveys its central argument. Fischer believes that there is an essential continuity in German "power structures" from 1871 to 1945, which was provided by a conservative alliance of "agrarian-aristocratic and industrial-bourgeois" power elites. Although the form of this alliance changed, its goals did not: a defense of power and privilege against the challenges of democracy and socialism. In times of crisis, this position required policies of repression at home and aggression abroad that ultimately culminated in the Nazi dictatorship's drive for world power.

Those familiar with Fischer's work will find nothing new in this book, but it does give a concise and vivid summary of the themes that he and his students have been developing for almost two decades. As such, it gives us an opportunity to review the accomplishments and shortcomings of the "Fischer school." First, I think that it is clear Fischer has succeeded in adding to our knowledge about the role of elites in the making of foreign and, to a lesser degree, domestic policy during the first half of the twentieth century. Second, he has managed to cast doubt on the sharpness of the distinction that some historians have tried to make between National Socialism and the main lines of German historical development. Once again, this achievement is much clearer in the area of foreign than in domestic affairs. Fischer has not, however, been able to demonstrate that his concept of continuity is the best organizing principle with which to understand the period between the formation of the Second Reich and the destruction of the Third.

Before we can accept the concept of continuity as Fischer defines it, I think we must know a good deal more about three issues. First, we need a precise and comprehensive analysis of the origins, education, occupations, and values of German elites from 1871 to 1945. We have gone as far as we can go with vague categories such as "agrarian-aristocratic" and "industrial-bourgeois," which may suggest some similarities over time but which also can mask some fundamental differences. Second, we need a much finer-grained picture of the political system. We now know quite a bit about some individuals and institutions, but we are a long way from understanding the changing nature and location of political power in the system as a whole. In this endeavor, notions like "power structures" and "power elites" do not help very much if they are used as slogans rather than as heuristic devices. Finally, we need some comparative studies in order to see if the power of conservative elites was in fact greater in Germany than in most other Western nations. In my opinion, a rather more plausible case for "continuity" can be made with regard to Britain, France, and Italy than it can for Germany.

Until we have a better idea about how to deal with these issues, the concept of continuity in German history must remain no more than a potentially useful hypothesis. It should not be allowed to ascend into the realm of accepted dogma, where it would obscure questions rather than illuminate them.

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THOMAS E. WILLEY. *Back to Kant: The Revival of Kantianism in German Social and Historical Thought, 1860-1914*. Detroit: Wayne State University Press. 1978. Pp. 231. \$17.95.

Neo-Kantianism was the major philosophical school in Germany from 1871 to 1914, yet the importance of this movement has slipped from historical memory. Two world wars, the Great Depression, the rise and fall of National Socialism, and, finally, the post-1945 conflict between East and West have made the lives and concerns of pre-1914 German intellectuals appear remote and irrelevant to our time. The significance for us of the movement back to Kant is that several generations of German liberals, in the midst of rapid economic and social change, failed to convince their compatriots of the value of individual liberty. Thomas E. Willey's study of Neo-Kantianism from the revolutions of 1848 to the Weimar Republic examines the fate of this neglected philosophical school.

Starting with Kant's credo, "Have the courage to use your own reason," Neo-Kantians took Kant's

concept of the autonomy of reason and united it with a view that the purpose of social existence is to encourage "the freedom of the individual to enhance his moral worth under laws hypothetically of his own making" (p. 23). By examining the Neo-Kantian applications of these principles to contemporary political and philosophical problems, Willey argues that he can see in these activities "the tentative and unsuccessful efforts of a segment of the upper bourgeoisie to make peace with the proletariat and to retain an attitude of cultural community with the West" (p. 23).

Willey divides adherents of the Neo-Kantian movement into two main groups. The early participants like Rudolf Lotze, Kuno Fischer, Eduard Zeller, Otto Liebmann, and Friedrich Lange revived Kant's ideas by rejecting metaphysics and forging an epistemological approach to philosophical problems. Lange closed the early phase by writing a definitive critique of materialism in 1866 and attempting to put Kantian ethics in the service of socialism. Willey gives Lange a special place in the development of Neo-Kantianism as the man who momentarily closed "the dangerous gap between thought and social reality" (p. 101).

The second phase of Neo-Kantianism, according to Willey, encompasses the works of Herman Cohen, Paul Natorp, Rudolf Stammler, Wilhelm Windelband, and Heinrich Rickert. These men examined problems of logic and values and methodological issues in light of the difference between natural and human sciences; yet their works turned increasingly to social and political questions. When examining the consequences of Neo-Kantianism in the Weimar Republic through the works of Ernst Troeltsch, Max Weber, and Ernst Cassirer, Willey attempts to convey the tragedy of Neo-Kantian liberalism: its inability to establish a viable link with the left and its failure to convince all Germans of the value of individual liberty.

Why did the Neo-Kantians fail to make a lasting mark on German society? Was it the result of their own philosophical outlook or the fault of forces beyond their control? The author believes that the Neo-Kantians had the right ideas but that the rest of the society let them down. Willey does not prove his case. He fails to explore the possibility that the individualistic orientation of Kantian philosophy would be repugnant to socialists who believed in the power of collective action; nor does he question that the abstract level of Neo-Kantian discourse would be incomprehensible and unacceptable to most Germans.

Lange's ability to take the fundamental insight of Kantian epistemology, the self-determining power of human reason, and apply it to the practical needs of the socialist movement is undeniable. Yet few liberals followed where Lange dared to tread.

Willey ignores this problem. In chapter 5, Willey emphatically denies that the Marburg failure to make allies on the left is the result of a flight from reality into philosophical abstractions (p. 103). The author proceeds to describe their works, however, in a manner that contradicts his generalization about them. Cohen made Neo-Kantian epistemology "a purely formal study of conceptualization" (p. 109), and Stammler's analysis of law led him to withdraw all empirical content from law in order to find an abstract, universal standard (p. 127). Clearly, some Neo-Kantians were trying to show that a priori laws of human reason were the basis of human actions. In pursuing this goal they may have demonstrated the self-determining character of human reason in theory, but the practical result was political irrelevancy. No one outside the movement was listening.

Willey's study is bold in its conception but faulty in its execution. The author tries to cover too much territory with too little time devoted to each man's philosophy. Consequently, the reader is given a few general ideas on each person but is left seeking the connection between these men's philosophies and the book's main argument. The author ascribes to the Neo-Kantian movement a liberal democratic bias that tried to go beyond the limits of German philosophy and middle-class politics. In fact, Willey has presented us a group portrait of individuals who expressed rather than transcended those limits.

DAVID LIPTON
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KARL HEINRICH POHL. *Weimars Wirtschaft und die Aussenpolitik der Republik, 1924-1926: Vom Dawes-Plan zum Internationalen Eisenpakt*. Düsseldorf: Droste Verlag, 1979. Pp. 452. DM 48.

Karl Heinrich Pohl believes that historians of German foreign policy in the mid-twenties have concentrated too heavily on the controversial and well-documented activity of Gustav Stresemann and too little on the role of the German iron and steel industry. His study focuses mainly on official trade treaty and private steel cartel negotiations between Germany and France, beginning after the London Conference of 1924 and ending with the private International Steel Pact of September 1926 and the final Franco-German trade treaty of August 1927. He relates these negotiations to the marketing problems of the French and German steel producers and to the conflict between the interest of German steel-makers in higher steel prices and that of the German machine industry in lower steel costs and better access to French markets. He also maintains that the Ruhr industrialists, by gaining control of their domestic and foreign markets, enhanced their ca-

capacity to pursue repressive social policies and reactionary political maneuvers at home.

At the start, Pohl writes that he regards the regaining of Germany sovereignty in trade policy (January 1925), the December 1924 and May 1926 agreements between the German steel and machine industries, and the International Steel Pact as "pre-suppositions or else pathfinders for 'Locarno' and 'Geneva' (the diplomatic high points) and as decisive events of German foreign policy" (p. 5). (Pohl's concepts are often loose, *Aussenpolitik* attaining Heideggerian obscurity on page 278.) Pohl does show that the Ruhr industrialists could veto a government initiative counter to their interests, the 1925 Saar agreement. But he does not show that industry cleared the way for, let alone influenced, Stresemann's proposals. Neither does he demonstrate (as Hans-Ulrich Wehler has done with grain tariffs in the 1880s) that interest-group programs indirectly shaped diplomatic relations. Actually, Pohl plays down the evidence in his sources that the chief Ruhr negotiator, Fritz Thyssen, wanted to use the strong position of the German steel industry to force a French evacuation of the Rhineland and other political concessions; this evidence is awkward for Pohl's thesis of dominance by heavy industry, since Thyssen's policy was not adopted. Pohl stresses the greed and willfulness of the steelmakers and seems to understate their problem of overproduction, their ultimate subordination to the political judgment of the Foreign Ministry, and their nationalism. This last was strong, even if it often served, and sometimes yielded before, their own advantage.

Pohl has done very extensive research, using forty-seven German-language archival collections. French public and private archives, however, might have added information, insight, and perspective. Pohl's bibliography is weak in non-German studies; notable omissions are Charles S. Maier's *Recasting Bourgeois Europe* (1975) and F. G. Stambrook's "Das Kind" article (*Central European History*, 1 [1968]: 233-63). The relation of the cartel negotiations to Stresemann's policies deserves another study, less parochial in its sources and more precise in its concepts, a study that examines its subject as an open problem and arrives at conclusions developed from relevant evidence by persuasive argument.

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JÜRGEN ARETZ. *Katholische Arbeiterbewegung und Nationalsozialismus: Der Verband katholischer Arbeiter- und Knappenvereine Westdeutschlands, 1923-1945*. (Veröffentlichungen der Kommission für Zeitgeschichte. Series B, Forschungen, number 25.) Mainz:

Matthias Grünewald Verlag. 1978. Pp. xxix, 252. DM 56.

The small-print subtitle of this monograph, *The Association of Catholic Workers' and Miners' Clubs in Western Germany, 1923-1945*, is a much better guide to its contents than the title. Historians hoping for that long-needed study of the last years of the Christian Trade Unions must remain patient. After giving a sketchy summary of its early development, Jürgen Aretz charts in exhaustive detail the policies of the association's leadership after 1930. The four main leaders, Joost, Müller, Letterhaus, and Gross were strongly anti-Nazi from an early date: one of them survived the concentration camps, one died in a prison hospital, and two were executed after July 20, 1944. Their organization, by far the largest in the Reich Association, covered the ten dioceses of northwest Germany; in 1930 the 192,000 members were organized in some 2,000 clubs and were catered for by 81 functionaries and by a weekly newspaper that sold over 150,000 copies. The internal structure of the association was authoritarian. It was a powerful organization that commanded a great deal of loyalty from its (gradually aging) male membership. (On the position of female Catholic workers the author says nothing.)

The popularity of the clubs is not adequately explained in this study. The services offered to members are only touched upon briefly, the nature of club life is barely mentioned, and relations with the Christian Trade Unions remain obscure. But popular they were: pilgrimages of disguised political protest attracted 200,000 participants in the latter part of 1934, and membership held up well until 1936 despite Nazi pressures. The clubs escaped destruction in 1933 because they were inscribed as religious rather than social or economic organizations, and they could plausibly claim protection under the concordat. The German Labor Front denied this claim and sought to prohibit dual membership. Against the background of recurrent local campaigns of intimidation, the Catholic hierarchy put the association's case (not always firmly or with enthusiasm) to the Reich government, which simply played for time. These protracted nonnegotiations are described well. That the undermining of the clubs in seven dioceses was a slow and piecemeal affair was due in part to the tactical skill and pertinacity of the association's leaders and to the willingness of members to risk losing their jobs or their supplementary pension rights. In 1938, however, the newspaper was permanently banned, and in 1939 the dissolution of the whole association appears only to have been forestalled by the intervention of General von Hammerstein-Equord. During the war activities and membership declined still

further, and the leaders engaged in political resistance.

This study rests on much newly available source material, and the narrative of the leadership's defensive strategies is potentially of general interest to students of the Third Reich. But Aretz's approach is remorselessly monographic; he leaves his readers to draw all their own conclusions concerning the wider significance of his findings, even within the context of the *Kirchenkampf*. It does not become fully clear why the persecution of the association was not more severe—the dissolution of all clubs in the Münster area in 1935 showed that the regime could risk a confrontation without causing major public disturbances. This revised version of a Bonn thesis tends toward homage to the leaders of the association rather than toward analysis. It is, however, anything but a popularizing celebration of these unusual men—the prose is leaden and ungainly, often with obscure allusions and three footnotes per sentence.

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WILLIAM E. GRIFFITH. *The Ostpolitik of the Federal Republic of Germany*. (Studies in Communism, Revisionism, and Revolution, number 24.) Cambridge: MIT Press. 1978. Pp. 325. \$25.00.

This is a remarkable and in large part authoritative treatment of the complex relationships of the Federal Republic of Germany with the Soviet Union and various Eastern European countries. Beginning with some historical antecedents, the author carefully reconstructs the Eastern policies of successive West German governments by contrasting lists of minimal and marginal goals with the possibilities and achievements of each of the governments involved. There is probably no better secondary treatment available today than William E. Griffith's analysis of the varied motives of different Eastern European governments at the time of the Czechoslovak invasion of 1968 or of the *Ostpolitik* initiatives of Schröder, Kiesinger, and Brandt. Even the impact of China on the polycentric perspectives of *Ostpolitik* and on the "globalization" of European politics receives its well-considered due. Against this background, West German *Ostpolitik* measures evolve with a certain logical inevitability rather than as the creative, or even subversive, schemes of certain men or as shortsighted concessions to the expanding colossus of Soviet power. The author concludes, quite rightly in this reviewer's opinion, that the *Ostpolitik* of the early 1970s on balance produced substantial benefits for the Federal Republic

of Germany, West Berlin, the West in general, the Soviet Union, and Eastern Europe, though perhaps least of all for the East German DDR.

In the midst of a welter of discursive details, Griffith often succeeds in giving the reader revealing insights into the nature of German politics such as in his characterization of the new German left: "the latest example of the recurrent German revolt against Western bourgeois, affluent, materialistic society, as opposed to ascetic, egalitarian, Prussian, romantic virtues . . ." (p. 163). His description of how the Czechoslovak invasion actually served to clear the decks for the rapprochement to follow (pp. 160–61), his multilingual analysis of the text of the Renunciation-of-Force Treaty between Bonn and Moscow (pp. 191–93), and his perceptive account of the decline and fall of Walter Ulbricht (pp. 201–06) are among the high points of this book.

On the debit side, the author has not made it easy for readers to find their way through the endless turns and details of West German foreign politics of the 1960s. Even specialists may well learn more here than they might care to know. Only rarely is there a summary or other guidepost for the reader. The copyeditors, too, have failed to use their blue pencils and have let innumerable typographical errors and the like slip by, particularly in the last chapters. Finally, the author's strength lies obviously more in the analysis of complex international relationships than in the description of domestic structures and conflicts. The tension between the democratic politics of parties, elections, and public opinion polls and the international interaction of policy making and intellectual elites is not always resolved in this book.

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BRIGITTE HAMANN. *Rudolf: Kronprinz und Rebell*. Vienna: Amalthea. 1978. Pp. 535. S 298.

The life of Crown Prince Rudolf von Habsburg symbolizes the hopes and disillusionments of Austrian liberalism between 1858 and 1889. His mysterious suicide at Mayerling and the chance that he might have been the savior of liberal Austria continue to stimulate a larger literature than any of his actual achievements would justify.

Those already familiar with the literature on Rudolf should not expect major revelations from Brigitte Hamann's new biography, but it is well written and displays a thorough knowledge of the sources, many of which have become available only since Oskar von Mitis's standard biography of 1928. This shortened version of a dissertation, completed under Adam Wandruszka in 1978, remains longer

and more detailed than necessary; but Hamann does present a full picture of Rudolf's thought and attitudes. She portrays Rudolf's remarkable education, his love of science, his devotion to his military duties, his secret career as a liberal journalist, and his political impotence as heir apparent. Her account succeeds not only as a sympathetic portrayal of a man's fate but also as an illuminating perspective on the decline of liberal values in the 1880s. Her argument builds persuasively and culminates in the inevitable preoccupation with the still unexplained double suicide.

Although Hamann rejects the theory of Rudolf's involvement in a Hungarian conspiracy against Franz Joseph, politics bulks large in her multicausal portrayal of Rudolf's state of mind before the suicide. Franz Joseph seemed unlikely ever to abdicate or to allow his son into circles of power and policy. Moreover, Rudolf was depressed by recent criticism of him in the anti-Semitic press and by the repercussions of his own sometimes dubious political activities. Finally, Hamann emphasizes the accession of his *bête noire*, Wilhelm II, to the German throne and the likelihood of a Franco-German war in the wake of Boulanger's electoral successes. Rudolf was convinced that war would mean the crushing of Austria by Russia and the end of his dream of a liberal-Slav alliance, including France, Austria, Russia, and perhaps even England.

Hamann seems to feel, however, that personal considerations weighed more heavily in Rudolf's decision to commit suicide. She is no more inclined than von Mitis to believe that Rudolf was in love with Mary Vetsera, but Hamann emphasizes Rudolf's gonorrhea and rapid physical decline, his loveless marriage, his father's denial of a divorce, Mary's pregnancy, and her adolescent encouragement of his suicidal fantasies. Apparently Rudolf was crushed by external factors too overwhelming to sort out, but a more psychological account would certainly have been justified. Rudolf's bad personal relationships with almost everyone in his family and in the court deserve more thoughtful analysis, and Hamann might have done more to make sense of his education, his emotional and religious development, and the tensions between the aristocratic and bourgeois components of his personality. All the available facts are here, but one can hardly avoid wishing that Hamann had attempted something more of a psychobiography.

Nevertheless, Hamann's book is now the most complete and scholarly account of Rudolf's life. The only justification for another biography would be firmer knowledge about the motives for Rudolf's suicide or the application of less traditional methodologies.

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STEFAN MALFER. *Wien und Rom nach dem Ersten Weltkrieg: Österreichisch-italienische Beziehungen, 1919–1923*. (Veröffentlichungen der Kommission für Neuere Geschichte Österreichs, number 66.) Vienna: Hermann Böhlau Nachf. 1978. Pp. 186. DM 58.

Wien und Rom nach dem Ersten Weltkrieg is a concise and remarkably objective account of a brief but critical period in Austro-Italian relations. The author's thesis is that the traditional enmity between the two countries need not have continued after the First World War. Stefan Malfer's detailed study of the period from the signing of the Treaty of St. Germain through Mussolini's first year in power describes the opportunity for good relations that the various governments on both sides—prior to Mussolini—were anxious to exploit.

The Italian people, or at any rate the democrats and socialists, were willing to forget the past and indeed pursued policies that were frequently beneficial to Austria. Even the South Tyrol was not an inevitable bone of contention; the democratic governments of Italy promised the area autonomy.

For diplomatically and economically isolated Austria, Italy was the only power that could help settle the many issues left unresolved by St. Germain. In the territorial disputes involving Carinthia and the Burgenland, its assistance was particularly valuable. Italy supported Austria's claim to most of southern Carinthia because it wanted to preserve its most direct rail link to friendly Hungary. Italy mediated a compromise for the Burgenland by which Hungary withdrew its irregular troops in exchange for Austrian agreement to a plebiscite in Ödenburg (Sopron). Although the Austrian public blamed Italy for the negative outcome of that questionable vote, Malfer points out that the Austrian political parties had already recognized the country's diplomatic weakness and the need for offering Hungary something tangible in return for receiving most of the disputed borderland.

Less helpful to Austria was Italy's role during the discussions leading to the Geneva Protocols and to the loan by the League of Nations in 1922. The attitude of the Italian delegation, however, was determined by its preference for an Austro-Italian customs union.

A reversal in the promising new Austro-Italian relationship came almost as soon as Mussolini took over as prime minister. The Duce aggressively pursued the aim of Italian nationalists and Fascists to make the Danube—especially Austria and Hungary—an Italian sphere of influence. For this policy good relations with Austria were obviously desirable. But even more important was the Fascists' goal of totalitarian centralization, which meant in part the Italianization of the South Tyrol. It was this policy that soured Austro-Italian relations. Malfer concedes, however, that the revival of

Anschluss sentiments in Austria helped aggravate growing Italian insecurity about the Brenner. By the end of 1923 all the issues that would lead to the deterioration of Austro-Italian relations were already apparent: Fascist radical denationalization, press wars, and angry parliamentary debates.

Wien und Rom nach dem Ersten Weltkrieg grew out of a dissertation written at the Historical Institute of the University of Vienna. It is based primarily on documents found in the Neue Politische Archiv of the Haus-, Hof-, und Staatsarchiv as well as on a wealth of secondary sources in German, Italian, and English. The author can be faulted only for neglecting to use Italian diplomatic documents and for providing too little background information, especially on Italy's role at the Paris Peace Conference.

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UMBERTO MAZZONE. *"El buon governo": Un progetto di riforma generale nella Firenze savonaroliana.* (Biblioteca di Storia Toscana Moderna e Contemporanea. Studi e Documenti, number 18.) Florence: Leo S. Olschki. 1978. Pp. xv, 217.

The *Riforma sancta et pretiosa*, published at Florence in 1497, was written by Domenico Cecchi, a modest artisan-shopkeeper. This text (transcribed in the appendix) has been known to historians of the Savonarolan years, but it has been neglected as a rude product of ignorance and partisanship. Umberto Mazzone, although he recognizes that Cecchi's treatise contained little of originality, attempts to salvage the reputation and historical value of the text. Taking as his point of departure the observation of Donald Weinstein that history of Savonarolan Florence devotes too much attention to personalities and far too little to society, Mazzone places the *Riforma* in context by discussing one-by-one the sixteen reforms proposed by Cecchi, setting them in the framework of the ongoing debates over those issues among supporters and opponents of Savonarola.

Mazzone begins by tracing Cecchi's family background and circumstances—a difficult task in which he achieves considerable success despite a scarcity of evidence—concluding that Cecchi's ardent pro-Savonarolan position and his interest in civic reform sprang from the fact that he was among that middle stratum of Florentine society that gained political participation as a result of Savonarola (though Cecchi himself seems never to have held office). Mazzone then advances to the heart of the book, the analysis of the particular proposals espoused by Cecchi. This painstaking analysis reveals that Cecchi was sensitive to the issues of his time: material issues like the equitable distribution of political offices and taxation, reform of dowry prac-

tices, encouragement of commercial activity, and military organization and moral issues like the elimination of sodomy (a catchword for a complex of sexual transgressions) and official corruption, establishment of a university, and expulsion of Jews. Cecchi's proposals also indicate that, although he was a Savonarolan, he was capable of criticizing and differing from Savonarola at points.

Mazzone's contextual analysis of the *Riforma* affords valuable insights into the issues current from 1494 to 1498. These insights, however, are limited by the author's method. He treats Cecchi's *Riforma* only contextually; he never discusses it as a whole to determine its textual logic. Also, the contextual logic he exposes is purely isomorphic with the text. One has no idea whether there were burning issues that Cecchi avoided or ignored. The piecemeal treatment of the *Riforma* means the reader never sees the forest for the trees. A more diversified approach might also have given Mazzone another perspective on an important issue—the *Riforma*'s silent reception in Florence. He argues that Cecchi's reforms were rendered moot by the waning of Savonarola's influence, but there is much more that could be said about the treatise itself. The unenthusiastic reception given the ideas of a typical member of the artisan class may provide a clue as to why Savonarola lost support and why there never was in Florence a *riforma sancta et pretiosa*.

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GABRIELLA CIAMPI. *I Liberali moderati siciliani in esilio nel decennio di preparazione.* (Istituto di Storia Moderna, Storia, number 5.) Rome: Edizioni dell'Ateneo and Bizzarri. 1979. Pp. 235. L. 7,500.

The political experiences, ideas, and aspirations of the unsuccessful revolutionaries of 1848–49 have been an important theme of Italian historiography in the last twenty years. Like earlier works by Della Paruta, Scirocco, Ganci, and others, this book focuses on the lessons that the "founding fathers" of modern Italy learned from the failure of the revolution. Using the moderate liberal leaders of the Sicilian revolution as a test case, Gabriella Ciampi analyzes their political and intellectual development in the crucial decade before Italy's unification. Her analysis is based on a wide range of published sources and on a few, but important, manuscript collections, such as the Torre Arsa family papers.

In contrast to their left-wing colleagues, the most prominent Sicilian liberals were spared long prison terms or persecution by the governments of host countries. Important protagonists of the Sicilian revolution, such as Filippo Cordova, found a hospitable refuge in Piedmont-Sardinia. For that reason, Ciampi rightly focuses her attention on their en-

counter with Piedmontese culture and political institutions while she makes only passing references to Sicilian exiles abroad. Known in Piedmont-Sardinia as the men who had offered their island kingdom to a member of the House of Savoy, the Sicilian liberals settled in Turin or Genoa with relative ease and were quickly integrated in the society and economy of their host country. Nonetheless, nearly all of them remained reluctant exiles, eager for a second chance to lead the liberation of their beloved island from Bourbon rule.

Ciampi's sympathetic, crisply written account of their experiences has several merits. First, she clearly shows that the postrevolutionary debate between the Sicilian liberals and their democratic critics on the left did not hinge upon institutional issues (monarchy versus republic) or even upon issues of revolutionary strategy. Rather, it stemmed from profound ideological differences between the two groups concerning the goals of the Italian revolution. Reflecting differences of social background and aspirations, the liberals conceived those goals in primarily political terms while the democrats stressed the social and cultural dimensions of the national revolution. Second, Ciampi's careful reconstruction of individual and group experiences demonstrates that the Sicilian liberals' rapprochement with Cavour and other northern leaders in the late 1850s and their acceptance of his annexationist policy in 1860 came very slowly. Even as they joined their fellow Sicilian Giuseppe La Farina in the effort to unify Italy under the constitutional regime of Victor Emmanuel of Savoy, the Sicilian liberals had reservations.

Basically, Ciampi argues, the liberals remained faithful to the ideal of 1848: a Sicilian constitutional monarchy, separate from the hated Neapolitans but also separate from other Italian regions whose traditions and social structure were incompatible with theirs. Their preference for a separate Sicilian kingdom, however, did not reflect a provincial or narrow-minded outlook. Keen jurists like Cordova and eminent economists like Francesco Ferrara were fully capable of appreciating the institutions and civilization of other lands. But as Sicilians and as liberals they were convinced, in the light of the events of 1848-49, that only a separate kingdom under their own leadership would provide the appropriate conditions for a political revolution without social upheaval.

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RAFFAELE ROMANELLI. *L'Italia liberale (1861-1900)*. (Storia d'Italia dall'Unità alla Repubblica, number 2.) Bologna: Il Mulino. 1979. Pp. 534. L. 6,000.

Multivolume historical studies, whether dedicated to centuries of diverse matters or less extensive treatments of particular sectors of the national scene, seem to thrive and proliferate in Italy with amazing vitality. Although the need for or *raison d'être* of some of these productions is indeed questionable, others can provide a pleasant and rewarding surprise. The latter is the case with Raffaele Romanelli's volume on the first decades of the liberal kingdom in Italy.

Romanelli, a highly promising young scholar of the exciting postwar generation in Italy, has given us a most refreshing interpretive work on an already much-studied era, but he shows us throughout how much more reflection the period still deserves. Conceived with chronology only as a slightly abstract backdrop, this book is essentially topical. Naturally many of the topics are the same ones that have long been present in any account of the same timespan; but there is a great deal of new information, and some of the old issues are dealt with in some detail for the first time in a general history.

The theme of this book, obviously reflecting the author's political convictions, is that most of the problems, frustrations, and disappointments the new nation faced derived from a *révolution bourgeoise manquée*. Although there is nothing novel in that view, the topics Romanelli deals with and his mode of treatment contain a surprising number of stimulating and provocative ideas based on a wealth of reading and the fruits of recent research in Italy. The result is revisionism in its best manifestation. If Romanelli is clearly an economic determinist and if he has been much influenced by the Marxist tradition in recent Italian historiography, he nevertheless maintains a commendable independence of thought and objective detachment. In short, he demonstrates a certain maturity that is now appearing in the treatment of the heritage of the Risorgimento. If anything, he almost overstates the anachronistic nature of the thought and action of the generation of leaders who united Italy as well as the backwardness of the regions stitched into the boot, but he is always on well-documented ground. He is, thus, realistic, without being either tragic or dramatic, in his assessments of a truly pathetic and staggering set of obstacles for those who would make of Italy at least the smallest of the great powers.

Another praiseworthy characteristic of this book is its total abandonment of that mode of viewing Italian history in terms of the reigns of its great prime ministers. Cavour, Depretis, Crispi, and Giolitti are reduced to very human proportions in Romanelli's treatment. Also some of the oft-deprecated figures in this period get praise for their work in appropriately reasoned measure where they deserve it: Crispi, for his domestic reforms of the late

1880s; Sonnino, for his financial measures in the 1890s. The same is true in the evaluation of the Southern Question, shown to have been perpetuated as much by northern doctrinaires as by any native problems in the region. And the same can be said for a number of other topics that have become trite and jaded in the hands of others. In other words, what might appear to be just one more narrative covering the same old ground is, instead, a fascinating account with much new information in a compact, intelligent, and highly readable book. The style is refreshingly crisp, clear, and rich.

Whether the other volumes projected for this series can meet Romanelli's standard is yet to be seen, but the authors are very able. For the years prior to Romanelli's work, Franco della Paruta is to deal with the struggle for unification. Alberto Aquarone has the Giolittian period, Danilo Veneruso that of Fascism, and Pietro Scoppola Republican Italy. It is to be hoped that they will devote more space to foreign affairs, Romanelli's one area of excessive brevity.

The format of this book, a reasonably priced paperback, is fortunately modest—as unpretentious as its author. The contents are still a great bargain. Following the four hundred pages of text are a chronology (too sketchy) of the years 1861 to 1900, a complete listing of the governments with inclusive dates and the personnel heading the ministries, fifty pages of extremely useful and enlightening statistics, and, finally, an unusually rich fifty pages of bibliography. The convenience of these data for Italian readers is clear, but for foreigners it is even more useful since much of this information is difficult to find outside major research libraries.

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HUGO BUTLER. *Gaetano Salvemini und die italienische Politik vor dem Ersten Weltkrieg*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 50.) Tübingen: Max Niemeyer Verlag. 1978. Pp. xxiii, 498. DM 116.

Gaetano Salvemini (1873–1957) constantly pricked the conscience of the Italian nation. Hugo Bütler's outstanding University of Zurich dissertation, based on an exhaustive examination of Salvemini's vast collected works as a political agitator and historian and the studies that he provoked, describes his thought and action to 1915.

Profoundly influenced by the poverty that he experienced and observed from his native Molfetta in Apulia and by positivism, which he encountered at the University of Florence, Salvemini maintained a primary interest in arousing and organizing the agricultural proletariat in the Mezzogiorno to seek a better life. He was impatient with speculative theo-

ries and an advocate of practical and concrete reforms such as free trade, more equitable taxation, the right to vote, a federal system of government, and school reform. One of his first practical successes came in 1901 when he helped organize the secondary school teachers into a union.

Salvemini was appointed professor of history at the University of Messina at 28. He lost his whole family in the earthquake of December 1908. In 1910–16 he was professor at Pisa and in 1916 became professor at Florence. He suffered defeats as a candidate for the Chamber of Deputies in 1910 and 1913 but was a member of the provincial council at Bari in 1914. Elected to parliament in 1919, he resigned at the end of 1920. He had been quite friendly to Mussolini in 1914, but he fled from Fascism in 1925 after helping to organize the underground publication *Non Mollare*. He taught at Harvard from 1934 to 1948 and returned to Florence in 1949.

Although a Socialist Party member from 1893 to 1911, Salvemini seems never to have been strongly attracted to Marxist theory and soon became one of the most severe critics of the Italian socialists. His reading of the works of Mazzini and Carlo Cattaneo in 1898–99 and the unrest and repression that he saw around him made him for a time a revolutionary republican and left him an advocate of federalism along the Swiss model. He first blamed the socialists for advocating moderation and calm during these troubled days and then for backing the Giolitti government in what he regarded as an unholy alliance with the feudal and city bourgeois elements in the South to keep the ignorant agrarian masses in subjection. Socialism, he charged, was sacrificing the agrarian proletariat for Giolitti's small favors to the industrial proletariat.

Having lost faith in all political parties, Salvemini established his own mouthpiece, *L'Unità*, in 1911 to agitate for major concrete reforms and to develop a new ruling class. His bitter opposition to Giolitti seemed to soften, but only briefly, when the latter began promoting manhood suffrage.

A severe critic of Italian imperialism, especially of the Libyan war, of the Triple Alliance, and of Italian foreign policy in general, Salvemini pushed for Italy's entry into war in 1915 on the side of the Triple Entente and volunteered for the army. A critical reappraisal of his views on foreign policy, based on a more complete examination of the scholarly studies now available, would have made this study even more valuable.

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FRANCA PIERONI BORTOLOTTI. *Femminismo e partiti politici in Italia, 1919–1926*. (La Questione Femminile,

number 18.) Rome: Editori Riuniti. 1978. Pp. 408. L. 4,800.

In *Studi Storici* in 1973, Franca Pieroni Bortolotti called for the kind of concrete and detailed research that would establish the "precise physiognomy" of the history of Italian women as distinct from the models already developed for other Western nations. She, herself, stands almost alone in fulfilling this prescription, having traced the development of the Italian women's movement in two earlier works, *Alle origini del movimento femminile in Italia, 1848-1892* (1964) and *Socialismo e questione femminile in Italia, 1892-1922* (1976). In her recently published third volume, *Femminismo e partiti politici in Italia, 1919-1926*, she analyzes the struggle between feminist and antifeminist organizations after World War I and until the repression of all non-Fascist women's groups by Mussolini's dictatorship in 1926.

Centering her book on the postwar debates concerning women's suffrage and divorce, Pieroni Bortolotti emphasizes that such nineteenth-century issues did not die with the war but re-emerged only to be co-opted by the Fascists until their power was great enough to deny civil and political rights to both sexes. The Chamber of Deputies, for example, overwhelmingly approved a bill granting political and administrative suffrage for all women, although the Senate failed to act on the issue. Feminist sentiment remained strong enough that in 1925 Mussolini supported a successful bill to allow a few categories of women, such as mothers of sons killed in war, to vote on the local level. Before these women went to the polls, however, Mussolini abolished the election of local officials, thus nullifying the very restricted concessions of his suffrage legislation.

Like much of Italian scholarship, Pieroni Bortolotti's book is shaped by her own explicit political views. A member of the Communist Party, she devotes over half the book to a detailed analysis of the position of those in the Socialist and Communist Parties as well as the Third International. She challenges the view held by many historians that the parties of the left have traditionally ignored the oppression of women to concentrate on class conflict; yet she is quick to criticize many of their members for sexism or insensitivity to the importance of civil and political rights for women. She admires those bourgeois feminists, often dismissed by their Marxist contemporaries, who kept a clear view of the importance of equal rights in the face of the Fascist challenge. Even when dealing with the Catholic and Fascist organizations, with which she is unsympathetic, she stresses the diversity and debate within these groups that have often been characterized as monolithic in their antifeminism.

Although Pieroni Bortolotti provides a wealth of new information, her manner of presentation hinders

her understanding. Rather than being grouped around themes, her discussion often follows the chronological and internal order of the speeches, memoirs, and party publications that she consulted. This tedious and confusing organization obscures and weakens her major points. Furthermore, in her effort to place Italy in a comparative framework, she misrepresents the character of American feminism by basing her account almost exclusively on the memoirs of Elizabeth Gurley Flynn, a member of the I.W.W. Her portrayal of American feminists as predominantly socialist displays wishful thinking rather than the reality of a country that lacked the large and institutionalized Marxist parties of Italy.

Despite the defects in her book, Pieroni Bortolotti must be hailed as the pioneer of Italian women's history. It is to be hoped that a fourth volume will continue her narrative of the vicissitudes of Italian feminism after 1926.

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DAVID D. ROBERTS. *The Syndicalist Tradition and Italian Fascism*. Chapel Hill: University of North Carolina Press. 1979. Pp. x, 410. \$22.50.

David Roberts's important and often controversial study traces the transformation of syndicalism from an ideology of proletarian renewal to "national syndicalism" that sought its social base among all producers, regardless of class. By tracing the development of thinkers like Agostino Lanzillo, A. O. Olivetti, and Sergio Panunzio, Roberts shows how national syndicalism merged with fascism and influenced Mussolini and several future Fascist leaders like Dino Grandi and Italo Balbo.

The author defies conventional wisdom about the purely reactionary character of fascism by linking left-wing fascism to the democratic heritage of Giuseppe Mazzini. In their attempts to grapple with a flawed liberal Italy, the syndicalists claimed part of the legacy of Mazzinian populism. Although Roberts is excessively hard on the leaders of liberal Italy, he reveals how defects in the economic and political system bred deep disillusionment. Bureaucratic overcentralization, economic parasitism, and political corruption led many young bourgeois intellectuals to identify with the proletariat and to seek an alternative society based on syndical association. This syndicalist faith in the proletariat had little in common with orthodox Marxism. These intellectuals, motivated by an intense need to find a mass base for their rejection of the existing political system, never established solid ties to the proletariat and during World War I drifted from proletarian syndicalism to national syndicalism of the productive classes.

Roberts stresses the sincerity and idealism of the syndicalists in their hopeless task of realizing through fascism a new conception of democratic mass participation based on corporative association. He devotes several perceptive chapters to the contrasts between syndicalist thought and the dominant nationalist orientation of fascism. Especially valuable is the final chapter dealing with the differences between fascism and nazism, due in part to left fascism's positive response to the challenge of modernization and to its ties with the democratic tradition.

On a few points, however, this book is misleading. Events like the Matteotti crisis are interpreted from a syndicalist perspective when the syndicalists were not decisive protagonists. The constant defeats of left fascism are explained away almost as though the author had a vested interest in reviving the corpse. Roberts is often excessively generous. It mattered little that Fascist labor leaders were sincere when the reality was that Fascist unions were totally unable to protect the workers, especially the large number of organized peasants, from the revenge of the employers. The failure of left fascism is inexplicable without dealing more fully with the opposition of powerful economic interest groups like the General Confederation of Industry. The author is simply not convincing in his efforts to show left fascism as a continuing possibility for the regime. Finally, the terms "left fascist" and "fascist populist" are used to refer to such diverse people (Bottai, Farinacci, Panunzio, Grandi) under such widely differing circumstances that they lose clarity.

Notwithstanding these reservations, this well-documented book will provoke a useful debate and will add a new dimension to our knowledge of Italian fascism.

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RICHARD CLOGG. *A Short History of Modern Greece*. New York: Cambridge University Press. 1979. Pp. viii, 241. Cloth \$26.00, paper \$8.95.

This book is part of a series of short histories of modern nations issued by Cambridge University Press and including already published surveys on Germany, Italy, France, Switzerland, England, Yugoslavia, Portugal, and Mexico. The author of this volume, Richard Clogg, is a specialist in Greek history, who has previously published studies on the pre-1820s and the post-1945 period. His narrative commences with a discussion of Byzantium after 1204 and concludes in 1978. His emphasis is on the modern era, with over half of the book (pp. 105-225) covering the years since 1913.

As the author recognizes, the major problem in preparing a short survey is the question of the material to be presented. It is difficult to fault the choices made here. The book emphasizes domestic political history and foreign policy, subjects on which a great deal of basic research has been done. Clogg regrets "having devoted less attention to . . . social, economic and cultural developments" (p. vii), but, in fact, he has included these aspects of Greek history when they are of crucial importance. For instance, he describes well the cultural background to the Greek Revolution of 1821 and the recurrent severe economic problems. The book, however, does not attempt to discuss life in the village and city as do, for instance, John Campbell and Philip Sherrard's *Modern Greece* (1968) and William H. McNeill's *Metamorphosis of Greece since World War II* (1978). The sections on international affairs deal primarily with Greek relations with the great powers and devote comparatively little attention to the country's neighbors. The conflict with Turkey over Cyprus is, however, fully described. Nevertheless, within the necessary limits on length, the author has succeeded in presenting the major issues facing the Greek people in these years.

This book should be extremely useful in classes on modern history and political science where a balanced, authoritative survey of Greek history is needed. This is certainly the best work of its kind available. It should also be of interest to anyone who would like to read a scholarly, well-written discussion of the background to the present events in the Mediterranean. One of the admirable aspects of this study is its careful treatment of the many controversial aspects of Greek politics, questions that are handled with reason and impartiality. The author, with wide experience in the field, has a sympathetic yet critical attitude toward his subject. It is to be hoped that this series will contain similar books on the history of other East European states.

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ISTVAN DEAK. *The Lawful Revolution: Louis Kossuth and the Hungarians, 1848-1849*. New York: Columbia University Press. 1979. Pp. xxi, 415. \$16.95.

This reconsideration of the Magyar struggle for independence, with its emphasis upon Louis Kossuth, tells its story logically and simply. Throughout, the author comes to grips with the legends that every romantic failure generates and with the controversies that national bias and economic determinism have stimulated. In the years 1971-73 Istvan Deak worked in the Hungarian National Archives to add to his analysis of the great amount of secondary material available.

Although Kossuth is its pivotal figure, the book contains admirable portraits of Lajos Batthyány, Arthur Görgey, György Klapka, Józef Bem, and Henryk Dembiński. Schwarzenberg and Windischgrätz receive shrewd appraisals, and Kossuth's wife, almost a Lady Macbeth in some accounts, emerges rather as a spinsterish and haughty person whose love for show gave rise to exaggerations of her influence over her husband.

Deak is sure that Kossuth fostered the "great crisis of the Empire" by his speech of March 3, 1848 (p. 62). The author argues that Kossuth was a dictator for only part of the period that ended with his exile and gives ample indication of the extent of his powers and of his encounters with Görgey. The challenges facing the Hungarian cabinet were the "Habsburg problem, the Croatian problem, the Jewish problem, the peasant problem, the worker problem, the radical problem, and the nationality problem" (p. 103). The author deals with the inevitable confusions of the time adroitly; his treatment of the military campaigns is condensed and yet quite adequate.

Deak does not hesitate to offer judgments, such as the following. The feudal diet of 1848 did more for social and economic reform than did the new House of Representatives, and it created voting provisions that were very progressive in the context of existing electoral arrangements elsewhere. Today it is impossible, as it was for Kossuth, to penetrate the motivation of the nobility, and there is no evidence that Kossuth wished to go further, after April 1848, to enhance the regime's standing with the lower classes. The country never suffered from a shortage of food and eventually it possessed a first-rate *honvéd* army. The government could not have created a system of self-government that would have ensured the predominance of the non-Magyar majority, but it might have worked out a compromise with some of the nationalities. The worst of its blunders was the attempt to defeat the Rumanians. Hungary did not become a republic on April 14, 1849, nor did the overblown Declaration of Independence force Nicholas I to intervene. The tsar worried over subversion everywhere, but he was only secondarily concerned with the chances of an explosion in Russian Poland. Moreover, his forces under Paskievich were rather lethargic, thanks to cholera and a rather surprising sympathy for the Magyars. Haynau and the Austrian soldiers were far more effective. The Hungarian nobles began to desert the cause when the deputies cut the ties with the Habsburgs; after May 1849, all classes realized that the war was lost and acted accordingly.

Excellent maps and illustrations complement this fast-paced and well-organized study.

WILLIAM A. JENKS
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JANOS DECSY. *Prime Minister Gyula Andrássy's Influence on Habsburg Foreign Policy: During the Franco-German War of 1870-1871*. (East European Monographs, number 52; Brooklyn College Studies on Society in Change, number 8.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1979. Pp. xviii, 177. \$12.00.

János Decsy's study of Count Gyula Andrássy's views of and influence on Habsburg foreign policy while he was the first prime minister of the resurrected Hungarian state from 1867 until 1871 is the latest of a wave of monographs on the makers of nineteenth-century Hungarian history. The wave began with George Barany's *Stephen Széchenyi and the Awakening of Hungarian Nationalism* (1968) and continued with Paul Bödy's *Joseph Eötvös and the Modernization of Hungary* (1972), Béla Király's *Ferenc Deák* (1975), and Istvan Deák's recently published *Lawful Revolution: Louis Kossuth and the Hungarians, 1848-1849* (1979).

Decsy focuses on Andrássy's efforts as prime minister to protect the Hungarian state created by the Compromise of 1867 against the revanchist policy of Emperor Franz Joseph, the court party, and Foreign Minister Count Ferdinand Beust, a policy that threatened the stability of the dualistic system by seeking to reverse the results of the battle of Königgrätz. After 1867, foreign policy became domestic policy for Andrássy. The preservation of the Dual Monarchy as a great power was necessary for the survival of the Hungarian state, but only if the monarchy's sphere of interest as a great power was shifted from Germany to the East. As prime minister, Andrássy already pursued the policy that became the leitmotif of his career as foreign minister, opposition to Russia and alliance with Germany. This led to Andrássy's opposition to all of Beust's plans for an Austro-French alliance directed against Prussia before 1870 and to his advocacy of simple neutrality at the famous meeting of the ministerial council of July 18, 1870 held to decide the Dual Monarchy's policy in the imminent war between France and Prussia. That Andrássy's point of view prevailed against the armed neutrality desired by Beust and the immediate mobilization and intervention urged by the court party is a measure of the influence that Andrássy had acquired and that allowed Hungary to veto foreign policy objectives inimical to Hungarian national interests as perceived by the Magyar ruling oligarchy.

Decsy's story is a familiar one, but his multi-archival research in Austrian, Hungarian, and German archives and his use of a large amount of published material (profusely attested to by forty-five pages of footnotes and an exhaustive bibliography) lend depth to the developments he describes and restores to them the complexity that has been

smoothed over in numerous retrospective historical accounts. With regard to the latter, the author comments critically on previous historiographical interpretations of the Andrassy-Beust conflict. On the whole, Decsy's judgments and conclusions are fair-minded and judicious. He shows that Beust was neither unqualifiedly revanchist nor uninterested in the East and that Andrassy was neither unconditionally pro-Prussian nor neutralist in the summer of 1870. The viewpoints of both men were characterized by inconsistencies and contradictions as they sought to influence each other and master a complex and fluid situation. One of the chief values of Decsy's monograph is the link he establishes between domestic structure and foreign policy. Unquestionably, Andrassy was an adroit politician, but his victory over Beust was as much a result of the inherent logic of dualism as of his political ability. Decsy's early chapters provide some interesting insights into Andrassy's transition from a Hungarian freedom fighter against the Habsburg dynasty in 1848-49 to a pillar of the status quo as Hungarian prime minister and Habsburg foreign minister.

Decsy ends his study on an ambiguous note. He clearly has a high opinion of Andrassy as an "aggressive, self-confident and flexible" statesman (p. 115) who ably defended Hungarian national interests. At the same time, the future foreign minister's greatest achievements masked "the tragic premonition of a not too distant future" (p. 116). One senses here some deeper problems of Hungarian and East Central European liberalism and modernization, which, it may be hoped, Decsy will explore further, perhaps in a biography of Andrassy. A modern biography of the statesman in a Western language is badly needed and the book under review shows that he is well equipped for the task.

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LADISLAV LIPSCHER. *Verfassung und politische Verwaltung in der Tschechoslowakei, 1918-1939*. (Veröffentlichungen des Collegium Carolinum, number 34.) Munich: R. Oldenbourg Verlag, 1979. Pp. 209. DM 55.

In this specialized monograph appearing in a series published by the Collegium Carolinum in Munich, Ladislav Lipscher studies the constitutional and political development of Czechoslovakia from 1918 to 1939. Lipscher's work consists of six chapters covering the construction of the state organization and varied aspects of the Czechoslovak constitutional and legal system. In addition to studying the formal political structure of Czechoslovakia, Lipscher devotes considerable attention to political parties and to individuals, especially President T. G. Masaryk.

Lipscher commands a firm grasp of his difficult subject, but one gains the impression that he has

not always organized his materials as thoughtfully as he might have. One value of his work lies in his stress on comparisons between Czechoslovakia and other Eastern European states and on the legal continuities between Austria-Hungary and Czechoslovakia; but, at the same time, he overemphasizes some of the developments of the postwar state. Does it make sense to talk about the framing of the Czechoslovak constitution without paying detailed attention to the ideas generated by the exile and home resistance movements and to particular issues like the controversial Pittsburgh agreement, which Lipscher mentions all too briefly in his text? Lipscher also examines extensively Masaryk's handling of the office of the president, while devoting too little attention to Beneš's tenure in that post. Many of the problems Lipscher has faced probably stem from the difficulty of integrating a discussion of the formal structure of the constitution and laws with a study of the political realities that shaped this structure and gave it meaning.

Despite the impressive documentation of this work, Lipscher has also probably tried to incorporate too much stray detail. There are numerous instances in areas with which I am familiar in which the text could have been clarified or improved. Josef Scheiner should not be described as a "professional" military man. Likewise, in note 590, General Syrový is identified as an opponent of Beneš; but in note 437, he is associated with that strange metaphysical animal, the "Castle," of which Beneš was presumably an integral part. In more important issues, one finds, for example, a discussion of the 1936 Law for the Defense of the State without relation to its creation of a frontier zone, surely one of its most noteworthy sections and one with serious constitutional implications. The Agrarians Udržal and Viškovský are identified as belonging to the "Castle Wing" of the Agrarian Party, but this "Castle" designation represents almost a *reductio ad absurdum* of what might be termed German Castle theory (*Burgtheorie*). At one point in 1927, Udržal, who had no love for the legionnaires, maneuvered Masaryk into appointing an inspector general of the army whom Masaryk vehemently opposed. Many of these matters, both large and small, might have been eliminated if Lipscher had shifted his focus to a more chronological and less theoretical account of Czechoslovak political developments. This would have helped him to fit together the details of his narrative more carefully than he has done.

Specialists may nonetheless find Lipscher's study a useful reference work. Its extensive documentation supplements existing historiography with helpful discussions of interwar Czechoslovak politics, political figures, and legal developments.

JONATHAN ZORACH

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JANUSZ TAZBIR. *Piotr Skarga: Szermierz kontrreformacji* [Piotr Skarga: Champion of the Counter Reformation]. Warsaw: Wiedza Powszechna. 1978. Pp. 343. 60 Zł.

The Jesuit Piotr Skarga (1536–1612), court preacher to Sigismund III, was one of the great figures of the Polish Counter Reformation. "The chief troublemaker of the Commonwealth" he was called by the Protestant and anti-Jesuit Catholics of the Zbrzydowski uprising against Sigismund III's regime in 1606. It is arguable that his *Lives of the Saints*, first published in 1579 and frequently reprinted, was the most influential book of Polish baroque Catholicism. In the late eighteenth century Skarga's splendid jeremiads, the *Sejm Sermons* (1597), were elevated to prophecy of the partitions. Liberals and Marxists have concurred in making Skarga the moving spirit of the "Catholic reaction," the advocate of persecution, obscurantism, and absolutism.

In 1962, Janusz Tazbir, one of the most prolific Polish early modernists, published a biography of Skarga that dwelt long and censoriously on the Jesuit's central role in the "Catholic reaction." No startling revelations are offered by this new biography (little, it seems, remains to be discovered in the sources), but the tone has softened. The partisan expression "Catholic reaction" rarely appears; instead, the reader is permitted to see the late sixteenth-century revival of Polish Catholicism as an authentic cultural experience, organically related to the Polish Renaissance. One of Tazbir's most effective points is in establishing close parallels between Skarga's social views and those of his heretical opponents. Drawing upon recent Western treatments of Reformation social policy, Tazbir sympathetically discusses Jesuit efforts to organize urban philanthropy, lessen exploitation of the peasantry, and deal rationally with usury.

The old Skarga is still here: the enemy of the Warsaw Confederation who repeatedly threatened dissenters with mob violence, helped force the Orthodox into the Union of Brest, and inveighed against the szlachta's privileges. But Tazbir's restraint permits a realistic appraisal of the Jesuit's attitudes and tactics. Of petty gentry origin, Skarga understood well the gentry temperament and avoided blundering into the pro-Habsburg camp, a mistake that caused other Polish Jesuits considerable political difficulty. Far from championing absolutism (a political system about whose reality in sixteenth- and seventeenth-century Europe Tazbir is skeptical), Skarga advocated reviving fifteenth-century "mixed monarchy," allotting a diminished but not insignificant role to a chastened, law-abiding gentry. Such medicine for the fevers of "gentry democracy" was prescribed by most political writers in Renaissance Poland, regardless of con-

fession. Tazbir skillfully analyzes Skarga's relations with Chancellor Jan Zamoyski and Sigismund III, showing that the king was hardly the Jesuits' puppet and that Skarga adroitly mediated between the court and Zamoyski's opposition. As for the Jesuits' attack on religious toleration, Tazbir holds that Skarga sought the conversion, not the extermination, of dissenters and schismatics and that he was remarkably effective in winning converts.

Tazbir's new biography is an important addition to the literature, full of suggestive insights into the *mentalité* of Counter Reformation Poland. It is heartening that this re-evaluation of the making of Poland's Catholic tradition was written by a prominent Marxist historian and published in a series meant for the nonprofessional public.

JAMES MILLER

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RICHARD A. WOYTAK. *On the Border of War and Peace: Polish Intelligence and Diplomacy in 1937–1939 and the Origins of the Ultra Secret*. (East European Monographs, number 49.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1979. Pp. 141. \$11.00.

This is a short book with a lengthy title on an intriguing topic. The reader's curiosity is whetted by the presumed correlation between the tortuous diplomacy of Poland on the eve of World War II and the work of its intelligence service. In recent years this service has attracted the interest of historians because of a unique asset at the disposal of the Second Department of the Polish General Staff. Known by the name of "Enigma," it was a machine enabling the Poles to decode German ciphers used in radio communications. "Enigma," handed over to the British in 1939, has been recognized as a major contribution to the war effort and is the subject of a number of books. Its story is also a part of this study, broader in scope, by Richard A. Woytak.

What Woytak terms the border of war and peace is the borderline separating Poland from Germany, which grew alarmingly in length with Hitler's bloodless conquests. The book covers the two last years of peace starting with the Anschluss. As the fall of Austria endangered Czechoslovakia, the Polish assumption was that the Czechs would not fight for the integrity of their state's territory and that Poland, itself faced with a growing threat, should secure its interests in the forthcoming crisis. Following the Munich Conference, which disregarded Polish claims to Teschen, Warsaw presented Prague with an ultimatum demanding the cession of the disputed area. In this manner the solution of the Teschen question was achieved, but it did not end Poland's involvement in what remained of Czechoslovakia after Munich. This book on the politics of

the borderlands brings into focus new Polish concerns with developments across the Carpathian range. Unlike other authors, Woytak emphasizes not so much the significance of Slovakia, but rather of Carpatho-Ukraine, an autonomous state that was becoming a center of Ukrainian nationalism. Polish intelligence was soon engaged in subversive activity to assist the Hungarians in their schemes to annex this territory. Hungary was prevented by the Germans from making this move until the final dismemberment of Czechoslovakia, an event that led to the strategic encirclement of Poland.

Despite its title, Woytak's book turns out to deal more with diplomacy than with intelligence, and the connection between the two is not clearly apparent. In effect there are distinct narratives of each. For all the advantages of possessing "Enigma," it does not appear that Poland's diplomacy was to any unusual degree influenced by the achievements of her secret services. In any case, the Germans in September 1939 modified their encoding equipment and the resulting difficulty for the Polish experts was not overcome during the remaining year of peace.

Woytak makes use of an impressive quantity of primary sources and interviews with surviving intelligence officers. Yet, his book may well serve as a reminder of the pitfalls of avoiding comment and interpretation. Stylistically the flow of narration is frequently broken by unexpected twists and switches from one theme to another, while some points seem blown out of proportion. In tune with his strictly factual approach the book ends with no conclusions—instead there is an extensive bibliography. Despite its flaws of structure and style it remains a thoroughly researched contribution on lesser known aspects of Poland's foreign policy in these crucial years.

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R. G. SKRYNNIKOV. *Boris Godunov*. (Stranitsy istorii nashei Rodiny.) Moscow: Izdatel'stvo "Nauka." 1978. Pp. 192. 70 k.

R. G. Skrynnikov's latest work is the best popular book on Boris Godunov since Platonov's classic. In a number of ways, Skrynnikov's approach resembles that of his predecessor. Like Platonov, he presents a picture of Godunov far more favorable than the one immortalized in Pushkin's play and Mussorgsky's opera. Boris, Platonov and Skrynnikov argue, was an intelligent and humane ruler and an attractive human being. He was in no way responsible for the death of the Tsarevich Dmitrii, in fact the victim of an accident during an epileptic seizure. Moreover, they argue, his government worked hard and with

considerable skill to solve Russia's pressing social, political, and diplomatic problems. The regime collapsed under the battering of forces that no ruler of the early seventeenth century could have fought—massive famine and the resulting explosion of social unrest.

Despite the similarity of some of his ideas to those of Platonov, Skrynnikov speaks with his own distinct voice. Like all of his work, *Boris Godunov* offers the reader the fruits of his masterful study of the sources. The author combines new archival material with a careful and imaginative reading of well-known texts to shed new light on a complex and poorly documented period. His analysis of the sources on Godunov's election as tsar and his unravelling of the stories on the early life of Grishka Otrep'ev, the False Dmitrii, for example, are masterful. On other subjects, such as Godunov's legislation on the peasantry, Skrynnikov has left the study of sources to others but presents their conclusions to the reader clearly and critically.

Skrynnikov's work, moreover, shows admirable sensitivity to the complexities of Russian political and social life. He eschews simple sociological statements, rightly criticizing Platonov for claiming that Godunov's policies consistently favored the provincial nobility at the expense of the traditional aristocracy. Social reality and the tsar's attempts to shape it were both much more complex, Skrynnikov insists.

On the negative side, Skrynnikov's book lacks the literary and interpretive strength of its predecessor. Like the author's earlier works, *Boris Godunov* resembles a late medieval chronicle. Each event or problem confronts the reader in strictly chronological order as a discrete entity. As a consequence, it is difficult to see the author's overarching scheme of interpretation. When Skrynnikov offers a general explanation of events, moreover, he all too often descends to the level of truism. Almost every government action of the period, for example, is presented as a response to incipient popular revolt. Personalities, too, tend to get lost in the march of events. Skrynnikov has many interesting things to say about Godunov's personality and political style, but he makes his comments in a series of digressions from his narrative rather than in a sustained portrait.

On balance, the strengths of *Boris Godunov* outweigh its shortcomings. The high quality of Skrynnikov's treatment of sources and the good sense of many of his judgments make this book a valuable addition to the literature on Muscovite history. The proposed English translation will be most welcome.

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T. S. MAMSIK. *Pobegi kak sotsial'noe iavlenie: Pripisnaia derevnia Zapadnoi Sibiri v 40–90-e gody XVIII v.* [Flights as a Social Phenomenon: The Attached Village of Western Siberia in the 1740s to 1790s]. Novosibirsk: Izdatel'stvo "Nauka," Sibirskoe otделение. 1978. Pp. 203. 1 r. 10 k.

This work has the potential of being a good monograph. It is, however, marred by the wholesale inclusion, without attribution, of a portion of a previously published work by the same author, T. S. Mamsik. The passage (a detailed discussion of the *kamenshchiki*—peasants living on the fringes of Russian authority in the Altai Mountains) is from her article in *Iz istorii sem'i i byta Sibirskogo krest'ianstva v XVII-nachale XX v.* (1975). There are some differences in phraseology between the two.

This problem aside, the work under review deserves notice as a well-researched study of an often-mentioned but little-examined phenomenon in Siberian history—peasants and others who left their registered places of residence to escape feudal obligations. The study is limited to the area of villages attached to the state-owned Kolyvano-Voskresenskii works in the southern part of the Ob and Irtysh valleys between 1748 and 1796. After the traditional historiographical introduction, the book is unevenly divided into three chapters. The first, and the longest, is a detailed analysis of 2,619 cases of flight (*pobeg*). The material was gathered by painstaking research in both central and *oblast'* archives, primarily from court records. The escapees examined include the local attached peasants (*pripisnye krest'iane*), nonagricultural personnel of the works (*masterooye*), and new arrivals (*prishlye*)—escapees from outside the area but caught within it. The latter constituted less than a third of the total and their numbers significantly decreased over the period. After an examination of the social status of the escapees, reasons for flight, and place of escape, the author turns to an analysis of the means of subsistence of the fugitives while they were "in flight." The sources allow that information to be substantiated in 20 percent of the cases.

In the last two chapters Mamsik attempts to set these fugitives in their historical context. The second chapter is a review of the empire-wide policy against flight and that policy's application in the studied area. Because of the vast territory involved and of the need for a population to develop the area, policy there could not be a replica of that in European Russia. But Siberian officials still faced problems of control and increasingly shifted enforcement measures to the *obshchiny* of the attached villages. A "circular (collective) guarantee" was used to assure the Kolyvano-Voskresenskii works of needed labor. In the third chapter, Mamsik examines the relationship of the peasantry of the villages

to the escapees and finds that the *obshchiny* actually encouraged flight. Behind this lay several reasons: kinship and personal bonds, the folk ethic of helping "runners," an ethic not unconnected to religious motives, and the development of economic ties between the villagers and the escapees. Here she argues that flight became a "social phenomenon." When escapes so depleted the work force that the "circular guarantee" placed excessive demands on the peasants, group devices such as refusals to work and petitions were used. The peasants' attempts to establish work norms based on a "single soul" support this argument. The administration responded with repression. The author's point is that the individualistic act of protest against the feudal order, flight, was transformed into a "social (class) phenomenon."

The argument has some merit. Certainly the peculiarities of the Siberian situation made for an alteration of national policy, and the presence of kinship and personal ties strengthened the *obshchina* against the bureaucracy. The emphasis on the economic relations between the productively engaged escapees, the majority of those whose means of subsistence could be established, and the village is well taken and suggestive. To make an individualistic act an economically determined part of the class struggle seems to stretch the point, however.

Thus the core of the work contributes to the historiography of some aspects of Siberian history—the illegal movement of the nonprivileged sections of society within and into Siberia, their role in the colonization of the area, and their relationship to the slowly developing bureaucracy, as well as to the settled village *obshchina*. The use of an extensive section of previously published prose is misleading, however. A short summary would have sufficed, and attribution is certainly in order, even when the material is from the same author.

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JOHN SHELTON CURTISS. *Russia's Crimean War*. Durham, N.C.: Duke University Press. 1979. Pp. xi, 597. \$29.75.

John Shelton Curtiss rightly anticipates objections from diplomatic historians to certain aspects of this work. Some problems are almost unavoidable in doing a narrative history of such well-trodden ground as this. Well-known events are recounted and familiar diplomatic exchanges summarized, largely from standard published sources. There are mistakes and dubious interpretations as well, due either to overlooking some of the vast literature or relying uncritically on Russian sources. For example, both the Anglo-French-Austrian alliance of

December 1854 and the peace feelers and moves in the fall of 1855 are explained on the basis of Russian versions of events long since refuted from other sources.

But the book should not be judged mainly on such details. Even its diplomatic history sections, the least successful aspect, present Russia's side of the story clearly and convincingly, on the whole. Curtiss's view that the Western powers were more responsible than Russia for the outbreak and prolongation of the war is certainly defensible, and by drawing on Russian archival materials he sheds some new light on Russian attitudes, especially during the peace moves and negotiations of 1855–56. The discussions of military events are very effective both as narrative and analysis, demonstrating Curtiss's expertise as a military historian and his considerable descriptive powers. The final chapters, though presenting no new ideas on the results of the war, convey a vivid impression of its impact on the Russian people and its leaders.

As for Curtiss's conclusions, they are generally reasonable, but perhaps do not go deep enough. For instance, he can plausibly argue that in 1853 Russia was relatively innocent of aggressive intent and represented no overt threat to Turkey's integrity or the European balance. He can even claim (p. 35) that Britain and France had been more expansionist in 1815–50 than Russia (though one is astonished to learn that Austria and the United States were also acquiring colonial empires at this time). But even if Russia mainly wanted to restore and guarantee her previous relation with Turkey, disturbed by France, and even if Nicholas's deepest motive for seeking influence over Balkan Christians was to repress and control revolution (p. 168), the point remains that the status quo ante, in Russian eyes, meant a strong Russia dominating a weak Turkey and holding the threat of Christian insurrection constantly in its hands. Such a status quo was tolerable only so long as it remained unacknowledged, informal. The very attempt to formalize and guarantee it, however intended, was implicitly aggressive, destroying the necessary fiction of Turkish independence and challenging the principle of joint European jurisdiction over the Near East.

While engaging in counterfactual arguments (itself a useful device), Curtiss also sometimes attaches too much significance to contingent events. He argues, for example, that by better strategy Russia could have won the Battle of Inkerman and, thereby, the war. The battle, yes; but the war? Were Britain and France more likely to make peace after a military disaster or to try other and more revolutionary means to wage war? He also suggests that a Russian victory in the war might have prevented the reforms of Alexander II—again, a too sweeping conclusion. The very fact of the war, not simply its

outcome, demonstrated the failure of Nicholas's system and the need to strengthen and modernize Russia to meet the challenge of a hostile Europe.

But these are points to debate, not charges leveled against the book. It remains commendable as a clear, sympathetic, and, especially on the military aspects, interesting portrayal of the Russian side of the war.

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B. V. TIKHONOV. *Pereseleniia v Rossii vo vtoroi polovine XIX v.: Po materialam perepisi 1897 g. i pasportnoi statistiki* [Migrations in Russia in the Second Half of the Nineteenth Century: Based on the Census of 1897 and Passport Statistics]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 208. 1 r. 60 k.

This book's title refers to migrations, and its author carefully delineates the diverse forms and directions of internal migration in prerevolutionary Russia. Using mainly published sources (especially the 1897 census), B. V. Tikhonov examines the origins, destinations, motives, and terms of settlement of various categories of migrants.

For the most part, migration followed a few well-worn channels: to Moscow and Saint Petersburg, to the southern and southeastern provinces, and to Siberia. Tikhonov presents not only the aggregate totals of each province's in- and out-migration, but also the precise origin and destination of each major stream of migrants. In the first of three major chapters, he identifies 276 pairs of provinces that together accounted for over half of all interprovincial migration. The second chapter offers a similar analysis of labor migration; this discussion is hampered, however, by the unsatisfactory categories used by the census-takers to describe the wage-earning population. The third chapter uses a different body of evidence—short- and long-term internal passports issued to workers—to illuminate the connection between temporary labor migration (*otkhodnichestvo*) and permanent resettlement.

Although the author, especially in chapter 3, shows a keen awareness of the subtleties of migrants' life patterns, he has an unfortunate tendency to oversimplify the causes and results of migration. For him, *otkhodnichestvo* is mainly a prelude to permanent out-migration from the peasant villages. Agricultural colonization reflects a broadening of capitalist relations, while nonagricultural migration is a sign of their deepening. The various forms and directions of migration, in other words, are presented as essentially complementary, but to this reviewer the evidence seems unconvincing.

Tikhonov asserts that poor peasants predomi-

nated among migrants but offers little solid evidence. Several recent studies have suggested an opposite conclusion: that migration may have attracted a middle stratum of more productive and innovative peasants. This is an issue worthy of close, careful consideration, but it will not be resolved by aggregate census statistics or passport records.

Another issue that deserves scrutiny is migration's effects on the provinces of heavy out-migration. *Otkhodnichestvo* in particular tended to leave agriculture in the hands of the old, the very young, and the unfit, and migrants' earnings were often sent home to prop up decrepit and unproductive agricultural households. Did this really strengthen capitalism's hold on the countryside? One might conclude that contradictory processes were at work, and that capitalist relations advanced in some regions at the expense of others. Tikhonov, however, barely mentions this possibility.

Using the same sources, Barbara Anderson, Daniel Brower, and other non-Soviet scholars have asked new questions, testing their hypotheses with advanced statistical methods. One can only wish that there could be a closer exchange of ideas between Soviet and Western colleagues.

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LEOPOLD H. HAIMSON, editor. *The Politics of Rural Russia, 1905-1914*. (Studies of the Russian Institute, Columbia University.) Bloomington: Indiana University Press. 1979. Pp. x, 309. \$19.50.

This admirable collection of well-coordinated essays describes and analyzes in detail the political processes of rural Russia in the last twelve years of the Old Regime. The common concern of these essays is the impact of the nobility on Russian politics and the chasm of political cultures between the nobility and peasantry and between rural and urban elements. Their common thesis holds that the obliviousness of the "Janus-faced," estate-service nobility, entrenched in Stolypin's "June Third" system, to the clamor for change is the key to an understanding of the character of the politics of the period and the dynamics of the revolution that culminated in 1917. The studies hold that the resilience of this system lay in the mutual dependence of the government and a nobility vigorously defending its privileged position against attacks from both above and below. They also maintain that contemporary and later generations of analysts of the period have been overly influenced by the assumptions of the intellectual, urban-oriented leadership of the liberation movement who identified its politics and purposes with those of all Russia. With no response

from the countryside to their programs, they could not conceive that rural politics could affect or explain Russia's political destiny.

Caught up briefly in the constitutional movement under the impact of the regime's poor showing against Japan and disgruntled by its new emphasis on industrialization, the nobles were quickly disenchanted by liberal and leftist demands that they yield some or all of their property to the peasantry and by peasant violence against that property. They swung sharply rightward to dominate the zemstvos and to disavow both the Kadets and the flabby, moderately constitutionalist Octobrists, who were trying to bridge the rural-urban gap and were standing on the dangerous ground of diluted expropriation. They merged their efforts in a nationwide organization, the United Nobility, or regionally in the southwestern Russian Nationalist Party. Working in tandem with the State Council, the upper house in parliament, to ward off bureaucratic threats to their control over local self-government, they were significantly instrumental in the promulgation of Stolypin's electoral law of June 3, 1907. They held that their cause represented the interests of the nation as a whole.

From the above it is apparent that in the long perspective since the end of the fifteenth century, at least, the position of the nobility has evolved along much the same lines as in a developing society. It remained essentially an estate-service element during the parliamentary period. State and estate depended upon each other as they had in the acute (sixteenth-century) and chronic (eighteenth-century) crises. Constantly threatened by bureaucracy and peasantry, the nobility had banded together to protect its interests. The nobles had countered the state when it failed militarily and proved incapable of maintaining internal order.

In this connection, the charge that historians, especially in the postwar period, have concentrated narrowly on the operations of the State Duma is somewhat oversimplified. They had to present a concrete picture of this institution in a new area of objective research: the structure, jurisdiction, and limitations imposed on it by a fretting regime that obviously rested on the bureaucracy and nobility. This is significant for an appraisal of the viability of a representative institution in a society with a six-century statist tradition. The regime had dealt with the oppositional zemstvos and State Dumas in much the same restrictive manner. When significant, the general nature of the pressures on the State Duma have been duly noted—as with the western zemstvo and education bills. The intricacies of the development of these pressures are the domain of the specialists. It is important to understand who challenged the *zuby*, and why, in their declining years. After all, there was a Progressive

Bloc and the emperor's last desperate concession was a "duma ministry."

ALFRED LEVIN
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JOHANN H. HARTL. *Die Interessenvertretungen der Industriellen in Russland, 1905-1914*. (Wiener Archiv für Geschichte des Slawentums und Osteuropas, number 10.) Vienna: Hermann Böhlau Nachf. 1978. Pp. 135. DM 60.

A notable feature of advanced capitalist development has been the movement of businessmen to form specialized institutions to promote and defend their interests. In Russia, limited attempts in this direction date mainly from the 1880s. But the most visible efforts grew out of a response to the Revolution of 1905 and continued into 1917. The stated goal of Johann H. Hartl's study is to provide an overview of the business association movement in Russia, while the unstated purpose is to throw more light on the chronic weakness of the Russian bourgeoisie.

Beginning with a quick glance at the theory of business organizations as pressure groups, the author briefly examines Russian industrialization and entrepreneurial federations before 1905. Following received opinion, Hartl stresses the state's initiating role in the industrialization process and the inter-related deficiency of private enterprise. Correspondingly, despite some relaxation in the Witte era, the state gave local exchange committees and regional industrial federations limited autonomy and often used them as levers for control and policy coordination. The annual meetings of a given association and the actions of its executive committee had real value for industrialists, however. They provided a channel of communications through which petitions to the government could be forwarded and pressure on decision-makers sometimes brought to bear.

According to Hartl, associations of industrialists continued after 1905 to concentrate on informal lobbying and pressuring within the bureaucracy. There the views of business were routinely heard and almost as routinely ignored. Not that business planned it that way. In feverish 1905, leading industrialists who were suspicious of the Kadets tried to build a political party to speak for "the commercial-industrial class," but the "party of plutocrats" shriveled in the heat of electoral competition. Industrialists exercised minor political influence in the Duma or the State Council between 1906 and 1914, even on questions directly affecting their interests.

In his longest and most original chapter, Hartl constructs a typology of business associations that is

combined with some representative case studies. The focus on structure and internal organization shows how weighted voting systems favoring the wealthiest industrialists were partially counterbalanced by permanent executive committees, quasi-independent statistical sections, and the need to attract less affluent members. The empire-wide Association of Industry and Trade, founded in 1906 to tie together regional and functional federations, and the Association of Mineowners of Southern Russia (1874), which served as the model for a host of followers, receive detailed treatment. Finally, the activities of employers' unions, first formed in 1905 to break strikes and coordinate labor discipline, are illustrated by a study of the Society of Moscow Manufacturers.

This is a modest but useful book, based on printed sources. In its careful attention to organizational structure and typology, it compliments the work of Ruth Roosa on the economic ideology of Russian industrialists. The specialist will find the case studies particularly revealing. And if the weakness of the Russian middle class remains enigmatic, there are some interesting hints for further research on relations between business and government.

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JOHN E. BOWLT. *The Silver Age: Russian Art of the Early Twentieth Century and the "World of Art" Group*. (Oriental Research Partners Studies in Russian Art History.) Newtonville, Mass.: Oriental Research Partners. 1979. Pp. 355. \$16.00.

The Silver Age is mistitled in that it implies a broad survey of cultural and intellectual history for Russia prior to 1914. Instead, this volume simply provides a useful narrative description of the World of Art Society (*Mir iskusstva*), which flourished in the 1898-1906 period and then dispersed into emigration and divergent individual paths inside Russia. John E. Bowlt is a well-known critic, publicist, and cataloguer of Russian art in America.

Bowl't's book contains two background chapters on Russian "realism" and "neo-nationalism" before 1900, five chapters on the World of Art's journal, art exhibits, and esthetic views; and seven mini-biographies of Sergei Diaghilev and other artistic luminaries of the society. It features numerous black-and-white illustrations and is based primarily upon recent Soviet and Western secondary sources.

Unfortunately, the book is marred by overly long descriptive passages and the absence of any explicit argument, method, or analytic framework. If there is a conclusion, it is that the World of Art had "historical and historic importance . . . in both 'calen-

dar' time and 'musical' time" (p. 271). There are frequent long lists of proper names followed by "etc." or "et al." and jargon descriptions of paintings in terms of their "energy," "musicality," or "ascetic tone." Alexander I is said to have liberated the serfs in the 1860s (p. 29). There is little attention to the broader cultural and intellectual trends of the day (especially theosophy), and European influence is consistently underestimated or ignored. Despite these shortcomings, this book remains a solid description of the World of Art for the English-language reader unable to read N. Sokolova's *Mir iskusstva* (1934) or N. Lapshina's *Mir iskusstva* (1977). It is a useful contribution to Russian art criticism.

ROBERT C. WILLIAMS
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GERT VON PISTOHLKORS. *Ritterschaftliche Reformpolitik zwischen Russifizierung und Revolution: Historische Studien zum Problem der politischen Selbsteinschätzung der deutschen Oberschicht in den Ostseeprovinzen Russlands im Krisenjahr 1905*. (Göttinger Bausteine zur Geschichtswissenschaft, number 48.) Göttingen: Muster-schmidt. 1978. Pp. 273.

The four volumes concerning the Livland *Ritterschaft* (Corporation of the Nobility) and nineteenth-century Baltic agrarian legislation that Alexander von Tobien published between 1899 and 1930 have profoundly influenced Baltic German historiography. Tobien's volumes idealized the actions of the Livland nobility and glossed over certain darker sides of Baltic agrarian development during the nineteenth century. Before 1945 this view of Baltic history, which Tobien documented on the basis of extensive research in the archives of the Livland *Ritterschaft* in Riga, was widely accepted by scholars writing in German.

Among the students of the late Reinhard Wittram who have undertaken to revise Baltic historiography since World War II is Gert von Pistohlkors, the author of *Ritterschaftliche Reformpolitik zwischen Russifizierung und Revolution*. Pistohlkors corrects Tobien's interpretation of the motives and goals of the Livland *Ritterschaft* during two crucial periods of reform: first, the 1840s and 1850s and, then, the revolutionary year 1905. In Tobien's interpretation, the Baltic nobility (especially in Livland) acted to defend the interests of all the inhabitants of the three Baltic provinces by working for reform that assured economic progress and social stability in the countryside. Thus he viewed the reforms of the 1840s and 1850s as a means of encouraging the best elements among the Estonians and Latvians to own or lease land and gradually to assume a more responsible role in local self-government. In the second part of the nineteenth and early in the twentieth

century, according to Tobien, only interference by St. Petersburg prevented the Baltic nobility from introducing further reform that would have effectively protected the local population from the corrupting influence of the *chinovnik* and the Russian revolutionary movement.

Using essentially the same materials as Tobien did earlier, Pistohlkors argues that the main concern of the Baltic nobility was not to promote the welfare of the inhabitants of the three provinces but to defend their own monopoly of economic and political control over the affairs of the Baltic countryside. In the 1840s and 1850s the Baltic nobles only agreed reluctantly to minimal reform in response to widespread social and religious unrest among Estonian and Latvian peasants, pressure from the Russian government, and the prodding of a reform party led by Hamilcar Baron von Fölkersahm. In so doing, Pistohlkors points out, they stubbornly defended their own special rights and privileges and generally ignored the economic, social, and political interests of the overwhelming majority of Estonians and Latvians. By the twentieth century the Baltic Germans had few political allies locally. The last flurry of reform activity of the Livland *Ritterschaft* during the revolutionary year 1905, therefore, had little, if any, chance of success, a point Pistohlkors makes convincingly with a careful analysis of the papers of Marshal of the Nobility Friedrich von Meyendorff and of the proceedings of the special meeting of the Livland Diet held in July of 1905.

Pistohlkors relies largely on German-language materials concerning two periods of reform separated by an intervening period of almost half a century. This intervening period is also important. The policies of the Baltic German leadership and the economic and social development of the Baltic region during the 1870s, 1880s, and 1890s can now, it should be pointed out, be studied in some detail thanks to recently published Soviet Russian, Latvian, and Estonian source materials and scholarly literature. Pistohlkors's monograph remains, however, a significant step toward a thorough revision of Baltic German historiography and an improved understanding of the role played by German nobles in Baltic and Russian history at the beginning of the twentieth century.

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V. N. GINEV. *Agrarnyi vopros i melkoburzhuannye partii v Rossii v 1917 g.: K istorii bankrotstva neopopulizma* [The Agrarian Question and Petty Bourgeois Parties in Russia in 1917: Toward a History of the Bankruptcy of Neopopulism]. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otdelenie. 1977. Pp. 294. 2 r.

For many years Soviet scholars expressed their contempt for the losers in the Russian Revolution by ignoring them. In the past two decades non-Communist political movements have received a more evenhanded treatment. In the Stalin era Soviet accounts made Bolshevik victory seem too easy and inevitable, because the opposition seemed so weak and shadowy. Today scholars take their measure of the enemy, mentioning names and numbers, thereby enhancing Bolshevik stature by demonstrating the truly formidable odds against a successful Bolshevik seizure of power. Non-Communist political movements are still subsumed under the "bourgeois" label, and their programs must still be dismissed as examples of "false consciousness," or errors in logic, but it is now possible to examine the sources of their popular support and to study their organization and ideology. V. N. Ginev's book on the agrarian question is a good example of this relatively new approach.

Ginev is less concerned with the agrarian question in general than with the agrarian program of populist parties. In particular, he focuses on the interaction between the Bolsheviks and their populist allies, the Left Socialist Revolutionaries, in the development of Bolshevik and Soviet agrarian policy up through January 1918. The subtitle of the book, "Toward a History of the Bankruptcy of Neopopulism," is a more accurate description of its contents.

Although Ginev's book is a remarkably full and objective account of this subject, it contains no new revelations and is based entirely upon published sources and Soviet secondary works. It ignores Western research on the subject, like the books by Oliver Radkey and Maureen Perrie. Two features of the book are noteworthy. First, although the author's conclusions remain well within the canons of Communist orthodoxy, the body of the work provokes some interpretations that are not emphasized at the end. Second, he provides a candid and revealing account of the evolution of Bolshevik agrarian strategy and tactics in 1917. It is a novelty to read in a Soviet account of the First Congress of Peasant Deputies in May 1917 that "the representatives of the peasantry as a whole were not ready to adopt Leninist proposals for the public cultivation of landlord land." It is even more surprising to find a Soviet scholar who is willing to describe how Lenin shifted his tactics in response to that situation, moving away from an emphasis on the preservation of large-scale holdings to support for the redistribution and "leveling" of landholding. "Agitation in Lenin's opinion had to support immediately the concrete peasant demands contained in the theses of the First Congress of Peasant Deputies," and "the program of the peasant poor, which is expressed in these theses, could only be fulfilled by the revolu-

tionary proletariat and by its avant-garde, the Bolsheviks."

Of course, Ginev does a fast shuffle on the question of Lenin's consistency, denying that Bolshevik support for small-scale proprietorship was opportunistic or contrary to the whole spirit of Marxism and omitting, for example, Chernov's comment that "Lenin copies out our resolutions and publishes them in the form of 'decrees.'" One could easily conclude from the body of this work that Lenin owed much of his success to the failure of the populists to carry out their own program and his willingness to pick up where they left off. Lenin later admitted that he copied the agrarian program of the Socialist Revolutionaries "without a single change." But, that is not one of Ginev's conclusions. He cannot emphasize the role of chance and political intuition. In his conclusion he states that Lenin's agrarian program and his alliance with the Left Socialist Revolutionaries were a result of deep insights into the true forces of history and the "correct" solution to the problems of the Russian peasantry and, therefore, provide an example for all future Communist revolutions to follow.

There is much that is valuable in new Soviet scholarship, but, unfortunately, it must still be sandwiched between introductions and conclusions frozen in Marxist catechisms and scriptural quotations from Lenin.

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T. H. RIGBY. *Lenin's Government: Sovnarkom, 1917-1922*. (Soviet and East European Studies.) New York: Cambridge University Press. 1979. Pp. xvi, 320. \$34.50.

For two decades T. H. Rigby of the Australian National University has been one of the most prominent political scientists specializing on Soviet affairs. As the title of his last chapter, "Some Historical Reflections," suggests, however, his latest book is primarily a historical treatment. Rigby's meticulous care for chronology and documentation is evident in every chapter. When he resorts to comparison, it is usually diachronic, as in his trenchant comparison of the emergent Soviet Council of People's Commissars (Sovnarkom) to the tsarist Council of Ministers. Nevertheless, the historical treatment rests on a framework of administrative behavior, though some political scientists will wish that Rigby had drawn on a somewhat broader range of administrative theory. The limited cross-cultural comparisons he does undertake—notably between the Sovnarkom and the British cabinet—are well informed and judicious.

Rigby is not primarily concerned with demon-

strating institutional continuity between the Soviet and tsarist periods. Indeed, it would be difficult to do so in a volume focusing on the Sovnarkom, for the revolutionary regime necessarily stressed *changes* at this level. Nevertheless, Rigby demonstrates, by painstaking accumulation of biographical data, that the new masters were not very different in *family* origins (though personal experiences, especially for initial commissars, did differ drastically) from their tsarist predecessors. His findings further substantiate the elitist, unrepresentative composition of the early Soviet leadership suggested by earlier scholars.

Unlike some recent researchers on the initial years of the Soviet regime, Rigby does not hesitate to call it oligarchical. It is *within* the overall oligarchical pattern that he seeks the reasons why the Sovnarkom rather than a *Communist Party* organization occupied the key decision-making position. Since a large majority of Sovnarkom members were also top party leaders (at the start prominence in the Bolshevik Party was the prime qualification, though specialized competence *among party members* became more significant by 1921), one may ask whether the particular institutional locus of decision making was very important. Rigby suggests two interlocking reasons that explain the initial saliency of the Sovnarkom, its rapid recession after 1921, and the persisting significance of the early Sovnarkom experience.

Just as the tsarist system had posited an autocrat, Bolshevik legitimizing principles required ultimate authority to reside in the single-party leadership. Hence the Sovnarkom could never become a responsible cabinet in a parliamentary system and for long periods was relegated to routine tasks. Rigby might have strengthened this aspect of his argument by showing how Stalin, as autocrat, reverted to use of government bodies, such as the Sovnarkom, for long periods between 1939 and 1952. On the other hand, early party doctrine did not specifically stipulate a *party* body as the regular decision center. Consequently, Lenin was able to utilize the Sovnarkom apparatus as his personal staff.

Since, in a sense, Lenin *was* the legitimizing principle of the early Soviet regime, his preference automatically placed the Sovnarkom at the top of the decision-making pyramid as long as he could dominate the system. Conversely, his rapid physical decline entailed the Sovnarkom's recession. Rigby's reading of Lenin's personality leads him to regard this development as avoidable. Although an exacting "schoolmaster" in his approach to associates, Lenin welcomed other strong voices in the Sovnarkom—though in fact they were often absent. Yet Lenin was reluctant to appoint a successor as Sovnarkom chairman strong enough to maintain the influence of the government body. I certainly do

not pretend to know enough about Lenin's complex character to advance an alternative interpretation; but it has often been suggested that, expert in bureaucratic politics as Lenin was, he manipulated the system to maintain his own ultimate authority. If that is truly the case, Lenin's example more than his ambivalent and often contradictory pronouncements may have constituted his persisting formative influence on the Soviet regime.

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VOJTECH MASTNY. *Russia's Road to the Cold War: Diplomacy, Warfare, and the Politics of Communism, 1941–1945*. New York: Columbia University Press. 1979. Pp. xix, 409. \$16.95.

Vojtech Mastny's important study goes far toward filling a major gap in the literature on the origins of the Cold War. Exploiting an impressive array of Soviet and Eastern European sources, as well as recently declassified British and American documents, Mastny carefully traces the evolution of Stalin's foreign policy during World War II. He skillfully illuminates the intricate, constantly shifting relationship among the many factors shaping Stalin's calculations: military exigencies and post-war goals; relations with allies and enemies; the problems and opportunities created by Communist parties in other nations. He offers fresh and sophisticated interpretations of such controversial issues as Stalin's attitudes toward the second front and a separate peace, the diplomatic maneuvering at the various wartime conferences, and the ultimate breakdown of the alliance. Perhaps the most interesting feature of the book is its detailed treatment of Stalin's complex dealing with the various groups competing for power in Eastern Europe and his ambivalent attitudes toward Germany.

According to Mastny, Stalin blended the traditional drives of Russian imperialism with his own special brand of ruthlessness. He was the supreme opportunist, an "accomplished practitioner of the strategy of minimum and maximum aims" (p. 309). At the outset, he could think only in terms of the restoration of Russia's June 1941 frontiers. As the Soviet military position improved, his visions of conquest expanded, but even then he was primarily concerned with winning the war as quickly as possible and avoiding confrontation with his allies. His "craving for security was limitless" (p. 41), however, and when Churchill and Roosevelt appeared to acquiesce in his expansionist aims at Teheran and the Normandy landing assured victory over Germany, he set out with determination to construct his empire. He succeeded, perhaps beyond his ex-

pectations, Mastny speculates, but his short-term success ensured his ultimate failure. At first overestimating Allied compliance, when, after Yalta, the West began to oppose him, he overestimated the depth of the opposition. Panicking, he provoked the confrontation he "neither desired nor thought inevitable" (p. 309).

Mastny's larger conclusions should stir renewed controversy on still timely issues. By failing to make clear to Stalin what they would accept, he argues, Churchill and Roosevelt contributed to the Cold War, a result that might have been averted by an earlier and more clear-cut policy of resistance to Soviet expansion. Perhaps so, but Mastny himself admits the difficulties of such an approach and concedes that the question is academic. He further contends that the "Soviet system" was the "cause of the Cold War" (p. 306), a rather narrow conclusion that ignores the larger historical forces set loose by World War II and does not do justice to the subtlety of the book as a whole. Also open to debate are his assertions that American diplomatic failures were the result of "innocence and inexperience" (p. 310) and that Stalin's commitment to the Cold War was irreversibly set by the end of World War II, the latter an argument not supported by evidence or indeed by sustained discussion. These questions aside, *Russia's Road to the Cold War* is an important and challenging book, far and away the best treatment of a complex and controversial topic.

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NEAR EAST

E. ASHTOR. *A Social and Economic History of the Near East in the Middle Ages*. Berkeley and Los Angeles: University of California Press. 1976. Pp. 384. \$18.50.

Since the publication in 1969 of his huge *Histoire des prix et des salaires dans l'Orient Médiéval* and the republication by Variorum Reprints of articles that appeared originally between 1949 and 1977, E. Ashtor has been recognized as one of the pioneering investigators of the medieval Near Eastern economy. His contributions in the history of prices and wages, based upon a close familiarity with the Arabic chronicles and Geniza documents, have been among the first of the kind. Ashtor's attempt to produce the first survey in English of economic and social conditions in the Near East for a nine-hundred-year period, designed at the same time for the general reader, therefore comes as a new and, it must be said, unsuccessful departure.

Beginning with the seventh-century Muslim conquests and their effects, particularly on agriculture,

Ashtor narrates the rise and decline of Abbasid power, a decline imputed in large measure to the insurrection of the Zindj—a "decisive phase in the history of the Caliphal empire," as the author sees it. Concomitant with the decay of central authority was the development of a feudalism that henceforth was only periodically interrupted. ("Bourgeois" ascendancy during the Fatimid period is mentioned as one example.)

Numerous charts and tables are adduced in support of these and the other leading trends that Ashtor identifies, together with observations that, while likely true, are framed in such a way that their opposites cannot also be excluded. "As in any pre-capitalistic economy there occurred changes in production which increased the accumulation of capital, which means the rich became richer and the poor poorer." But certain other changes in production surely had other results. Indeed, the book is full of instances in which the rich too get poorer. The bravery, moreover, with which the author uses his often very incomplete statistics continues, as in his previous works, to be a matter for concern and surprise.

It might well be that the available sources will not allow any book to be what this one purports to be. Even a thin volume bearing such a title needs something on thought and attitudes, clothing, travel, schooling, marriage and children, buildings, science, recreations, and habits. What actually is offered is a commercial, agricultural, and monetary record of the caliphate and the successor states, introduced by summaries of the political and military context. As for "social" history, there is almost none. Moreover, the general reader to whom Ashtor addresses his book is not likely to be interested in what interests Ashtor. Exceptions to this are the political narratives that comprise by far the most engrossing, readable, and—because already available elsewhere—least needed part of the work.

Ashtor's discoveries and his obvious store of knowledge might more appropriately have been imparted in a series of articles. As for a book on this deserving subject, we must wait further.

ALBERT PERDUE
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HANNA BATATU. *The Old Social Classes and the Revolutionary Movements of Iraq: A Study of Iraq's Old Landed and Commercial Classes and of Its Communists, Ba'athists, and Free Officers*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1978. Pp. xxiv, 1, 283. \$75.00.

Most observers concerned with the seeming advance of communism in the Middle East used to hold to one or the other of two theses. To some,

communism was promoted by the existing socioeconomic structure, which made communism extremely attractive to the masses. To others, the Communists' potential lay in their tight and effective organization, which gave them great advantage over their opponents, who were numerically larger but greatly factionalized. Hanna Batatu accepts the first thesis. In the opinion of this reviewer, he shows that the correct choice is neither of the above.

In Batatu's analysis, capitalist imperialism's incorporation of Iraq into the world market set loose the socioeconomic forces that have determined Iraq's history in the last century or so. The process began with the creation of the "old Landed and Social Classes" that gained the pre-eminent position under the British mandate and dominated the Hashimite monarchy. The tribal sheik was supreme until the mid-1930s, but the last two decades of the monarchy saw the union of the sheiks and the monarchy, supported by nontribal elements, for the purpose of controlling the masses of Baghdad, which had become the major counterforce. The monarchist-sheik alliance, based on the maintenance of privilege, could not succeed. The Communists, led by middle-class elements, began organizing the masses early in the mandate and, despite repression, survived. The example of the Communists was followed by Ba'thists and Free Officers. The result was the destruction of the monarchy in 1958 and the construction of a new society.

The first third of the book deals topically with the "old classes"—the sheiks, the Ottoman official families, the merchants, and the former Sharifian officers. The second third is devoted to a detailed narrative of the activities of the Communists from the earliest efforts until 1958. The last third provides details of the events of 1958 and the following years until the 1970s. An appendix provides materials relating to contacts between Iraqis and the Bolsheviks in the 1920s. Frequent statistical and non-statistical use is made of copious socioeconomic data, including demographic, economic, and biographical. Much of the socioeconomic data and an even greater quantity of information on events is derived from unpublished records and from interviews with participants or first-hand observers. The unpublished records include British diplomatic papers, police reports (both British and Iraqi), and both Communist and Ba'thist internal documents.

Batatu's utilization of his material is as impressive as its mass. Unlike many who address great questions, he has a decent respect for facts. In exemplary fashion, he gives the evidence in detail and argues each point in turn. At the less abstract level, his generalizations are persuasive and illuminating. But, in this reviewer's judgment, his main theses are counter to his own particulars. Exploitation and repression of the masses are depicted as the main

causes of the monarchy's failure, yet the few cases of violent outbreak for which details are available are clearly related to other matters, and governments have been changed by army coup alone, not insurrection. Batatu overstates the Communists, whose leadership is shown to have been rent by rivalries, their ties to the masses of non-class nature. The party indeed has acted as "traveling companion" of elite groups, such as the *Ahali* group and Free Officers, rather than the reverse. In the case of the Portsmouth Treaty riots, Batatu slights the activities of the other opposition groups and ignores dissension within the cabinet. The statistical data for the comparative assessment of the monarchy and the revolutionary government is inadequate; others have given quite different evaluations. The statistics on land-holdings in 1958 (Tables 5-1 and 5-3) incorrectly call the holdings private holdings, but in fact they include state lands and land of unsettled title. The only certainty regarding land-holding since 1958 is that official information is confusing.

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AFRICA

GABRIEL WARBURG. *Islam, Nationalism, and Communism in a Traditional Society: The Case of Sudan*. London: Frank Cass; distributed by Biblio Distribution Centre, Totowa, N.J. 1978. Pp. xi, 253. \$22.50.

This book consists of four essays that revolve around the theme of the impact of Islam upon Sudanese society and politics. They are revised and enlarged from previously published papers. All are concerned with the pervasive power of sectarian Islam in the Sudan, which undermined or overwhelmed, depending upon the situation, the Sudanese intelligentsia, represented either by the members of the Graduates' Congress or the Communist Party, who sought unsuccessfully to become independent from the religious sects.

Gabriel Warburg's first essay, "From Ansar to Umma, 1914-1945," describes how the Ansar, under the surviving members of the Mahdi, emerged from defeat in 1898 and constant surveillance by the British intelligence department from that date on, to become a powerful political force after the First World War. This essay is a brilliant analysis of that transformation in which the relations and interaction between the leader of the Ansar, Sayyid Abd al-Rahman, the government, and the leader of the rival Khatmiyya sect, Sayyid Ali al-Mirghani, are intimately woven against the background of Anglo-Egyptian rivalry. Warburg's two main conclusions concern the strength and flexibility of pop-

ular Islam and the inability of the authorities to understand the power of popular Islam that led to their failure to formulate policies to deal with it. Instead, the British expended their efforts to encourage the growth of nonsectarian Sudanese leadership, a tactic that in the end proved futile. Although he recognizes the British failure, the author does not describe what alternative policies the British might have employed; but his analysis of these themes in the preindependence period in the Sudan are accomplished with penetrating power and persuasiveness.

The second essay is concerned with Ismail al-Azhari and the struggle for independence. Warburg sees al-Azhari as a symbol of the failure of the intelligentsia to break loose from the religious grip of the Sayyids and be successful secular nationalists. His conclusion that the Ansar and the Khatmiyya were always behind the scenes in the crucial decisions of the nationalists is as perceptive as it is irrefutable.

The second part of the volume consists of two essays, one on the rise and fall of the Sudanese Communist Party, the other on four aspects of Sudanese Communist ideology and practice. Here the author analyzes the role of the Communists in Sudan politics and their attitude toward Egypt, Arab unity, Islam, and military coups. Unlike the first two essays that are based on extensive documentary sources, many of them secret British intelligence reports, the essays on the Sudanese Communist Party are the work of a historian-detective coming to conclusions, undoubtedly sound, on the basis of public documents. But the information simply is not available to expose the internal workings and personalities of the Sudanese Communist Party.

As a package, these four essays are an important and scintillating analysis of Islam, nationalism, and communism in the Sudan, completed by a final chapter of five documents published by the Sudan Communist Party during the crucial years 1970-71.

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BARBARA E. HARRELL-BOND *et al.* *Community Leadership and the Transformation of Freetown (1801-1976)*. (Change and Continuity in Africa, Monographs under the Auspices of the Afrika-Studiecentrum, Leiden.) The Hague: Mouton; distributed by Walter de Gruyter, New York. 1978. Pp. xxi, 416. DM 68.

In many African cities, the day-to-day authority that most of the inhabitants perceive is provided not by municipal institutions but by local community leadership. Freetown, Sierra Leone, has always

had a diverse population. Artificially created in the late eighteenth century as a home for black settlers from Britain and North America, it became, after 1808, the center where recaptives liberated from slave ships by the British navy were settled. Immigrants from the surrounding country were attracted—traders, chiefly Muslims, from a distance, laborers from nearby. Kru laborers from the coast of what later became Liberia also came regularly to work. All of these communities evolved self-contained, self-constituted administrations under their own headmen.

The three authors of this collaborative book—a social anthropologist, an economic historian, and a historian concerned with Islam in West Africa—found a common interest in studying their own particular aspect of these community institutions and have fruitfully combined to describe them synoptically. The authors show how each community gave protection and the support of familiar regulations to its members to help them adjust to life in a strange city. Successive colonial governments faced an unwelcome decision. If they recognized the headmen's unauthorized jurisdictions, they implicitly accepted the humiliating existence of "no-go areas" outside the authority of the colony's legal and administrative system. If they prohibited them, the colonial governments would have had to provide alternative forms of local government that they lacked the means to introduce or enforce. They settled for an uneasy compromise, eventually recognizing and making use of headmen but closing their eyes to the illegal jurisdiction exercised by headmen's courts.

At independence the system continued; the headmen inevitably swept into party politics. But many people felt it perpetuated outmoded divisions within a population that was supposed to be striving for national unity. During the military regime of 1967-68 the office of headman was abolished. But they soon reappeared—indeed, as the authors show, in practice the headmen ignored the abolition decree and carried on as usual.

Collaborative writing needs strict editing, and this volume could have been improved by more of it. The early chapters in particular are disjointed. Information from one is repeated in another, and there is no clear unifying chronological sequence. Perhaps as a result of this lack of overall historical perspective, one important element is missing: the community institutions of the early settlers (in their churches) and of the recaptives. Apart from a brief reference to the Yoruba kingship, no mention is made of the numerous recaptive institutions or of the "Seventeen Nations" that joined them. Nor is Abner Cohen's study of the political role of the masonic lodges in Freetown mentioned.

There is also a needless confusion over nomencla-

ture. Recaptive descendants in Sierra Leone have been labeled "Creole" (today often written "Krio") since the mid-nineteenth century—a label used, for instance, by the most articulate member of their community, Africanus Horton, in his *West African Countries and Peoples* (1868). For reasons inadequately justified, the authors call them "Settlers"—a name usually restricted to the early North American immigrants—thus unwarrantably bemusing the unwary reader.

A final section compares the authors' findings with some similar investigations in other West African cities, giving their useful, worthwhile study a wider relevance; this work will interest and inform those studying the same kinds of urban phenomena elsewhere.

CHRISTOPHER FYFE
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KENNETH LUPTON. *Mungo Park, the African Traveler*. New York: Oxford University Press. 1979. Pp. xxii, 272. \$19.95.

Biography is a difficult genre, particularly when the subject is someone whose career has already been detailed. Kenneth Lupton has avoided most of the inherent problems in this excellent, short, detailed biography of one of the first, and probably the most famous, European explorer of West Africa largely because he maintains the focus on Mungo Park's African adventures. The short, informative sections devoted to Park's early life and his private affairs merely prepare the way for the main theme.

This thoroughly researched biography is a work for the educated reader rather than the specialist, although it holds much value even for the advanced student of West Africa. Lupton writes well and does not intrude to a noticeable extent on the narrative. Three appendixes provide much important information that would have been out of place in the main body. Maps are sadly lacking in most such studies; thus, the three good maps at the beginning of the book are a valuable adjunct.

The most lively and interesting part of the biography is the first section since it concentrates on the earliest of Park's adventures. Sponsored by the fledgling African Association, the young Scottish surgeon, accompanied by two African translators, left Pisanian in the Gambia during the dry season of 1795. He hoped to discover answers concerning the fabled River Niger. The events of this journey, duly noted in Park's journal, rival any encountered by later explorers of Africa. Forced to turn back at Silla with many of his questions unanswered, Park had nevertheless seen the river flowing eastward and had recorded the customs of some of the people of the western Sudan. Park returned to England in

1797, received the accolades of the association, and then transcribed his notes into a very popular book. One of Lupton's significant contributions is his detailing of Park's life during the seven-year interim before the government sponsored the ill-fated second expedition. Park, who wanted to continue exploring either in Australia or Africa, was troubled by conflicting desires as well as the slowness of officialdom. He married and fathered four children. Despite this, the taciturn Scot was so eager to return that he left his family behind even though he knew that the late sailing date of January 1805 placed the venture in danger because of the advent of the rains.

There are but a few negative items to report. At times Lupton intrudes to tell us that Park or someone else had made an error in reporting; yet he provides no explanation on how he arrived at this conclusion. The chapter devoted to Park's attitude toward slavery is unnecessarily long for the conclusion reached. The information about Jenne, Timbuktu, and other places visited by Park are reconstructions from other sources since Park's journals of this part of the second expedition were lost. Lupton's re-creations of what might have occurred at each place are interesting but overly long. The various accounts of Park's death at Bussa, although not fully settling the many questions, are both informative and provocative.

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JOHN DUNN, editor. *West African States, Failure and Promise: A Study in Comparative Politics*. (African Studies Series, number 23.) New York: Cambridge University Press. 1978. Pp. 259. Cloth \$27.50, paper \$7.95.

Whatever happened to West Africa? During the 1960s there was a rush by political scientists and historians to tell us why West Africa got its independence before other black African nations. This was followed by a lean decade as political scientists migrated elsewhere and historians retreated to earlier centuries. Now this gap is partially filled by a new volume of essays edited by John Dunn on seven West African nations. To his credit, he did not omit the critical francophone areas (the essays on Senegal and Guinea are sharply honed additions), but one wonders why this ecumenical attitude did not provide for continental representatives, such as Mali or Upper Volta, to balance the coastal states.

Fortunately, Dunn has not imposed a methodological straightjacket on his contributors; they are free to pinpoint idiosyncrasies in their case studies. Richard Rathbone wisely opts against boring us

with yet another review of Ghana's recent history and instead coolly ticks off what went wrong with Britain's former prize colony. Christopher Allen, by contrast, retells postwar political events in Sierra Leone before analyzing the plight of a nation on the verge of exhausting its resources.

The most stimulating essay in the volume is R. W. Johnson's superb update on Guinea, a difficult country to penetrate, much less to understand. He strikes a moderate stance and demonstrates how historical perspective can help inform an arcane subject; the analysis that emerges is possibly the clearest ever written in English on the theatrical politics of Sekou Touré and his proud but bankrupt nation. Donal Cruise O'Brien's graceful and literate essay on the philosopher-king of Senegal, Léopold Senghor, conveys his first-hand impressions after a four-month sojourn to observe recent political developments in a land whose political subtleties are often misunderstood.

Although the essays on Ivory Coast by Campbell and Nigeria by Williams and Turner contribute fresh insights to our knowledge of these two industrial giants, they suffer from being put into constricting analytical molds. Both essays succeed, however, in explaining why these countries are currently prosperous and why they will probably continue to be the economic pillars of West Africa. Christopher Clapham's essay on Liberia retells a familiar story of the Americo-Liberian elite and its propensity for power and maneuver. The participants should be proud of a solid and intelligent collection that demonstrates the vitality of current British scholarship on West Africa (often overshadowed by Africanists in East and Southern Africa); it should be required reading for anyone visiting for the first time the countries mentioned here.

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ASIA AND THE EAST

WILLARD J. PETERSON. *Bitter Gourd: Fang I-chih and the Impetus for Intellectual Change*. New Haven: Yale University Press. 1979. Pp. xiv, 228. \$22.50.

Willard J. Peterson's study of Fang I-chih (1611-71) contains a most impressive translation of a piece of literature (Fang's "Seven Solutions") that has the quality of greatness and is written in murderous Chinese. Beyond that, Peterson writes profoundly on Chinese thought, penetrates a personality of almost Dostoevskian complexity, and sheds much light on a pivotal period in Chinese history.

During the seventeenth century, great socioeconomic change was accompanied by a major in-

tellectual shift, political upheaval, and the penetration of Jesuit learning into elite intellectual circles. In this study, Peterson is interested mainly in what was on Fang's mind as he helped bring about this shift from the long-standing concern with the metaphysics of moral effort (*li-hsiieh*) to "substantial studies" (*shih-hsiieh*), an approach that became increasingly fashionable in the next century, that attacked the "empty talk" of metaphysics, that regarded philology and a bewildering variety of other topics as its domain, and that emphasized the need for factual or textual evidence. While Fang's famous role as a commentator on Jesuit learning has been treated by Peterson in another study, Peterson here barely mentions it, convincingly implying that other problems were more basic for Fang.

Illuminating these problems, Peterson exploits the autobiographical import of "Seven Solutions" to show how Fang defined his life possibilities, ranging from dissipation to scholarship. Most striking is the diffuse sense of frustration with which Fang negated one alternative after another. Far beneath this brilliant son of an elite family was even the thought that settling into the life of a burgher or landlord to work for his family would be a good way to realize China's most famous virtue, filial piety. Driven, like so many of his peers, by a nervous desire to be like the men "of old," Fang could consider three possibilities more seriously: government service in the Confucian sense, character-cultivation in the tradition of *li-hsiieh*, and the esthetic, somewhat un-Confucian style of the *wen jen* (man of culture). He turned from these to life as a Buddhist monk concerned with knowledge as a form of fact-gathering through which one could somehow reveal morality and reach for some totality of understanding.

Peterson can only partly show how this choice was made. Because Fang I-chih, unlike contemporaries such as Huang Tsung-hsi and Liu Tsung-chou, seems never to have been seriously attracted to *li-hsiieh*, Peterson's materials do not greatly help us understand the meaning of this problematic alternative in Fang's day. Neither does Peterson explain how Fang diverged from his many contemporaries, recently discussed in Lu Pao-ch'ien's study of Ch'ing thought, for whom "substantial studies" also meant writing on the restructuring of the central government. Moreover, Peterson only partly analyzes the socioeconomic changes of the day, even ignoring Li Wen-chih's very pertinent and solid hypothesis about the decline of the "privileged landlord." Similarly, in his revealing discussion of the problem of access to the ear of the emperor, he curiously ignores the central Neo-Confucian vision of access through the *tsai-hsiang* (prime office). Also curious is his attempt to identify the heroic ideal of

the knight-errant with the career, rejected by Fang, of that contemptible person who seeks to ingratiate himself with the powerful. Such reservations aside, Peterson's superb study is must reading for anyone interested in the Chinese tradition.

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ELISABETH CROLL, *Feminism and Socialism in China*. Boston: Routledge and Kegan Paul. 1978. Pp. 363. \$20.00.

Scholarly engagement is necessary, yet the engaged historian always risks looking backward to see his or her own reflection—a mirror image that has been especially distorting in pioneer feminist scholarship on Chinese women. Elisabeth Croll, like others, has turned to the history of women in modern China as a seemingly still untarnished example of a feminist movement that succeeded through its association with a socialist revolution. In the Chinese experience she hopes to find clues to the viability of a feminist strategy that subordinates sexual politics to class struggle and that makes liberation dependent upon structural economic transformation. These frankly stated ideological concerns, however, have not resulted here in a narcissistic theoretical analysis but have inspired a readable introductory survey of Chinese women's emancipation movements since the nineteenth century.

Quite simply Croll is the first to try to put the whole story before us. Sinologues will notice that she has relied largely on translated sources, while being forced to acknowledge that the narrative political history she presents needed writing. The work includes a good, if inevitably static, chapter on women's traditional status. Modern feminism is not presented as a creation of the Communist Party but is set in the historical context of earlier reform and nationalist movements. The Kuomintang government of the 1930s and 1940s, with its reformed legal code and middle-class volunteer organizations, is shown clearly as offering a bourgeois alternative to revolutionary feminism. Within the revolutionary movement itself, Croll documents the unresolved tension between CCP campaigns attacking domestic patriarchy and the social conservatism of the otherwise revolutionary peasantry. Still, with the bourgeois and traditional alternatives in view, readers are unlikely to dispute her conclusion that both socialism and feminism benefited from their revolutionary coalition.

After 1949 Croll's mirror clouds up, partly because of an inability to transcend either the analysis or the data base of her sources. The literature of official exhortation and mobilization vibrated with

campaigns—for women's participation in labor, for raising political consciousness, for equal pay for equal work, for cadre development. Here confused and often contradictory shifts in line show at least that the relevance of women's issues to socialist development was never, in this most experimental of times, officially forgotten. The Great Leap Forward brought millions of women into the labor force; its accompanying drive to socialize domestic labor faltered. Cultural revolutionaries suppressed separate women's organizations but pushed for "affirmative action" goals of female political participation. Were such "twists and turns" caused by the backward drag of traditionalist attitudes, not only among the masses but also within the leadership? Or was there a battleground of conflicting socialist-feminist strategies, pitting separatist against assimilationist, economic determinist against political activist, those who saw accommodation to female backwardness as a necessary adaptation to reality against those who branded it a betrayal? The documentary evidence only hints that such conflicts existed; it points to no clear socialist consensus as to their solution.

Therefore Croll's own tentative conclusion, that by the early 1970s feminism and socialism were achieving a "synthesis," seems premature—a piece of instant history fostered by her own trip to China in 1973. To prove such an ideological resolution would require a better comprehension of the Mao era than the murky last years of the "Gang of Four" could possibly afford an observer writing in 1975. To prove that social reality now approaches a Maoist ideological vision would require that we shift perspective from politics to the sociology of women's emancipation in China, an inquiry that since Mao's death is just beginning.

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HOLMES WELCH and ANNA SEIDEL, editors. *Facets of Taoism: Essays in Chinese Religion*. New Haven: Yale University Press. 1979. Pp. 302. \$22.50.

"Religion" is a key word in this book's title because it signifies that the subject treated is religious and not philosophical Taoism. These two thought systems, though historically and intellectually linked, are also sharply different. Philosophical Taoism began in the fourth and third centuries B.C. with Lao Tzu and Chuang Tzu and thereafter enormously influenced Chinese life and thought. Religious Taoism began in the late second century A.D. as a socioreligious movement and thereafter developed into a major religion with temples, priests, a large pantheon, and beliefs stemming not only from philosophical Taoism but also from folk religion, sha-

manism, and Buddhism. It furthermore fostered techniques, notably alchemy, for the achievement of long life or immortality. Until fairly recently, philosophical Taoism received much more attention from Western scholars than did its religious counterpart. The present book is notable, therefore, as the first major scholarly production in English wholly devoted to religious Taoism.

The book's lucid editorial introduction and its nine subsequent studies are outgrowths of a 1972 symposium on religious Taoism held in Tateshina, Japan. Its cosmopolitan spirit is manifested by the fact that several of the studies were originally written in Japanese or French and that its editors and contributors include five Japanese (H. Miyakawa, T. Noguchi, N. Ōfuchi, T. Sakai, and the late Y. Yoshioka), four representatives from France (C. L. Hou, actually a Taiwanese Chinese, M. Kaltenmark, A. Seidel, and R. A. Stein), and three from the United States (R. B. Mather, M. Strickmann, and H. H. Welch). Unfortunately, efforts to gain the participation of scholars from mainland China proved unavailing.

Chronologically, the studies range from analysis of a religious scripture of possibly the second century A.D. to observations about life in a major Peking Taoist monastery during the 1940s. Two studies are bibliographical: one on the complex and voluminous *Taoist Canon*, the other on Japanese studies of Taoism during the past century. The editorial introduction also concludes with a brief list of major French publications on Taoism from 1972 to 1977.

It should be stressed that the book is highly technical. All of its studies represent good or excellent scholarship, but some have such an abundance of detail that it will probably be difficult for non-specialists to see the forest for the trees. Somewhat easier reading in this respect are Hou's paper on Chinese beliefs in baleful stars, Mather's on the fifth-century Taoist theocracy of K'ou Ch'ien-chih, and Strickmann's on the slightly later alchemy of T'ao Hung-ching.

Strickmann's proposal (pp. 165-67), however, to redefine the word "Taoism" by limiting its use to religious Taoism only, while dismissing early philosophical Taoism as a mere "prehistory of Taoism," is an attempt that strikes this reviewer as both narrow-minded and presumptuous. After all, the term *Tao chia*, meaning a Taoist school of thought, occurs in Chinese writings (for example, *Shih chi*, chap. 130) a full two and a half centuries and more before the religious movement (late second century A.D.) with which Strickmann would begin use of the word "Taoism." This reviewer will be astonished if the proposal gains support from any significant number of scholars.

In sum, this book adds notably to scholarly

knowledge about an important but hitherto poorly explored aspect of Chinese civilization. One hopes it will inspire sequels.

DERK BODDE

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DAVID KOPF. *The Brahmo Samaj and the Shaping of the Modern Indian Mind*. Princeton: Princeton University Press. 1979. Pp. xxiii, 399. \$25.00.

Founded in Calcutta during the second quarter of the nineteenth century, the Brahmo Samaj (Society) consisted of a section of the emerging Western-educated elite who, influenced largely by Unitarianism and philosophic liberalism, reinterpreted their Hindu traditions along monotheistic and rational lines. Accordingly, they rejected idol worship and other rituals of popular Hinduism and, more importantly, promoted far-reaching reform of the Hindu social system, placing particular emphasis on the education and general emancipation of women. Although few scholars will endorse David Kopf's sweeping assertion that the society "played a crucial role in the genesis and development of every major religious, social, and political movement in India from 1820 to 1930" (p. xiii), the Brahmos represented the most radical wing of the modernizing spectrum in nineteenth-century India and had a substantial effect on the intellectual and social life of urban Bengal until the beginning of the century. As such, the Samaj has long merited scholarly attention and Kopf's masterly study, based on a thorough examination of English and Bengali sources, fills a major gap in the intellectual history of modern India.

Kopf gives little attention to the structural or institutional history of the Samaj, which even at its peak never had more than a few thousand members distributed among some two hundred local branches. Instead, and more interestingly, he focuses on the philosophic and theological ideas of leading Brahmos and their role in social reform and the intellectual renaissance in Bengal. He categorizes the Brahmos into three main types, devoting a separate section to each. The first type consists of Brahmo modernizers or "liberals" who were most closely attuned to Western progressive ideas. They were mainly the later generation of Brahmos associated with the secularist and reform-oriented Sadharan Samaj established in 1878 following the second major schism in the movement. This group included two internationally recognized scientists and a distinguished surgeon as well as prominent lawyers, philosophers, journalists, social reformers, and political leaders. Kopf makes a valuable contribution in his perceptive analysis of their ideas and activities. The book's second part deals with those

Brahmos who had greater misgivings about foreign dominance and Western-inspired social reform. Generally labeled "conservatives," this faction, which generally followed the leadership of Devendranath Tagore and formed a separate Adi Samaj in 1866, Kopf more appropriately characterizes as "cultural nationalists"; he emphasizes their importance in defending reformed Hinduism against the Christian missionary assault. Part three, the most original and interesting section of the book, concentrates on attempts to reconcile modernization and nationalism, firstly, in the Brahmo-neo-Hindu evangelism in rural Bengal; secondly, in the activities of Keshub Chandra Sen, the charismatic leader who dominated the Brahmo movement in the 1860s and 1870s; and, thirdly, in the ideas of Rabindranath Tagore, modern India's most famous poet and writer. The theme of Brahmo evangelism has not been previously explored, and Kopf's portrait of Keshub as an Indian prophet of a universal religion of harmony rather than as a near-Christian is quite convincing.

In his passion for his subject Kopf exaggerates the influence of the Samaj, often neglecting the broader influences of Western education. There seems to be an assumption, for example, that the activities of every Brahmo were shaped solely by his Brahmoism. At other times, as in the chapter on Brahmo evangelism, the line between Brahmoism and Vaishnavism seems very thin. On more specific issues, although it is appropriate to designate the reformist Brahmos as "liberals," to characterize them further as "non-nationalists" is to confine one's interpretation of nationalism to anti-imperialism. Many of them clearly had a concept of an Indian nation. Also, Kopf's justification of Keshub's giving of his daughter as a child bride to a feudal prince, while interesting, is not convincing. More analysis of why the movement ceased to be a dynamic influence after the late nineteenth century would also be valuable.

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ALGERNON RUMBOLD. *Watershed in India, 1914-1922*. London: Athlone Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xii, 344. \$32.25.

Sir Algernon Rumbold was employed in the India Office in London from 1929 until the attainment of Indian independence in 1947. Since his retirement, he has devoted his energies to reflections on the way in which Britain's Indian empire ended. His principal conclusion is that it need not have happened. "Autocracies are not necessarily doomed to die," Rumbold says, "There are still subject peo-

ples . . . there is no principle of inevitability operating against men ready to rule over others who are willing to be bidden" (p. 321). Answering the implicit question of what went wrong in India led Rumbold to the period of his scholarly researches, 1914-22. In these years, shortly before his own arrival at the India Office, Rumbold argues, British officials played their hand so poorly that Indian awe of British omnipotence was irretrievably undermined.

Rumbold has done his research carefully and presents his findings in a forceful, elaborately documented argument. His book thus deserves careful study, even though his conclusions will strike many readers (as they do me) as often deplorable or even bizarre. Mahatma Gandhi, we are assured, for example, could easily have been rendered ineffectual at any time by a display of British firmness. Only the sapping of Britain's willingness to act with confident ruthlessness doomed British rule. In Rumbold's words, "the worst of the wounds which eventually proved fatal to the Indian Empire were self-inflicted between 1916 and 1921" (p. 322). The man Rumbold holds mainly responsible for destroying Britain's will to rule is Sir Edwin Montagu, Secretary of State for India from 1917 to 1922. Rumbold has little good to say about Montagu, either as a policy maker or as a person. "The darting qualities of his mind," he writes, "the belief that he was open to flattery or pressure and his lack of anchorage, introduced an uncertainty into Indian policy which made him suspect. . . . Instead of welcoming advice, his almost feminine instinct was to attribute malice or stupidity to those who counselled caution" (pp. 101-02).

As if having Montagu at the helm of Indian affairs in London were not bad enough, in 1921 an Indian viceroy was appointed who was known publicly as Lord Reading, but who was born with the name Rufus Isaacs. "Isaacs had risen by his talents alone. That would not in itself have been a handicap in ruling the Indian Empire. The fact however that he was of Jewish race, and that he was appointed at a time when the Secretary of State for India was also of Jewish race, roused fears that, without the ballast of tradition, the two between them might embark on ill-thought-out ventures" (p. 242).

Rumbold's book explains and exemplifies the strengths and the limitations of the class of British officials who clung to power in India until the bitter end. Self-assurance, diligence, contempt for "politicians" of the ilk of Gandhi and Montagu, and a certainty that chaos was the only alternative to autocracy were characteristic of this class and are characteristic of *Watershed in India*. Anyone who has trouble understanding how and why the British held onto India long after the compromise of their

capacity to innovate constructively there would be well advised to read this book.

In conclusion, Rumbold notes regretfully that, if Indian independence had been averted, "the middle of the twentieth century might not have seen the massive move of dependent peoples to sovereignty" (p. 322).

FRANCIS G. HUTCHINS

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B. R. TOMLINSON. *The Political Economy of the Raj, 1914-1947: The Economics of Decolonization in India*. (Cambridge Commonwealth Series.) London: Macmillan Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xiv, 199. \$57.50.

The object of this short, readable book is to analyze the decolonization of India in terms of the fluctuating economic conditions that brought about changes in the nature and objectives of British rule in India between 1914 and 1947. The author concentrates on imperial policy and argues that the process of decolonization came about through a series of short-term decisions made by British governments concerned with a limited number of specific objectives and constraints. Obviously, B. R. Tomlinson is not one of those historians who see a grand design in British rule in India. Rather, he contends that the British had no long-term strategy for decolonization. In the twentieth century the empire faced a number of crises, each of which left India's imperial role altered. This steady erosion of the imperial role assigned to India by British policy makers was the most important single regulator of the development of constitutional reform.

India's role in the imperial system had three aspects: commercial, strategic, and monetary. This role, however, only became apparent at times of imperial crises: the two world wars, the trade depression of the 1920s, and the Great Depression of the 1930s. Before dealing with the impact of these crises on India's imperial role, Tomlinson sets the stage by analyzing the political economy of the Raj in 1913. Until that year, the impact of a growing international market for Indian produce helped to strengthen rather than weaken traditional agencies such as the indigenous Indian banking system. India's imperial commitment then meant three things: India had to be a market for British exports, so the government of India was not to impose tariff barriers to trade; the Indian Army had to be available for imperial purposes; and the government of India had to ensure the repayment in sterling of interest on government bonds. In the remaining three chapters Tomlinson shows the impact of world events on each aspect of India's imperial commitment in a period when the demands of Indian

nationalism also became a force to be reckoned with.

The author does not claim to have written a definitive study of the economics of decolonization, but this does not deprive his work of value. One of its most useful features is the way it ties changes in the imperial relationship to the institutions that integrated the Indian economy and connected it to the outside world. For example, Tomlinson shows how, in the period between the wars, the impact of a depressed world demand for Indian produce on the internal credit networks was to make the money market in India better integrated than before, with a definite switch of internal investment out of agriculture and trade and into industry. Such changes had an important effect on Indian and imperial politics and government, and these are dealt with in chapters on the relationships between the British and Indian governments, and between the latter and its subjects. In a postscript, Tomlinson returns to his principal theme: the cause of decolonization must be traced to the changes in the structure of the Indian economy and its relationship to the imperial and Indian economies. This theme is well grounded in a solid base of documentary source material and is argued with admirable clarity and economy. I strongly recommend this book to Indian and imperial historians.

PETER HARNETTY

University of British Columbia

GOWHER RIZVI. *Linlithgow and India: A Study of British Policy and the Political Impasse in India, 1936-43*. (Royal Historical Society Studies in History Series, number 13.) London: The Society; distributed by Swift Printers, London. 1978. Pp. x, 261. \$20.50.

Of the British vicerealties that occurred during periods of crisis in India, Lord Linlithgow's administration deserves special attention. Gowher Rizvi's aim is both to analyze and to humanize Linlithgow, partly because of Linlithgow's unsympathetic image as a hard-lining imperialist. Rizvi skillfully blends official correspondence from British collections, viceregal and otherwise; and he sheds both fresh and provocative light on Linlithgow and his times.

Linlithgow began in 1936 with the challenge of implementing the Government of India Act of 1935, which provided for democratic Indian ministries in the provinces of "British India" and for a federation combining representatives of those provinces with representatives of the princely, largely autocratic "Indian States." By early 1939 Linlithgow had succeeded with the ministries, but the controversial federation still eluded him. Rizvi's assessment of Linlithgow's inability to secure the

federation is less persuasive than H. V. Hodson's *Great Divide: Britain-India-Pakistan* (1971). (For example, compare Hodson, pp. 58-60, with Rizvi, pp. 74-78.) But from 1939 onward Linlithgow had more pressing concerns. When the Second World War broke out, Linlithgow patriotically declared India at war with the Axis powers. The Indian National Congress's ministries in six provinces resigned in protest at this unilateral decision. Congress made a condition of its further cooperation that Linlithgow and the British government take immediate steps to assure India of postwar independence, the most important step being to convene a constituent assembly. Linlithgow balked. He was fighting a war and had never favored Congress aims. Now, relieved of Congress ministries, he was in no hurry to accommodate Indian nationalism. He was equally quick, as Rizvi demonstrates (pp. 106-23), to co-opt M. A. Jinnah and the Muslim League to the British side—widening the existing gap between the League and the Hindu-majority Congress, underwriting the League's quixotic claim that it represented all of India's 26 percent Muslim population, and encouraging the League's demand that autonomous Muslim provinces be created if necessary to counteract the Hindu majority in the subcontinent. In other words, Linlithgow instituted his own version of that recurring British expedient in India, *divide et impera*.

Readers interested in India's progress toward partition in 1947 will find Rizvi's work thought-provoking. His eagerness to be fair to Linlithgow leads him, however, to overstate parts of his case. Although he deplores a search for scapegoats, he tends to attribute most of the blunders of the period to the Indian National Congress. For example, he repeatedly blames Congress leaders like Gandhi and Nehru for actions that, if hasty or lacking in the studied pragmatism of their Muslim antagonist, Jinnah, were based on the accurate observation that most British politicians, including Linlithgow, were unwilling to underwrite national or democratic goals for India. Congress impatience led them to overreach their resources for a revolution. But on Rizvi's own carefully marshaled evidence, Linlithgow's purposeful cultivation of Jinnah and the League during 1939-42 did at least as much as Congress bungling (or indeed Jinnah's cleverness) to thwart the emergence of a territorially integrated Indian nation. Because of Linlithgow's uniquely privileged position as viceroy, it is hard to accept Rizvi's rather complacent tendency to lay the main responsibility elsewhere. Rizvi himself makes clear that when Linlithgow left India in 1943 he had pushed the Muslim League and the country measurably down the road toward "Pakistan."

RAY T. SMITH, JR.
San Diego State University

CARL A. TROCKI. *Prince of Pirates: The Temenggongs and the Development of Johor and Singapore, 1784-1885*. Singapore: Singapore University Press, for Institute of Southeast Asian Studies; distributed by Ohio University Press, Athens, Ohio. 1979. Pp. xxi, 251. \$12.00.

Nineteenth-century Johor was unique among the Malay states in several respects, and Carl A. Trocki's well-written study brings out clearly the main reasons why. For one thing, its rulers—known first as temenggongs and later elevated to the status of sultans—were especially enterprising and distinguished characters. The temenggong in the traditional Malay state was a senior official with a variety of administrative and ceremonial duties, but by the end of the eighteenth century the temenggong of Johor had come to specialize in something like piracy, being chief of marauding bands of sea people (*orang laut*) who operated in and around the Straits of Malacca and the South China Sea.

The founding of Singapore by the British in 1819 did not at once disrupt the temenggong's business; indeed, it may have doubled it. His successor, though acting as Singapore's official suppressor of piracy, was himself described unofficially in 1836 as "the chief pirate." But the successful growth of Singapore as an entrepôt, the discovery of gutta-percha in the forests of Johor, and the spread of pepper and gambier planting by Chinese immigrant farmers across Singapore island and on the mainland of Johor in the 1840s combined to open up a brave new world for the temenggongs, enabling them to abandon maritime enterprise for territorial rule and in the end to build a model Malay state.

The Chinese plantation system in Johor became the basis of the wealth and power of its small Malay ruling class. It was a system fortunately unbedeviled by the intra-Chinese group feuds that characterized the turbulent frontier of the tin-mining Malay states to the northwest; and this circumstance, combined with the modernizing ability and political acumen of its rulers, enabled Johor—despite its proximity to the headquarters of British administration in Singapore and its continuing dependence upon Singapore's economy—to retain internal independence long after other Malay states had been brought under British control. The wealthy and astute Abu Bakar (1862-95), most illustrious of the nineteenth-century temenggongs and, indeed, "the single most important Malay political figure of the century" (p. 118), first assumed the grandiose title of Maharajah, became powerful enough to deal directly with the colonial office in London over the heads of the local British administration in Singapore, and was officially recognized as sultan in 1885.

Although Trocki's study owes something to the earlier work of such authors as Nicholas Tarling, Mary Turnbull, and Eunice Thio, it is enlivened by a freshness of approach, and its value as a contribution to the subject is enhanced by his use of the state archives to examine in depth the operation of the *kangchu* system by which Chinese entrepreneurs were licensed not only to open up the river valleys of Johor to commercial plantation but also to raise taxes and maintain law and order. It all provides a striking example of that cooperation among the interests of British colonialism, the Malay ruling class, and Chinese immigrant enterprise, which shaped not only twentieth-century Johor but also modern Malaysia as a whole.

BRIAN HARRISON
Rye, England

BARBARA WATSON ANDAYA. *Perak, The Abode of Grace: A Study of an Eighteenth-Century Malay State*. (East Asian Historical Monographs.) New York: Oxford University Press. 1979. Pp. xviii, 444. \$54.00.

The Malay state of Perak on the west coast of the Malay Peninsula is a typical riverine sultanate. Barbara Watson Andaya has written the history of this state in the eighteenth century, focusing on the half century from 1746 to 1795. These were the years in which the Dutch East India Company had a treaty arrangement with successive Perak sultans. Under this treaty the rulers of Perak delivered tin exclusively to the company, while the company constructed a fort near the mouth of the Perak River to protect the sultanate against threats from sea raiders, especially Buginese and Minangkabau, and to enforce its tin monopoly. Despite the continuous failure of the Perak rulers or the company to eliminate smuggling, the perpetual grievances and negotiations about the price of tin, and the dismal failure of the company fort to provide protection in moments of crisis, the treaty seems to have been important to the Perak sultans. The reason for this, we are told, is that Perak was a weak state. When the company declined and disappeared at the end of the eighteenth century, the fate of Perak was sealed, for in the nineteenth century (not treated in this book) Perak was ravaged by invasion and internal dissension. Perak Malays in these later years looked back to the eighteenth century as a golden age in their history.

Since the available published information on Malay sultanates in the eighteenth century is rather small, this book expands our knowledge and provides interesting insights into the functioning of a rather average Malay sultanate. The fact that a major collection of source materials, namely the company records, is external to the inner workings of

the sultanate tilts the account in the direction of diplomatic-commercial history. The author's efforts to balance this through the use of Malay records, especially the contemporary *Misa Melayu*, is not totally successful. This chronicle is without a doubt a fine example of classical Malay writing, but precisely for that reason it should have been subjected to more rigorous questioning than it has here received. Instead of repeated eulogies of the splendor and glory of the court of the Perak sultans, the reader might wish for more consideration of why this well-to-do state was so incapable of organizing its human and material resources toward self-survival.

ROBERT VAN NIEL
University of Hawaii

UNITED STATES

JOHN HIGHAM and PAUL K. CONKIN, editors. *New Directions in American Intellectual History*. Baltimore: Johns Hopkins University Press. 1979. Pp. xix, 245. \$15.00.

This anthology—dedicated to Merle Curti—is close to an “official” directive on the state of American intellectual history studies today. Its official quality is strengthened by the presence of John Higham—co-organizer of a 1977 conference where the essays here were first presented as papers and author of a characteristically graceful introduction to the volume. Ever since 1951 and his famous essay on “The Rise of American Intellectual History” (*AHR*, 56 [1950–51]: 453–71), Higham not only has been explaining the field and its past to colleagues but also has been prophetic of its future.

If this volume be taken as prophetic, in the decade ahead American intellectual historians will (1) increasingly use *social science perspectives* in their studies (see especially the essays here of Laurence Veysey, Gordon Wood, and Murray Murphey), (2) focus on ideas of *particular subcommunities* rather than aim for a general “American Mind” (see Thomas Bender’s magnificent essay on intellectual professions and the city, and, for a cogent dissenting opinion, Rush Welter’s “On Studying the National Mind”), (3) be more sensitive to the *media* through which historical ideas are communicated and not uncritically take the printed word for the whole of cognitive reality (see the essays of David Hall and Neil Harris), and (4) continue being *self-conscious*, and occasionally anxious and defensive, about their field (see the critical essay by Thomas Haskell and the piercing “Afterword” of Paul K. Conkin).

The first “new direction” is the most striking. In-

stead of humanistic terms like "mind" and "myth" and "image"—favorites of past intellectual historians—we find here a language grounded in the social sciences—"social aggregates" (Veysey), "modes of communication" (David Hollinger), "disciplinary matrix" (Haskell), "division of intellectual labor" (Murphey), "intellectual hegemony" (Bender), and, of course, "paradigm" (used by almost everyone).

Higham writes that Clifford Geertz was "virtually the patron saint of the conference" (p. xvi). That is not quite true. If Geertz and Thomas Kuhn are noted by an equal number of contributors (six each of the fifteen total), Geertz is mostly relegated to footnotes, while Kuhn's sense of paradigm and paradigm communities exercises a powerful hold on the participants.

Missing, however, is Kuhn on *change* or, for that matter, much detailed attention anywhere in the volume to historical ideas in the process of transformation. Haskell comments insightfully on the statics of paradigm functioning but confesses that the dynamics of change in Kuhn are a "mystery" (p. 144). I believe Haskell is wrong, but his comment is indicative of a field that has not effectively handled change or strain in historical ideas.

Missing also are newer perspectives from women's history and from studies of minority cultures. A Nancy Cott or a Lawrence Levine have elsewhere gleaned ideas from nonpublic or non-written sources not ordinarily used by intellectual historians. Though there is much discussion of such sources here, there is little actual representation (save for Harris's essay on the half-tone engraving process).

Finally, the volume—perhaps the conference too—might have profited from an *outside* perspective on the field. Only Sacvan Bercovitch of those represented here is not an academic historian, and he is not far removed. Commentary from a Clifford Geertz, a Thomas Kuhn, a Peter Berger, or perhaps a Kai Erikson could have offered a more detached "new direction" for the field not found in the professional historians here.

GENE WISE
University of Maryland

CAROL HYMOWITZ and MICHAEL WEISSMAN. *A History of Women in America*. New York: Bantam Books. 1978. Pp. xii, 400. Paper \$2.95.

This general history of women in the United States will appeal to students because of its readability and to teachers because of its scope and accuracy. Although they are not professional historians, the authors accomplish a difficult task. Drawing upon their reading of specialized historical studies, they

carefully blend multiple themes into a lively and informed history of American women that spans from colonial times to the present. In addition, this reasonably priced text is extremely well written and abundantly illustrated.

Unlike many surveys of women's history, this work gives significant space to the struggles of working-class and black women, as well as to the well-known battles of leading feminists and professional women. The brutal sexual abuse of the female slave is realistically presented and its connection to the idolatrous legend of pure southern womanhood is skillfully shown. The plight of female wage earners, from the early mill-girls to immigrant mothers and daughters, is also discussed, including their attempts at labor organization. The authors seek to highlight the history of the masses of women. Yet they also stress the lives of the individual feminist "greats" and the social movements that they sparked.

The post-1920 discussion of women's history emphasizes the economic and psychological changes that affected women's status. It concludes with an excellent summation of the New Feminism—its origin, ideology, and challenges.

This book should prove to be a popular text for beginning students in women's history. It is especially well suited for study in high school classes. Its breadth of coverage and variety of historical analysis also make it an appropriate choice for undergraduate survey courses.

DEE GARRISON
Rutgers University

DEWEY W. GRANTHAM. *The Regional Imagination: The South and Recent American History*. Nashville, Tenn.: Vanderbilt University Press. 1979. Pp. xiv, 269. \$11.95.

Over the past two decades Dewey Grantham has built for himself a solid reputation as a southern historian. Two books, three edited works, and a large number of perceptive essays and articles on assorted topics constitute the basis for his high standing in the profession. A survey of this well-known historian's work reveals four major themes: the continuity of southern distinctiveness; the convergence in the thinking and behavior of southerners and non-southerners in modern times; the functions of sectionalism in national politics; and the interplay of southernism, racism, and the national behavior in regard to the black segment of American society.

These themes run through this volume's fourteen chapters, which are devoted to specific subjects such as sectional politics, the Bourbons, southern violence, the Black Patch War, Hoke Smith, Ralph Bunche, and Jimmy Carter. All but one of these

chapters have been published before, either as chapters in books, introductions to edited works, or articles in such respected journals as the *Journal of Southern History*, the *Journal of American History*, and the *South Atlantic Quarterly*. Thus, except for the one heretofore unpublished essay on the Little Rock school crisis, all of the material in this volume is easily accessible elsewhere. This accessibility raises the legitimate question as to whether this volume is needed at all. A more important reservation about the advisability of the publication of this volume is related to the original dates of many of the essays. Since some of the articles are nearly twenty years old, many of the interpretations are dated. In fact, Grantham recognizes this and in his brief introductions to the essays he includes the authors and the titles of recent publications so that the reader can update the subject under discussion. One example will suffice. In the introduction to his article on the southern Bourbons, Grantham lists three recent scholarly monographs on the subject and writes: "Readers may be struck by the extent of the fresh scholarly writings on the period dominated by the southern Bourbons." He concludes: "One scarcely needs to add that if the essay of 1961 were being written in 1978, some additional questions would have to be raised and some new interpretations considered" (p. 24). Since this statement is essentially applicable to all his essays, one raises again the reasonable doubt about this volume's value.

Dewey Grantham has been doing original research on the southern Progressives for nearly two decades. Those of us who know him as a scholar have been waiting for many years for the publication of what will surely be a major contribution to southern and national politics. Grantham's attention to several edited works and the volume under review surely has played a role in the delay of his completion of a very important book based upon original research. I for one wish this able scholar would not allow diversions such as this volume to keep him from giving us the benefits of his considerable research and writing talents.

MONROE BILLINGTON
New Mexico State University

JEROME O. STEFFEN, editor. *The American West: New Perspectives, New Dimensions*. Norman: University of Oklahoma Press. 1979. Pp. ix, 238. \$14.95.

In his introduction to this collection, editor Jerome O. Steffen acknowledges that writing in the history of the American West has yet to reach its full potential. Steffen offers here eight articles (one of them his own) as pointers toward realizing that potential. The pieces stem, he writes, from "different but

equally valid categories of inquiry into American western development" (p. 5). Six of the contributors are historians; one is a geographer and one a psychologist.

John Opie leads off with a helpful review of the literature in the field of environmental studies, but his advocacy of ecology as an "alternate interpretative context for frontier history" (p. 25) is less than persuasive and if adopted would surely lead to excessive presentism. Geographer John Hudson argues convincingly for "refocusing the study of frontier populations to include their broader geographic and demographic contexts" (pp. 57-58), while psychologist Roger Barker advances some tentative assumptions about the nature of frontier environments and suggests their consequences for pioneer behavior. Barker's theoretical framework seems overly elaborate for the rather obvious conclusions it yields.

In a provocative piece comparing "insular" and "cosmopolitan" frontiers, Steffen seeks to "generate a larger approach to western history" (p. 94) and to help re-establish "a dialogue between western historians and the rest of the historical profession" (p. 116). His argument must be read to be understood. To understand it, in this reviewer's opinion, is largely to reject it. Reginald Horsman contributes a most useful historiographic essay in which he surveys contours of controversy over the interpretation of white-Indian relations and points out areas of research that need attention. Richard Etulain examines the fusion of western history and fiction through the works of Vardis Fisher, A. B. Guthrie, and Wallace Stegner. His treatment of Stegner is particularly thorough.

Ronald L. F. Davis contends that the history of urbanization in the West lacks an analytical framework. In response, he proposes that the western city be viewed as "an attribute dependent upon the existing and preexisting modes of production in society [thereby providing] a basis for seeing the diversity and shared character of western cities" (p. 192).

In the last essay Gene Gressley reviews the largely abortive attempts of the West that lies between the Rockies and the Sierras to achieve regional coherence. As population flows into the area and its natural resources continue to dwindle, Gressley sees a chance that the inter-mountain West may shed its localism and join in a regional compact strong enough to offset the centrifugal force of the federal government.

Although the eight essays are diverse in content, method, perspective, and style, one distressing feature marks most of them: the quality of writing is poor. One need not demand the grace of Parkman, but is it not reasonable to expect straightforward, muscular prose that clarifies rather than obscures

the writer's meaning? With two, possibly three, exceptions, the contributors to this collection fail to meet such a standard. To make matters worse the book is pockmarked with typographical errors—more than 60 in only 238 pages. In thirty years of academic life no sorrier piece of book making has come across this reviewer's desk.

EDWIN R. BINGHAM
University of Oregon

JAMES W. CEASER. *Presidential Selection: Theory and Development*. Princeton: Princeton University Press. 1979. Pp. xiv, 371. Cloth \$20.00, paper \$4.95.

Americans have never much liked their political parties, yet few elements of the American political system are as vital as our parties. We are never so mindful of this reality as in presidential election years, for the whole question of presidential selection is inexorably tied to the quality of competition both within and between the major national parties.

James W. Ceaser has transformed a Harvard doctoral thesis in political science into an ambitious and relatively comprehensive historical analysis of the way our political parties have shaped presidential selections. The author takes as his focal problem the failure of the contemporary presidential election system to provide a moderating influence on the pursuit and exercise of power. He candidly espouses a selection system that rewards the politics of moderation, the politics of coalition-building, and especially rewards those who willingly work within the mainstream of party politics. In short, this is a book that contains frequent praise for the party regulars in the Martin Van Buren mode and just as frequent criticism for the pro-participatory activism of recent reformers.

The book is both an exercise in American political thought and a rigorous institutional analysis. Lengthy chapters are devoted to the intent of the Founders, the theories and practices of the Jeffersonian period, the vital role played by Martin Van Buren in nurturing permanent party competition in America, the political thought of Woodrow Wilson, and the evolution of the contemporary party and presidential selection systems. The author's chief contributions come in his helpful examination of evolving theory and practice in the nineteenth century. Ceaser builds upon the work of Richard Hofstadter and Robert Remini to provide a vivid portrait of Van Buren's critical contributions to the art of politics in America.

This study also poses a variety of criteria by which we might evaluate the way we recruit and elect our chief executive. Some readers will doubtless quarrel with certain of the author's values,

which often enter into his evaluative standards, but few will come away from a reading of the book without a richer understanding of many of the dilemmas and paradoxes of the selection process.

Perhaps the most troubling aspect of this book is its topical last chapter, entitled "Modern Party Reform." Here the author becomes an advocate. Among changes he calls for are reductions in or elimination of primaries, lessening or elimination of public financing of presidential elections, and a lessening of "popular" leadership. Political scientist Ceaser apparently yearns for a pre-Progressive-era party system and hopes for an unlikely decided rollback from the democratic impulses of the past decade or two. He may be right or he may be off base—this is for the reader to judge—but the tone is strident at times and not a little jolting after the earlier analysis of historical evolution and political theory.

On balance, then, we have in *Presidential Selection* a readable and informative analysis of a vital American process. It can be read with profit and insight and will surely stir debate and reappraisal.

THOMAS E. CRONIN
University of Delaware and
Colorado College

ABRAHAM S. EISENSTADT *et al.*, editors. *Before Watergate: Problems of Corruption in American Society*. Brooklyn: Brooklyn College Press; distributed by Columbia University Press, New York. 1978. Pp. xiv, 231. \$15.00.

Not too surprisingly, the revelations of Watergate have stimulated a new awareness of corruption in public affairs and of its significance. Perhaps the best indication of this in academe is the conference held at Brooklyn College in May, 1977, with a theme that became the title of this interesting and wide-ranging work. Among the participants was Arthur Schlesinger, jr., whose brief remarks, forming the introduction, begin appropriately enough with a quotation from the Book of Genesis: "And God looked upon the earth, and, behold, it was corrupt." The scholar's task, as Schlesinger observes, is that of seeking to understand the "deeper social mystery" that lies behind corruption (p. 1). He turns to Edward Gibbon, Karl Marx, Robert K. Merton, and others for theories and insights on the subject. Fourteen contributors to the book examine a variety of questions in both the European and American past, although predominantly they concern the United States. All but one of the essays are essentially political in emphasis; four of the fourteen are biographical. Generally the book is loaded with ideas, and a short review cannot possibly do justice to it.

How does one define "corruption"? This is a problem that keeps arising from chapter to chapter and is never settled. Morton Keller in his "historian's-eye view" of American public affairs seems to assume a rather clear distinction between the numerous acts of greed or venality and the more dangerous attempts, occasionally, to usurp power (pp. 8, 16). Edward Pessen, writing on the Jacksonian era, decides that officeholders were frequently guilty of both "venality" and "abuse of power" and that the two were interrelated (pp. 80, 94). Arnold J. Heidenheimer, a political scientist, insists that charges of corruption should be confined to those who have used their positions of public trust for either "direct or indirect material enrichment," while more serious abuses or usurpations should be considered separately (p. 25). In this reviewer's opinion, such a distinction is often difficult, if not impossible, as shown notably in the Teapot Dome scandal and, to an extent, in Watergate itself.

Another theme or recurrent idea is the comparison of corruption in particular instances with other evils whose effects socially may be more damaging. Linda Levy Peck, writing on "The British Case" in early modern history, is one of a half-dozen contributors who cite the influential works of Samuel P. Huntington, a social scientist who treats corruption as a natural ingredient during national growth and modernization. Peck emphasizes, however, that while corruption has its useful purposes and may contribute to political stability, it also can be a "dissolvent," as it was in English history during the 1640s and in British colonial relations in 1776 (p. 47). Four biographical essays strike this reviewer as being among the best. They are: "Albert Gallatin and the Problem of Corruption in the Federalist Era" by Edwin G. Burrows; "Aaron Burr as a Symbol of Corruption in the New Republic" by Mary-Jo Kline; "Carl Schurz, the South and the Politics of Virtue" by Hans Trefousse; and "The Case of Nelson W. Aldrich and the Sugar Trust" by Jerome L. Sternstein. Trefousse describes how Schurz in the 1870s virtually abandoned the cause of newly freed black people and turned his energies instead to fighting corruption through civil service reform—a lesser cause. Good essays by Ari Hoogenboom and Robert Muccigrosso also deal with the Gilded Age. The latter, writing on "Corruption and the Alienation of the Intellectuals," traces forms of cultural protest against "pervasive, corrosive materialism"; one result, of course, was expatriation (pp. 166, 175).

The book contains a few disappointments, one of which is the lack of an index. But, as the editors say in their foreword, the hope is to encourage further research rather than to complete the inquiry here. Heidenheimer is the contributor who seems most concerned at the dearth of scholarly studies in the

United States relating to corruption, and he offers a number of interesting suggestions. A. S. Eisenstadt in a concluding essay raises some questions that ought to be pursued. All in all, this book should serve as a major stimulus to further thinking and research.

J. LEONARD BATES
University of Illinois,
Urbana-Champaign

ALLAN J. LICHTMAN and JOAN R. CHALLINOR, editors. *Kin and Communities: Families in America*. (Smithsonian International Symposia Series.) Washington: Smithsonian Institution Press. 1979. Pp. 335. Cloth \$17.50, paper \$7.95.

Important topics are the product of important questions. Historical study of social mobility produced some of the most innovative research of the 1960s, partly because it sought to answer a significant question, whether or not America was an open society in which persons could "make it" through ability and hard work. Once the answer to that question came in (a qualified yes), the topic receded in salience, partly because no similarly compelling question emerged to re-focus the research effort.

Family studies may suffer the same fate. They started off in the early 1970s with a significant question—whether or not modernization shattered the cohesion of the traditional extended family and led to the isolated nuclear family. Once the answer came in (the traditional family was less extended and less cohesive than believed; the modern family less isolated), family research has been left without a central organizing question. The result of this void may be the emergence of a lot of research details, but no picture, if the present volume is a harbinger.

In one sense, *Kin and Communities* represents the official arrival of family studies. It is the published outcome of a national symposium held in 1977 at the Smithsonian Institution, sponsored by a raft of corporations (ranging, alphabetically, from American Security and Trust to Weyerhaeuser) and addressed by such personages as Hubert Humphrey, Alex Haley, Margaret Mead, and Rosalynn Carter.

After such a buildup the book disappoints. The problem is not the fault of the contributors. The articles, taken individually, are good, and a few merit special attention. It is simply that the range of topics is so broad, and their focus so diffuse, that the whole is less clear than the parts.

There are ten articles in the volume and seven touch only tangentially on the family. These include Devra Kleinman's study of polygamy among lower mammals, Dale Stewart's sketch of the early trans-Siberian migration that populated the New

World, David Musto's tracing over several generations of the Adams family's high achievement values, David Schneider's unraveling of the "symbolic connections" between kinship and community in American culture, Seena Kolil's study of women pioneers in Saskatchewan, Rosenkrantz and Vinovskis's study of the asylum in antebellum Massachusetts, and Francis Hsu's comparison of individualism and family authority in ancient myths of China and the West. One other essay, Jacqueline Jackson's study of how black families treat their aged members, is more to the point but is fairly unremarkable in its conclusion (that they are treated similarly in both black and white families).

Two essays merit special attention. Glen Elder and Richard Rockwell's "The Depression Experience in Men's Lives" expands upon Elder's path-breaking study of the long-term effects of the Depression on the psychological development and family life of over one hundred adolescents. The present essay demonstrates that the effects were mitigated for those who experienced that event as children rather than as adolescents. The authors conclude that a critical historical event can have quite different effects on people depending on the life-cycle timing of its occurrence. John Modell, provocative as always, throws some cold water on our nostalgic admiration for the cohesion and cooperation of family members in times past. Modell details the enormous disruption of disease, death, and economic distress on families in the nineteenth century and infers that, given the extensive evidence on the darker side of cohesion and cooperation—family tension, working women and children, delayed marriage, sexual frustrations—much that we admire was externally mandated rather than internally motivated.

The second part of the volume contains the results of a series of workshops and colloquia on the value and use of photographs, interviews, and "family documents" in inspiring, researching, and writing family history. There is an extended introduction by the editor that helps impose order on the topical chaos.

LAURENCE GLASCO
University of Pittsburgh

EDMUND JEFFERSON DANZIGER, JR. *The Chippewas of Lake Superior*. (Civilization of the American Indian Series.) Norman: University of Oklahoma Press. 1978. Pp. xiv, 263. \$14.95.

The attention focused by the media on the Indians in recent years has created a climate favorable to intensified research on North American tribes. The resulting spate of histories of individual tribes evinces an attempt to foster a better understanding

of the events and forces that shaped contemporary Indian cultures. In contrast to the histories of only a few decades ago, these new studies draw upon anthropological and ethnological findings and offer more sensitive accounts of Indian cultures. Edmund Jefferson Danziger's study is one of the better examples of this trend.

Dividing the Chippewas into five major groups, Danziger limits the scope of his study to one group, the Lake Superior Chippewas, who live both on reservations and in off-reservation communities south and west of Lake Superior. An ethnohistory of the Lake Superior Chippewas from the first French contact at Sault Ste. Marie in 1641 to the present day, Danziger's work advances no new interpretations. It brings together much of the major research on the Chippewas and offers a solid account of the centuries of Chippewa-white interaction, especially the dealings of the United States government with the Lake Superior Chippewas. Of greatest value are the last chapters, which present Danziger's own synthesis of recent Chippewa history drawn largely from personal interviews with Chippewas.

In covering so long a period, Danziger paints his picture with broad strokes that too often treat cavalierly certain aspects of Chippewa history. For example, the Sioux did not recede in the face of Chippewa incursions into the upper Mississippi Valley as accommodatingly as he implies. But Danziger passes rapidly over inter- and intratribal contact to focus almost exclusively on European and American policy towards the Chippewas. His emphasis is partly dictated by the structure, which follows the process of Chippewa acculturation through the exposition of political and economic events. Much of the evidence of acculturation that Danziger advances, however, is not convincing. While he admits that it is difficult to determine "the rate of acculturation of each Lake Superior band" (p. 107), Danziger does seem to equate change with acculturation and cultural disintegration. As do all people, the Chippewas changed over time, but they did not therefore believe themselves to be any less Chippewa as a result. In fact, the Lake Superior Chippewas continued to cling to old ways far longer than was environmentally viable. Their intransigence led to poverty and to a "white paternalism" that Danziger finds "not unreasonable" (p. 113). Yet one could argue that this paternalism contributed to their continued dependence upon the government and to even greater poverty.

Danziger employs statistics to profile Chippewa economic and social conditions but without sufficiently analyzing their significance. Major concerns, such as Chippewa perception of government policy and the changes and constants of Chippewa culture, do not receive adequate attention. Although it

is possible to quibble with certain statements, in the final analysis Danziger has written a useful book for the study of Chippewa-white relations, especially during the twentieth century.

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RALPH J. COFFMAN. *Solomon Stoddard*. (Twayne's United States Authors Series.) Boston: Twayne. 1978. Pp. 224. \$9.95.

Solomon Stoddard, pastor of the Congregational church at Northampton, Massachusetts, from 1661 to 1729, was known in the nineteenth century mainly as the originator of the custom of admitting the unregenerate to full communion and as the grandfather of Jonathan Edwards, his successor at Northampton, who reversed his practice. In 1941 Perry Miller wrote a ground-breaking article on Stoddard. More recently, manuscripts of the poet Edward Taylor, pastor in neighboring Westfield, have been discovered that throw light on Stoddard and the communion controversy. Articles by Brooks Holifield, Paul Lucas, James Walsh, and several others have sought to reinterpret Stoddard in light of our expanding knowledge about Puritan theology and social history. The time is therefore ripe for a comprehensive treatment of Stoddard that will sum up these new developments and provide a base line for further scholarship.

Ralph J. Coffman's first chapter brings together practically everything that is known about Stoddard's family background, early years, and education; the second provides important data on Northampton's population and its social and political arrangements. Stoddard's career is seen as having had three successive phases. Each receives an appropriately entitled chapter: Stoddard the "Sacramentalist" tried to cure New England's moral and spiritual ills first by admitting the baptized but unconverted to the Lord's Supper as a "converting ordinance" and then, as "Pope of the Instituted Church," by advocating an essentially Presbyterian polity; finally, "The Evangelical" abandoned both and became exemplar and promoter of an aggressive evangelistic style of preaching that anticipated the Great Awakening.

Apparently aware of the pioneering nature of his biography, Coffman has tried to make it as serviceable as possible. A chronology of major events precedes the first chapter, and each of Stoddard's works (including unpublished manuscripts) is briefly described in its appropriate context. The appendixes contain early verse of Stoddard, a list of his library in 1664, his family tree, charts of "the Northampton Oligarchy," a biographical glossary,

a list of Stoddard's writings, and a select annotated bibliography. There is also an index of names (topics appear only under "Stoddard"). Coffman also cites many sources, such as archives, genealogical registers, manuscript collections, demographic studies, and works by Stoddard's contemporaries.

Unfortunately for Coffman's purpose, his book is poorly written, and typographical errors are legion. There are also gross and multiplied inaccuracies in proper names, titles of works, quotations, references, and bibliographical data and annotations. Interpretive statements contradict the adjoining quotations on which they are presumably based. Scores of examples could be cited.

At the level of interpretation, the author is simply not in command of his material. For example, his effort to demonstrate a later shift in Stoddard from intellectualism to voluntarism is sabotaged by Coffman's confusion of intellectualism with rationalism. Again, there is merit in Lucas's thesis that there were successive emphases in Stoddard's writings and that he became preoccupied with evangelism in his later years, but some aspects of it are debatable. Coffman simply takes the idea and runs with it, making the stages even more mutually exclusive by attributing to Stoddard in his second period an *opus operatum* sacramentalism, ignoring Stoddard's own statements and Holifield's excellent delineation of Puritan sacramental theology. Interpretive positions on which Coffman draws (Miller is another example) are seldom acknowledged and never critically discussed.

One must conclude that this biography is not a reliable guide to Stoddard, either in its broader interpretations or in its factual details.

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W. CLARK GILPIN. *The Millenarian Piety of Roger Williams*. Chicago: University of Chicago Press. 1979. Pp. viii, 214. \$17.00.

W. Clark Gilpin's volume continues the tradition, since Perry Miller, of treating Williams in terms of religious motivations. He argues that the unifying element of Williams's wide-ranging thought was his sense of religious vocation, interpreted within a distinctive "millenarian vision of history." Gilpin derives Williams's separatism from preoccupation with outward sanctification following conversion, while noting the similarity of his ideas to those of the General Baptists. He finds that Williams shared the widespread eschatological excitement of the Civil War period but distinguished himself from other Puritans and sectaries who found the millennial dawn in their own efforts to establish "pure" churches or spiritualized it as a personal transformation. For Williams, restoration of the true, visible

church had to await Christ's coming in the indefinite future. Until then, all churches are corrupt and all nations, civilized and savage, "heathen"; a Christian's proper calling is to prophetic "witness" against the corrupt mingling of spiritual and political realms.

These "Williamsian" notions are not unfamiliar. Gilpin, though, gives them coherent treatment, with attention to Williams's literary *corpus* and to controversial and religious currents that shaped them (though the latter occasionally crowds treatment of Williams himself). He demonstrates the pervasiveness of millenarian elements in Williams's thought, an important contribution. (Williams's millenarian consciousness, Gilpin maintains, emerged fully only after 1636, though it is not clear from evidence presented whether as the result of new influences on him or through development of motifs already present in his separatism.) He skirts the issue of Williams's relationship to contemporary "seekerism," a deficiency.

The force of the book is to suggest that Williams was fundamentally a millenarian. Yet this seems an inadequate impression of him as theologian and actor. His mature theology included a Reformed doctrine of nature and grace (shared with the Massachusetts divines), a perfectionist doctrine of sanctification (with radical implications for church membership), a restorationist doctrine of the church (with similar implications for church polity), a doctrine of Christ's spiritual lordship manifest in worldly weakness and humiliation (with implications for Christian discipleship and church-state relations), as well as a millenarian view of Christian history. Gilpin acknowledges this complexity but neglects Christology entirely, barely touches the issue of nature and grace, and tends to subordinate ecclesiology to eschatology. Williams's characteristic eschatological hope, however, seems the necessary reflex of his separatist-perfectionist ecclesiology, which constituted the abiding concern of his life. Gilpin says nearly as much (p. 159), yet his title and treatment imply that eschatology, as such, was dominant. The actions for which Williams is justly remembered, however, appear to have been motivated less by a longing for Christ's coming than by an earnest seeking after, and failing to find, the true church of Christ's institution.

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CECELIA TICHİ. *New World, New Earth: Environmental Reform in American Literature from the Puritans through Whitman*. New Haven, Conn.: Yale University Press. 1979. Pp. xii, 290. \$18.50.

A few years ago an advertisement for an American engineering company ran in many national maga-

zines. Two hard-hatted workers gazed across an alpine scene to a high snow peak; "O.K.," the text of the ad began, "where do you want it moved?"

The idea that Americans are divinely appointed and culturally prepared to move mountains, clear forests, drain swamps, and make the desert blossom like the rose is the subject of *New World, New Earth*. Cecilia Tichi's meticulous account does not extend to contemporary advertisements. She explores instead the roots of the concept that the wild New World environment must be reformed (literally reformed) so as to become the promised land of American millennial expectations. The New World would thus become the New Earth.

The genesis of this work appears to be the author's displeasure with the way the "ecology" movement of the 1960s altered the reputation of American pioneers. Instead of the heroes they were once held to be, frontiersmen, beginning with the Pilgrims and Puritans, were seen as ecologically blind exploiters who selfishly ravaged the virgin land. Unfair, responds Tichi. In their own minds at least the pioneers' alteration of the North American continent was a heaven-appointed task of world importance. In order for the perfection of man and society to proceed, the environment had to be redeemed from its wild condition. The transformation of the land, like the displacement of the Indian, was a work of God. Tichi's adept textual analysis traces these ideas in the work of Edward Johnson, Joel Barlow, and Walt Whitman, each of whom receive entire chapters. Figures of lesser importance, such as George Bancroft, and partial dissenters like Henry David Thoreau, are also well handled.

The revisionist element in *New World, New Earth* has to do with motives. Greed, fear, and hatred, says Tichi, took second place to idealism in explaining the drive of early Americans to vanquish the wilderness. Such reassessment is important. The 1960s was unfair in shaking the finger of moral reproach at previous generations. It is logical that the 1760s perception of such things as population growth, deforestation, and the extermination of wildlife would be radically different from that of contemporary Americans. Still one can not suppress an occasional suspicion that Tichi has been taken in by the rhetoric of her subjects. The idealism she describes might be viewed as intellectual oil to grease the wheels of a very materially oriented process. Just as Manifest Destiny rationalized the bullying of Indians and Mexicans, so the ideal of a new earth smoothed over anxieties concerning the wholesale alteration of the environment. As Bob Dylan recognized, Americans have been adept at putting God on their side.

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DAVID HOROWITZ. *The First Frontier: The Indian Wars and America's Origins, 1607-1776*. New York: Simon and Schuster. 1978. Pp. 251. \$9.95.

The author of this slim, fast-paced volume believes that Americans have repressed the memory of their origins, which were above all else steeped in conflict with native occupiers of the land. To correct this historical amnesia, David Horowitz has written a breezy account of the first 150 years of American history, beginning with the Pilgrim arrival at Plymouth and culminating with independence. The book is largely narrative history. It details, usually with factual accuracy, the conflict with tribes in New England and Virginia in the founding years, the wars in 1675 that all but ended Indian resistance along the coastal plain in both the north and south, and the eighteenth-century wars for empire that left the powerful interior tribes bereft of a European ally to play off against the Anglo-Americans.

Considering the outpouring of work on Indian-white relations in the last fifteen years, one cannot claim that Horowitz has broken new ground. But it is possible that he will bring an interpretation that is well grounded in recent scholarship to the attention of those in the history-reading public who like their accounts of the past ungarnished by footnotes and unencumbered by analysis.

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GARY M. WALTON and JAMES F. SHEPHERD. *The Economic Rise of Early America*. New York: Cambridge University Press. 1979. Pp. x, 226. Cloth \$19.95, paper \$4.95.

This survey sketches commercial developments, not general trends, in the British colonies of North America from the fifteenth-century background to 1792. Gary M. Walton and James F. Shepherd believe that foreign trade provides the key to understanding the economic growth that they feel occurred, and they think that recent research justifies a new synthesis, "a supplement to larger texts." They draw almost exclusively on books and articles by economic historians.

After a short introductory chapter, they take the reader quickly through the period of discovery and conquest, arguing for the critical importance of silver on the European economy. The next five chapters of one hundred and fifty pages contain the heart of the book and deal with colonization, the "ascent" of foreign trade, and economic "progress." A final fifty pages deal briefly with some economic aspects of the revolution and its results, stopping be-

fore the wars of the 1790s. Factually, the heart of the argument rests upon limited trade statistics during 1768-72 and 1791-92, which Walton and Shepherd have used in earlier publications. The authors insist that the colonists' wealth and standard of living grew, especially in the seventeenth century, due primarily to the commercial sector. They also emphasize increasing inequality as a result of commercialization.

A good editor would have improved the writing style and eliminated some errors. Among others, Irene "Niu" spells her name "Neu" (p. x), and it is "Bruchey" not "Bruckey" (p. 95). North America includes Mexico, so statements on pages 34 and 37 are false. All of the colonies south of Pennsylvania had resident merchants (p. 47). The statement about mortality rates (pp. 54-55) is not true of the coastal south. Freed indentured servants did not receive mules (p. 58). The British did not enforce energetically or effectively the acts of 1764-65 (p. 69). Pontiac's Rebellion did not precede but followed the Proclamation of 1763, a fact that changes its origin (pp. 162-63). The upper house in the colonies was not called "The Lords" (p. 154). The Sugar Act contained far more than a modification of the Molasses Act (p. 164). The Currency Act did not prohibit all paper money (p. 172).

Some historians will question certain interpretations. The emphasis on foreign commerce and commercial activities leads the authors to minimize the significance of agriculture and manufacturing and virtually to omit any description of colonial currency (for example, pp. 48, 88). As a result, the actual economic situation becomes distorted. Proof is needed that colonial mobility of labor and capital exceeded that in Europe (p. 115). The section on growth (pp. 140-42) is entirely speculative. The discussion of income and wealth rests upon very shaky foundations, and there is at least as much evidence on the other side; the authors present as fact what is only hypothetical.

In all fairness, the authors are not addressing historians but undergraduates. For students of economics who need some history, the book's limitations may not matter, and they will profit from its discussion of trade. History majors would require careful editing, broader coverage, and the incorporation of additional scholarly work.

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JOHN PHILLIP REID. *In a Rebellious Spirit: The Argument of Facts, the Liberty Riot, and the Coming of the American Revolution*. University Park: Pennsylvania State University Press. 1979. Pp. 168. \$12.95.

In a Rebellious Spirit is the latest in a series of monographs by John Phillip Reid dealing with legal conditions, forensic rhetoric, and political machinations in Massachusetts during the pre-Revolutionary decade. It focuses upon the legal and rhetorical skirmishing in Boston between British officials and Whig leaders that preceded the stationing of two regiments of troops in the town in October 1768.

In a point well taken, Reid argues that the letters, speeches, reports, and pamphlets issued by each side can only be properly understood within the framework of eighteenth-century English and colonial law and must be seen as persuasive documents designed not to record with strict objectivity and accuracy the state of public affairs but to impress upon officials in the mother country a particular interpretation of what was happening in Boston and what ought to be done about it. The aim of British officials—especially Governor Bernard and a clique of customs officers—was to portray Boston as a city utterly under the control of radical Whigs who had so reduced it to a state of lawlessness that the normal operations of government could not proceed without the protection of regular troops. Reid demonstrates how Bernard and the customs commissioners manipulated evidence, distorted events, and created “facts” in order to circumvent the Massachusetts constitution and persuade the ministry that soldiers must be sent to Boston.

Throughout the book Reid is highly critical of the motives and discourse of Bernard and the customs officers. So much so, in fact, that he tends to overlook the argumentative defects and exhortative excesses of the resistance movement. Reid's sympathies clearly lie with the Boston Whigs, and he affirms their allegation that quartering troops in Boston was not really necessary to keep the peace and resulted instead from an assiduously executed plot by Bernard and the customs commissioners. Although Reid does not treat either the course of events in Boston from 1766 to 1768 or the motives of Bernard and the commissioners in their full complexity, his case for conspiracy is at least plausible and deserves attention at a time when much historical scholarship has become sympathetic with the Tory view of the Revolution.

Reid's assumptions also warrant notice. He understands that the meaning of events—and their significance for the historian—rests, not in the events themselves, but in the perceptions of those people who participate in, observe, or respond to them. He also reminds us of the necessity of approaching the public documents of men like Bernard, Samuel Adams, and James Otis as what they were in their own time: rhetorical discourses designed to impose particular constructions of reality upon their readers. Such an approach is not new, of course, but the more it is employed, the better we

are likely to understand the essentially rhetorical origins of the American Revolution.

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DONALD WALLACE WHITE. *A Village at War: Chatham, New Jersey, and the American Revolution*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1979. Pp. 310. \$17.50.

The comparatively undistinguished village treated in this book has been the subject of half a dozen histories. Evidently Chathamites enjoy both reading and writing about their town. Donald Wallace White, a former resident and graduate student in history, was asked to write this book as a contribution to the bicentennial, although a general history had appeared in 1967.

The author chose as models recent New England town studies. He carefully combed through printed and manuscript state and local materials and prepared a study complete with scholarly apparatus. He identifies the 288 residents of Chatham during the 1775–83 period and in a series of tables summarizes data on the consequences of the Revolution for this hamlet. Some of the more intriguing points are the effect of the Revolution on marriages; the value of a newspaper in awakening the local people to a wider world; and the economic consequences, although the author weakens this section by confusing wealth with income.

Chatham was before 1806 divided politically among two counties and four townships; consequently, the political history during the Revolution is awkward to handle. White does not tell us if the Revolution encouraged unification of this locality. On political effects he seems contradictory, finding that after the Revolution “new Faces” came into a “remarkably free and democratic government” (p. 199), while representation and suffrage were “little affected” (p. 215).

White provides data on the religion (Presbyterian), occupations (mostly farmers), and economic status of the Chatham soldiers (pp. 66–67), though without enough attention to whether the Revolution was, as other studies indicate, a poor man's fight. He should have explained the contribution of this locality in light of New Jersey's low proportion of continentals raised and rather disorganized militia.

He might also have studied the general New Jersey circumstances carefully. That taxes in 1780 were incredible and impossible (p. 143) was not the view of the inhabitants; the percentage collected was relatively high. And a couple of minor points: no one had “to pay five hundred pounds of New Jersey

money" to obtain a marriage license (p. 158)—as a review of the clandestine marriage act would clarify; nor was Robert Ogden a Tory at the outbreak of the Revolution (p. 57).

Residents of Chatham should enjoy the book. Students and scholars will more profitably turn to Robert A. Gross's study of Concord for a depiction of the American Revolution in the small town.

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DONALD JACKSON and DOROTHY TWOHIG, editors. *The Diaries of George Washington*. Volume 3, 1771–75, 1780–81; volume 4, 1784–June 1786. Charlottesville: University Press of Virginia. 1978. Pp. xv, 494; xv, 405. \$20.00; \$15.00.

In March 1785 Washington referred to his work at Mount Vernon as his singular "amusement," which is what Donald Jackson and Dorothy Twohig continue to provide readers of Washington's diaries. These volumes stand in welcome contrast to the growing colorlessness that has become the hallmark of too many documentary editions. In the oftentimes admirable quest to eliminate undesirable editorial intrusion and to hold down costs, some editors have lost sight of the basic ingredients necessary to produce a good historical show. Jackson and Twohig have not.

With two notable exceptions in 1781 and 1785, the diaries remain terse to the point of being drab. The editors infuse the spare entries with a concoction of information. The result is a private narrative, as differentiated from an exchange of letters, made vivid by historical imaginations bent upon having Washington "come alive." The editors established early that the identification of individuals Washington named was of central importance. For the specialized or the general reader this information gives added human dimension to Washington's society, to his travels, and ultimately to his values. Whether he is meeting with his Pennsylvania tenants in 1784, Jean Antoine Houdon and Mrs. Catherine Macauley Graham in 1785, or Philadelphia's leading citizens in 1772, Washington resolutely refused to imbibe in the kind of personality sketching that titillates so many diary readers. Jackson and Twohig avoid the sin of editorial gossip and, unless there is clear evidence, do not postulate Washington's assessments of other people.

The other important annotation device involves the use of Washington's own correspondence as commentary for diary entries. Simple enough in conception, the task requires painstaking care in research and selection. Perhaps the most moving example appeared when Patsy Custis died, an event that brought forth only the briefest mention in the

diary and yet a loss that Washington's correspondence clearly shows as having moved him deeply. In a lighter vein, the editors' selection of various comments following the arrival of a jackass sent from the King of Spain highlight Washington's agricultural interests as well as his humor when he wondered if "Royal Gift" would respond to republican charms. The editors manage these selections in such an unobtrusive manner that the annotation becomes an almost integral part of the diary. Finally the editors utilize annotation to familiarize the reader with significant events. The drama of the spring and summer of 1774 would be lost without the notes, as would the significance of the day Congress voted to install Washington as Commander-in-Chief. The editors resort only once to a headnote, that increasingly popular expression of editorial virtuosity. Add to this the editors' steadfast and courteous refusal to exploit annotation for historiographic pyrotechnics, and a rather full-blown annotation policy for Washington's diary becomes clear. Presumably each reader will find certain areas of annotation either lacking or excessive. Given my pack-rat proclivities, even more illustrations, profuse and useful as they are, would be welcome. Of greater consequence, the maps in the first two volumes can be used in conjunction with these volumes, even though the editors make no references.

Inevitably questions are raised regarding the historiographic thrust of annotation, particularly when it occupies a central place as it does here. Jackson and Twohig, following the natural lead of the material, accentuate Washington's private side—family relationships, friendships, social and cultural concerns, and his sense of the land. Absent are discursive comments about Washington's military leadership, his financial interests in opening navigation to the Ohio, and the resumption of his political interests after the war. Throughout these volumes there is an implicit caveat against inductive leaps. The result is a delight for readers, regardless of their interests. In a time when the function of professional historians is increasingly called into question, these volumes stand as apt illustration of the assertion that competent professionalism can produce a strikingly evocative view of the past, in this instance, Washington during his most energetic years.

GEORGE M. CURTIS, III
Papers of John Marshall

BURTON SPIVAK. *Jefferson's English Crisis: Commerce, Embargo, and the Republican Revolution*. Charlottesville: University Press of Virginia. 1979. Pp. xiii, 250. \$17.50.

Thomas Jefferson's two terms as President present a striking contrast. The first four years, marked by

domestic policy successes and the purchase of Louisiana, culminated in a re-election victory that nearly crushed the opposition party. The second term was filled with mounting disappointment and capped by costly failure. The embargo shattered hopes of harmony between the sections and revitalized the opposition. Its repeal demoralized the president, weakened his successor, and suggested that the warring European empires could continue to pursue restrictive policies without provoking an effective American response.

This volume seeks the sources of Jefferson's failure either to avoid or to resolve the crisis in Anglo-American relations that developed after 1803 with the renewal of the Napoleonic wars. The emphasis is on the changing course of the administration's English policy as it was influenced by political considerations, personality, and—most especially—republican assumptions and ideals. As Burton Spivak sees it, the United States was lured into a full-blown crisis by the administration's determination to preserve the wartime carrying trade, which was both highly lucrative and crucial to the effort to conciliate New England even if at odds with some of the most basic Jeffersonian beliefs about a sound republican society. Strained by the effort to protect a kind of commerce he had never thoroughly approved and readily convinced of the malevolence of British policy, Jefferson was ready to resort to war in the aftermath of the *Chesapeake* affair. But his decision not to call an early session of the Congress prevented preparations and cooled the country's anger. When Congress did convene, new French and British edicts posed a danger that was met by the embargo.

Spivak argues that, for Jefferson himself, the embargo was originally intended as a temporary measure, a traditional preface to hostilities. Only gradually did Jefferson accept James Madison's conception of an indefinite embargo as a method of coercing France and Britain. As he did, however, he increasingly envisioned the embargo as an agency for shaping a healthier republican economy and as a test of the success of the republican polity. Confronted by its failure in his final weeks in office, he could neither give it up nor bring himself to recommend a war.

Non-specialists will find this study difficult to follow. It is not well organized or written. Specialists are likely to resist the most revisionary aspects of the argument. I think it over-emphasizes the role of the defense of the re-export trade and simplifies the progress of Jefferson's thought, while making him too ready to resort to war. Yet *Jefferson's English Crisis* usefully explains the way in which the Jeffersonians combined (perhaps confused) two different policy traditions—non-intercourse as peacetime method of coercion and embargo as defensive pref-

ace to a war—and Spivak offers a revealing illustration of the complexity of Republican thought and practice.

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PHILIP L. WHITE. *Beekmantown, New York: Forest Frontier to Farm Community*. Austin: University of Texas Press. 1979. Pp. xv, 378. \$24.95.

This is a detailed description and analysis of a Lake Champlain frontier community over two generations, extending roughly from 1790 to 1850. Lack of relevant data precludes a demographical analysis, yet on the basis of tax records, decennial federal census returns, and other material, Philip L. White poses several provocative hypotheses on the influence of the heavily forested frontier. The economic advantages of lumbering and related industries attracted a first generation of settlers relatively high in socioeconomic status. The original proprietors, the Beekman family, failing to dispose of their tract in large blocks for a quick profit, decided to sell or lease land in farm-lot pieces to individual settlers. Competition from other proprietors forced the Beekmans to be reasonable, indeed, even compelled them to accept violations by settlers of the terms of their agreements. Many of the earlier settlers were businessmen and entrepreneurs as well as laborers and farmers.

Through the first generation of settlement the marketing of forest products sustained an expansive, prosperous economy. From the mid-1820s on, approaching exhaustion of marketable timber sharply curtailed prosperity, altered the economy, and drove the upwardly mobile, those with resources, to move. The men who remained in Beekmantown evidently lacked capital for a move west or the experience necessary to take advantage of opportunities elsewhere. While relatively poor French Canadians and Irish Catholics moved into the region about Lake Champlain, immigrants from New England and other areas in New York seemed to prefer the more rewarding opportunities offered farther west where land was cheaper, more likely to rise in value, and more productive. The result of these shifts in population and the exhaustion of the forest frontier was evident in the census of 1850. Land in Beekmantown was less valuable than land elsewhere in the state. The size of farms was "considerable"—White's term—but relatively few farms were either very small or very large. The twenty poorest farmers held from 6 to 50 acres, the wealthiest from 90 to 250. Only four held over 177 acres of improved land. Disparities in the size of individual land holdings were much less than during the first generation.

Not surprisingly, the dominant political loyalties of the region were Jeffersonian Republican and Jacksonian Democrat, reflecting a provincial backwater rather than cosmopolitan community. The economic lot of the common people improved, White concludes, despite the economic decline. Beekmantown was a more egalitarian society. One wonders if this might not have been an equality of near poverty. Unfortunately, the hard evidence on which to base these conclusions depends on early tax records and the federal census. How reliable are these? How typical was Beekmantown, Clinton County, and the Lake Champlain frontier? Other frontier regions less dependent on timber and blessed with soil more conducive to agriculture might show a different pattern.

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HOWARD B. ROCK. *Artisans of the New Republic: The Tradesmen of New York City in the Age of Jefferson*. New York: New York University Press. 1979. Pp. xviii, 340. \$17.50.

This is an exceedingly good and useful book. Howard B. Rock has studied New York's mechanics during the early national period (1801-19) with care and clarity. The research is thorough, the analysis is balanced, the presentation is effective. Rock provides an excellent microcosmic elaboration and undergirding for Alfred Young's seminal *Democratic-Republicans of New York* (1967).

New York's early national mechanics participated in partisan politics as a means to advance their status in the community. Their general preference among national political parties was for the Jeffersonian Republicans, a preference based on both ideological and practical considerations. The Federalists' elitist leanings repelled the artisans, but practical, local political issues related to conflicting party ideals were equally influential in determining their votes. The Republicans represented opportunity for the common man—opportunity to participate in society and government as the equals of the gentry and to leave behind the deference of the eighteenth century, opportunity to effect suffrage reform, opportunity to hold significant political offices, and opportunity to support social and economic policies that advanced their well-being. The choices were not always easy: during the Embargo and the War of 1812 the tradesmen had a difficult choice to make "between principle and pocketbook" (p. 89), and frequently local issues and local politicians presented problems that disillusioned the urban working class. Yet Rock's detailed analysis never loses sight of the long-range commitments.

It is clear that democracy for the common man in New York did not have to await the advent of the Age of Jackson.

Rock's analysis of economic history is, if anything, more perceptive than his political history. He finds great diversity in the artisan community, not only between masters and journeymen but also between those who adopted emerging modern techniques of production and marketing and those who did not. For the artisan, "the central meaning of the Revolution, together with the fulfillment of republican ideals, was the opportunity for unfettered entrance into the marketplace" (p. 152)—an opportunity they explored and advanced aggressively in the early nineteenth century. They borrowed corporate business models from merchants, they experimented with modern capitalist credit schemes, they organized to support tariffs, they both established monopolies and fought against monopolies, they campaigned for favorable governmental policies, they caused labor unrest and used primitive collective techniques, and they even developed a new "work culture and moral outlook" (p. 295). Rock shows that these developments had roots in a struggle for economic leverage, but he is equally clear that the mechanic was not a mere pawn of his own economic striving: "standing in society" and "striving against deferential expectations and contemptuous treatment" (p. 279) were just as important to him.

Rock's book will be a standard reference for a long time. Its analysis is complete, it uses both traditional and novel source material, it balances modern quantitative methods with literary, biographical, and visual analysis, and it argues the case for the new social history without rejecting the considerable contributions made to our knowledge by the old.

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WILLIAM R. BROCK. *Parties and Political Conscience: American Dilemmas, 1840-1850*. (KTO Studies in American History.) Millwood, N.Y.: KTO Press. 1979. Pp. xvi, 367. \$20.95.

Parties and Political Conscience may be the best single-volume history of national politics in the 1840s ever written. It is graced by acute portraits of major personalities, lucid dissection of issues and opinion, and brilliant comparative insights into the ways the American constitutional system influenced the policy-making process and the workings of political parties.

William R. Brock is concerned with the collapse of the second party system, and his thesis hinges on the centrality in mass political behavior of moral attitudes, or "political conscience." The intense loy-

alty American voters developed toward the Whig and Democratic parties by 1840, he argues, was rooted in differing interpretations of the nation's moral purpose and in a belief that the rival parties offered alternative ways to effect it. A series of developments during the 1840s—the failure of the Whig majority to enact its economic program in 1841–42, the timid inflexibility of both parties when confronted with changing social and economic conditions, the outbreak of the Mexican War, and the virtual cessation of policy output by decade's end—eroded voters' confidence that the parties could meet their moral expectations.

Above all else, Brock contends, the emergence of the slavery extension issue raised fears that the nation's moral purpose was threatened and turned men against the parties. As Northerners and Southerners became convinced that their respective views of the proper moral order could be obtained only through the prohibition or achievement of slavery expansion, more and more of them prepared to jettison the national parties as useless. Whereas most historians locate this decision in the 1850s, Brock insists that disillusionment developed earlier. Events between 1844 and 1846 formed a critical turning point from which the parties could never recover. By 1848, "most thinking and humane men" (p. 153) correctly regarded the party system as bankrupt. The Compromise of 1850 only confirmed their suspicions, he asserts, because it sacrificed political conscience for expediency.

Although much of Brock's analysis is thus a forceful reassertion of the primacy of the slavery issue in destroying the second party system, historians will find much that is fresh and worthy of serious attention here. Certainly his imaginative attempt to link so many disparate developments as solvents of public confidence in the party system merits applause. Yet one can agree that such a loss of confidence ultimately destroyed the system without agreeing that it occurred as early as he says or that antagonistic sectional attitudes toward slavery were as disproportionately important in causing it as he asserts. For one thing, many of his boldest generalizations about public opinion lack evidence and are debatable. For another, he exaggerates the weakness of the major parties with the electorate in the 1840s by inflating the importance of atypical dissidents from the system like Conscience Whigs, Barnburners, and intellectuals. Finally, his narrow focus on Congress and national politics blinds him to the role that state-level political conflict played in reinforcing voter loyalty throughout the 1840s. Despite the vigorous arguments of this important book, in short, a case can still be made that it was only in the 1850s, when the old parties were viewed as useless at all levels of the federal system and not just by

men who gave priority to the slavery issue, that the system collapsed.

MICHAEL F. HOLT
University of Virginia

TAYLOR STOEHR. *Nay-Saying in Concord: Emerson, Alcott, and Thoreau*. Hamden, Conn.: Archon Books. 1979. Pp. 179. \$14.50.

Recently, the best books on the Concord Transcendentalists as a group (I'm thinking primarily of Lawrence Buell's *Literary Transcendentalism*) have concentrated on their strategies as writers. Only biographies and studies of individual figures have really examined their thoughts and deeds.

Taylor Stoehr, author of *Hawthorne's Mad Scientists* and editor and authorized biographer of Paul Goodman, takes the rather old-fashioned view that "The transcendentalists' first question was not how to write or speak, but how to live" (p. 149). Even more old-fashioned, perhaps, he seeks the meaning of their lives for our own, as leaders of an earlier "counterculture movement" against injustice, alienation, and commercialism.

The focus, therefore, is on the three general issues in the lives of Emerson, Alcott, and Thoreau that Stoehr thinks most relevant to the counterculture of the 1960s as it survives today. Stoehr's choices are civil disobedience and the nonpayment of taxes; utopian communities and attitudes towards division of labor, property, and family; and, finally, "Food for Thought," or the relation of vegetarianism and dietary self-denial to introspection and the "new consciousness"—a phrase Emerson used in "Historic Notes of Life and Letters in New England" (p. 119). By comparing the different positions that these three men took on these issues, Stoehr manages not only to make their experiences instructive but also to give us sharp and sometimes humorous insight into their characters. Taking his evidence from journals, letters, and early memoirs (and almost ignoring modern biographies and criticism), he rediscovers the freshness and limpidity of their original behavior.

What Stoehr finds that they had in common is perhaps the most important message of the book. Their achievements, he feels, were all made "by virtue of fierce self-discipline and restraint." They drew back and revolted against "a world of *too much*"—"too many careers, too many possessions and pleasures, too much complexity and ramification, too much leisure in which to think about all these alternatives, and to dream up more" (p. 155). Their nay-saying, therefore, was not Melville's cosmic "NO! in thunder" or the sense of history and tragedy through which Hawthorne viewed Ameri-

can optimism. It was more fundamental. It was conservative self-denial and voluntary abnegation from a world going crazy over getting and spending.

This reading of Emerson, Alcott, and Thoreau should alarm anyone who still sees them as soft-headed idealists and thinks that there is greater virtue in Hawthorne's, Melville's, and Poe's "Power of Blackness." To call the Transcendentalists "naysayers" is indeed a radical countermove in the old critical debate. Yet Stoehr does not make it just for effect or from infatuation with the modern counter-culture. On the contrary, he argues that modern students desperately need the Transcendentalists' lesson of "refusals and abstinence."

This is a challenging, timely book.

ROBERT F. SAYRE
University of Iowa

HERMAN BELZ. *Emancipation and Equal Rights: Politics and Constitutionalism in the Civil War Era*. (Norton Essays in American History.) New York: W. W. Norton. 1978. Pp. xviii, 171. Cloth \$10.95, paper \$3.95.

Without any doubt the author of this slim volume has fulfilled with distinction the purpose of the Norton Essays in American History. The continuing series edited by Harold M. Hyman seeks to provide concise interpretations of certain aspects of American History.

There is no pretence that this is an original research contribution. As an interpretive study, however, it is based on such research completed by the author in earlier works and on the very real contributions to knowledge by recognized scholars in the field of the Civil War and Reconstruction. Footnotes indicate specific indebtedness. A bibliography and index are especially useful to those not already acquainted with the period and topic.

Conclusions are clearly stated. Herman Belz contends that truly "significant nationalizing changes" (p. 142) did take place as a result of Republican reconstruction and that they maintained the essence of a federal state with state rights that conformed to the basic concepts of nineteenth-century thought. In these respects the author claims that reconstruction was successful not only in restoring the Union but also in confirming the emancipation of former slaves with equal legal rights in a free society. He recognizes the purpose of political expediency that prompted many acts but argues that they were not less just because of mixed motives.

Although recognizing that many achievements in reconstruction were only temporary and inadequate, the author contends that they formed the essential bases of greater results in the twentieth

century. According to this interpretation, Republicans succeeded rather than failed because the increment of change was of great and permanent significance. Racial prejudice existed but was diminished by events of the Civil War era. As measured by nineteenth-century conditions and outlooks, progress was made despite the apparent failure of much that was then initiated. Even "segregation was a decided gain" (p. 147) as compared with treatment of blacks before the Civil War.

Readers are warned against judging events and results of the period by modern standards. In the context of the 1860s a large impact was apparent. Thus the success of the Republican program is declared to be more than simply foundation for the future.

This interpretive work is literally crammed with facts, but they do not interfere with a casual reading. The secret seems to lie in a smooth and clear style that uses the mass of detail to contribute in an interesting way to the interpretation. Because it is an interpretive study, there will be those who disagree with certain conclusions, but none can deny the careful marshaling of facts to support the author's views.

Probably because of Belz's earlier studies of the Civil War era, he has devoted a disproportionate share of this work to the war years, when theory and basic attitudes toward reconstruction were evolving. Nevertheless, the postwar years are adequately treated as related to his purpose. Criticisms such as this and certain questions of the complete accuracy of some statements may be raised, but they are few. In different ways the book is interesting and useful to both layman and scholar.

JOSEPH B. JAMES
Wesleyan College

PATRICK W. RIDDLEBERGER. *1866: The Critical Year Revisited*. Carbondale: Southern Illinois University Press. 1979. Pp. xiii, 287. \$18.95.

Almost fifty years ago, Howard K. Beale published *The Critical Year: A Study of Andrew Johnson and Reconstruction*. A well-written indictment of radical policies, the book sought to demonstrate that Andrew Johnson's defeat was due to his failure to exploit the alleged underlying economic differences between the radicals and their opponents and that racial problems were only of secondary importance. Since that time, many revisionist writers, inspired in part by Beale's own call for a re-evaluation of Reconstruction historiography, have not only refuted most of his contentions but reasserted the justice and sincerity of the radical cause. Now Patrick W. Riddleberger, the biographer of George W. Julian, has

sought to re-examine Beale's thesis in a new book, 1866: *The Critical Year Revisited*.

In general, the author has succeeded very well. Reviewing the political and economic history of the period, he tends to reinforce the findings of the revisionists, especially those of Stanley Coben, Eric L. McKittrick, and LaWanda and John H. Cox. Thus he once again emphasizes the importance of the political and racial impact of Johnson's policies. His expertise in tracing the development of presidential Reconstruction, its threat to the supremacy of the Republican Party, and its inevitable alienation of the moderates, is commendable. A detailed analysis of the Civil Rights Act and the Fourteenth Amendment, as well as a meticulous account of the Memphis and New Orleans riots, serve to clarify the growing rift between president and Congress.

In view of the time that has passed since Beale first wrote his volume and the many revisionist works that have appeared since, the present study serves as a convenient summation of modern scholarship. If it adds little that is new, it nevertheless re-emphasizes the total inadequacy of Johnson's policy. As Riddleberger correctly reminds us, Abraham Lincoln, far from being as interested in speedy restoration as his successor, was infinitely more flexible and had long been moving toward his radical critics. The author also points out that the lengthy debates concerning the Fourteenth Amendment showed that the Civil War did not result in as much centralization as is usually thought.

What Riddleberger fails to explain is the reason for Johnson's refusal to come to terms with the moderates. That he might have prevailed by cooperating with such congressional leaders as Lyman Trumbull and William Pitt Fessenden is evident, but in view of his determination to keep the South "a white man's country" he could hardly be expected to do so. It was precisely this determination that led him to defy such moderate proposals as the Freedmen's Bureau Bill and the amendment. Only after he had succeeded in thoroughly undermining Reconstruction did he finally enter into compromises with the moderates, making agreements that resulted in his acquittal in the impeachment trial. But by that time he had already encouraged southern conservatives to such a degree that he contributed greatly toward the ultimate triumph of reaction.

Notwithstanding this oversight, the book is a valuable addition to the historiography of Reconstruction. Its lively style and attractive format ought to guarantee it the wide readership it deserves.

HANS L. TREPUSSE

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ROBERT F. HOROWITZ. *The Great Impeacher: A Political Biography of James M. Ashley*. New York: Brooklyn

College Press; distributed by Columbia University Press, New York. 1979. Pp. xii, 227. \$15.00.

The subtitle of this volume accurately describes its contents, for it is primarily a narrative account of James Ashley's political career with heavy emphasis on his years in the House of Representatives. Since the only other biography of this controversial and enigmatic figure was written by the congressman's son and published in 1907, this book fills some long-standing gaps.

The author has two major objectives in *The Great Impeacher*. First, his title notwithstanding, Robert F. Horowitz wants to demonstrate that Ashley should be remembered for his significant role in the history of Radical Republicanism writ large, not merely as the earliest and most aggressive Congressional advocate of impeaching Andrew Johnson. Second, Horowitz is out to defend Ashley against the charge of being "a nut with an *idée fixe*" (as C. Van Woodward phrased it) on the subject of trying to remove the president.

Horowitz is reasonably successful in meeting his first goal. The sections on Ashley's espousal of black rights, on Ashley as a political realist driving the Thirteenth Amendment through Congress, and on Ashley as a pioneer in the formulation of reconstruction policy are the strongest in the book. Horowitz is less successful in defense of Ashley the impeacher. Early in the biography Horowitz hints at a psychohistorical explanation of Ashley's patricidal behavior, but the hints are not followed up. Horowitz is able to document Ashley's longstanding fear of executive tyranny, but that alone, however fashionable in the 1970s, hardly justified a course in the 1860s that embarrassed even Ashley's closest ideological allies. When the successful impeachment finally began, Ashley's fellow Republicans tried to freeze him out of a leadership position in the proceeding, and the Ohio State Republican Committee refused to help him run for re-election in 1868. Since Ashley almost won that race anyway, there may be a case for the Toledo Congressman's actions after all. But the case Horowitz makes is inconsistent and apologetic.

Because Ashley's private papers burned during his lifetime, Horowitz has relied primarily on the public record, newspapers, other histories, an autobiographical memoir written late in Ashley's life, and letters culled from collateral manuscript collections. His research, especially in the collateral collections, is impressive, and he deserves credit for uncovering as much as he has. Nonetheless, James Ashley, a maverick even by the standards of the most tumultuous period of American history, remains a controversial and enigmatic figure.

JAMES C. MOHR
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WILLIAM C. HARRIS. *The Day of the Carpetbagger: Republican Reconstruction in Mississippi*. Baton Rouge: Louisiana State University Press. 1979. Pp. xiv, 760. \$37.00.

James W. Garner's *Reconstruction in Mississippi* (1901) has long been considered not only the standard treatment of the subject but also the best of the state studies by members of the "Dunning school." In one book Garner covered the Civil War and both presidential and congressional reconstruction. Now William C. Harris, having already produced *Presidential Reconstruction in Mississippi* (1967), devotes the whole of a huge volume to congressional reconstruction in the state. His two volumes constitute a prodigious work, much the most circumstantial account of statewide reconstruction anywhere in the South. To the period 1867-1876 he gives three times as much space as Garner gave. One may question, however, whether he adds proportionally to historical truth.

Harris has "not attempted a detailed account of the social and cultural history of blacks," he explains (p. xii), but has "sought to weave black activities into the general story." In telling the general story, he devotes separate chapters to education, economic developments, and "country and town life," including such sidelights as the rising popularity of baseball after its introduction by Union troops. He shows that, in the larger towns, "Republicans and conservatives, blacks and whites, northerners and natives" were "in favor of economic growth and the construction of railroads" (p. 286). He deals with issues that arose independently of the reconstruction question—and that also agitated contemporary Wisconsin and other northern states—such as the promotion of railroads for the exploitation of timber, the use of school lands to subsidize them, railroad regulation, temperance, compulsory school attendance, prisons and the convict lease system, and pauper relief. In his excellent handling of these topics, he makes his most valuable contribution, going far beyond Garner.

In recounting politics Harris relies very heavily on Democratic or conservative newspapers, especially the *Jackson Clarion* of Ethelbert Barksdale, whom he describes as "the most vigorous foe of the 'mongrelization' of the state" (pp. 131-32). He quotes a carpetbag editor who justified the use of falsehood in a good cause, but he seems oblivious of the Democratic and conservative editors' widespread resort to the tactic of the big lie. He even cites the self-serving history by John S. McNeily, one of the most rabid of all the white supremacists. On the other hand, he slights or ignores the testimony of regular Republicans, in particular the testimony they gave before the congressional committee investigating the 1875 election.

Quoting and paraphrasing conservative spokes-

men at length, and adopting their words and their views as his own, Harris makes the carpetbagger Adelbert Ames and the Ames Republicans the villains of the Mississippi story. The "opportunistic" Ames had an "almost paranoid attitude toward 'rebel democrats'" (p. 54) and in 1869 "played the role of military despot" (p. 253). His career reveals the "intermingling of political ambition and wartime idealism" (p. 93). He "opened wide the door of opportunity for adventures and scamps," for "soldiers of fortune" (p. 54). By his "ineptness" and provocation he exacerbated race conflict, and he and his followers were to blame for the ultimate failure of the Mississippi experiment in biracial democracy. So Harris charges, while sparing Barksdale, McNeily, J. Z. George, L. Q. C. Lamar, and the rest of the reactionaries. He does not question their motivation, nor does he apply any pejoratives to them.

Throughout, Harris minimizes the role of violence in Mississippi politics. He says that Mississippians had always been violence-prone and that most of the mayhem and murder during the reconstruction period was nonpolitical. Here, again, he follows the conservative line of that time. In regard to the Ku Klux Klan, he disagrees with Garner, Vernon L. Wharton, and Allen W. Trelease, who have maintained that one of the Klan's basic aims was the destruction of schools for blacks. Harris admits (p. 328) that there were "attacks on obnoxious teachers" but contends that the Klan directed its hostility against the teachers' "political and social activities" more than "their work in establishing free schools," as if the two kinds of activity could, in the circumstances, have been separated. He also explains the rise of Ku Kluxism, if he does not justify it, on the grounds that anti-Republican counties were "saddled with a slate of obnoxious appointed officials" who were "in disrepute" and hence incapable of suppressing crime (p. 371).

Finally, playing down both the role of violence and the issue of white supremacy, Harris emphasizes the "failure" of Republican leadership and the economic grievances of white landowners in the 1875 overthrow of the Republicans. This he sees as essentially the culmination of a "taxpayers' movement." Here he accepts what elsewhere (p. 375) he himself characterizes as "extreme anti-Radical propaganda, which was only partly believed by the conservative leadership." Garner, no advocate of equal rights, was frank to say "a superior race will not submit to the government of an inferior one" (Garner, p. 408). Harris, by contrast, does subscribe to the principle of racial equality—as did the Mississippi conservatives in their 1875 platform.

RICHARD N. CURRENT
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DWIGHT B. BILLINGS, JR. *Planters and the Making of a "New South": Class, Politics, and Development in North Carolina, 1865-1900*. Chapel Hill: University of North Carolina Press. 1979. Pp. xiii, 284.

In an analysis patterned on Barrington Moore's *Social Origins of Dictatorship and Democracy*, sociology professor Dwight B. Billings, Jr., challenges C. Vann Woodward's interpretation of the post-Civil War eclipse of southern planters. Moore projected the model of a "Prussian Road" to modernization forged by industrialists and landed elites allied against lower classes. Presumably southern modernization fits this pattern, preserving and intensifying authority and repression. Unfortunately, this work contains more interpretation than primary research. One manuscript collection is cited followed by thirteen pages of published works, theses, and dissertations. With considerable understatement, the author warns: "My goal . . . is as much to develop hypotheses about the South as to test them."

The work's major contribution is its contention that the planter elite dominated the New South. Mini-biographies illustrate planter persistence in North Carolina's power structure, are based on solid historical research, and should be taken seriously. When the author moves from analyzing data to interpretation, however, he encounters problems. His analysis of Populism and Progressivism are examples. Following other historians of Populism, Billings noted that many Populist leaders were from the landed class and charges that the Populist refusal to engage in "an honest treatment of southern stratification" was "a key factor in their political demise" (p. 190). But Billings himself demonstrates that some of these landed "aristocrats" became radical Populists. Their failure was due less to internal restraints than to external forces: whatever effort Populists made at biracial economic radicalism was fastened upon by conservative Democrats and used to bludgeon them into submission. Billings' notion that "genuine class consciousness in the rural South was an awesome possibility" presumes that more radical Populism would have been successful, a dubious proposition. The author also concludes that Progressive educational reforms actually were "profoundly conservative." But this is a theorist's definition of "progressive" that does inadequate justice to a different age, which believed that schools and compulsory attendance laws would end illiteracy, child labor, and poverty.

The author's use of comparative history also is strained. The race factor makes it difficult to compare modernization in Germany, Japan, and the American South. Japanese cultural patterns, which Ruth Benedict described in *The Chrysanthemum and the Sword* (1946), including confidence in hierarchy

and the industrial paternalism resulting from *giri*, seem quite remote from North Carolina's mill villages and plantations.

Billings's hypotheses also must be applied cautiously to other southern states. Jonathan Wiener's *Social Origins of the New South* preceded Billings's work by a year. Wiener, one of Moore's students, applied the "Prussian Road" thesis to Alabama, the state that Billings uses for many of his comparisons. Wiener, like Billings, found remarkable planter persistence. But where Billings emphasized upper-class consensus after 1865, Wiener discovered fierce conflict between planters on one side and New South merchants and industrialists on the other. Whereas many Carolina planters apparently endorsed industrialism, Alabama planters opposed "the growth of a genuinely industrial society in their midst" (Wiener, p. 162). Although sharing Moore's interpretive assumptions with Billings, Wiener believes that Alabama Populism was "a period of decisive class conflict in Alabama; the Populist uprising was the first organized threat to planter rule since . . . secession" (Wiener, p. 222).

Studies of five Alabama counties (Wiener) or one southern state (Billings) should have yielded more temperate claims; a great deal more basic research needs to be done before one of Moore's devotees treats us to a Dixie-bred Bismarck.

WAYNE FLYNT
Auburn University

PAUL D. ESCOTT. *Slavery Remembered: A Record of Twentieth-Century Slave Narratives*. Chapel Hill: University of North Carolina Press. 1979. Pp. xv, 221. Cloth \$12.50, paper \$7.00.

New black militancy has led to a transformation of the historiography of American slavery. We have had a new literature heavily based on antebellum slave narratives and upon twentieth-century interviews with former slaves. The largest part of this material is the former slave narratives collected by the Federal Writers' Project of the nineteenth-thirties.

Paul D. Escott has submitted an important part of the Federal Writers' Project materials, as well as two volumes produced at Fisk University, to an exhaustive statistical survey and has written a straightforward work that affirms much of the recent work on American slavery based on the slave narratives. Among Escott's conclusions about slavery based on his examination of the narratives are; an overwhelming majority of former slaves saw their masters as "pursuing a selfish advantage" (p. 24); a great majority reported a personal experience with whippings or beatings; hard work was nearly a universal lament; food, clothing, and shelter were

simple but adequate in quantity; terrible physical abuse often did occur; roughly one-fifth reported at least partial breakup of their families under slavery due to sales.

Escott argues that under slavery there was an autonomous and viable slave community and culture, although slavery was a harsh and heavily exploitative social system. Moreover, he devotes considerable space to a partial refutation of Eugene D. Genovese's view that the slave narratives indicate the masters and slaves lived in the same world on the plantation, a world over which the masters had "hegemony" and exercised paternalistic control. Escott says that "the narratives make clear that masters and slaves lived in different worlds . . ." (p. 20) and that "paternalism related more to talk about the plantation than to what actually went on there" (p. 20). One might wish that Escott had been more explicit about the use of the narratives by other authors.

One weakness in the book is that a list of the interviewers of the former slaves for the Federal Writers' Project by state and race based upon his evaluations after reading the narratives is inaccurate. An examination of government personnel records would have provided a more accurate list. Some of those listed as white were black; some of those listed as black were white; for almost all of the many listed as "race unidentified" the personnel records supply the data.

Nevertheless, this is a useful, intelligent, readable book, which presents the findings of a most important statistical survey and clears the way for further, more detailed work utilizing the slave narratives.

GEORGE P. RAWICK
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St. Louis

MARTIN KAUFMAN. *The University of Vermont College of Medicine*. Hanover, N.H.: University of Vermont College of Medicine; distributed by University Press of New England. 1979. Pp. xviii, 275. \$15.00.

This is a detailed account of the "country" medical school that was founded in Burlington in 1804 and that, although it was dormant from 1836 to 1854, unlike most such schools, has managed to continue in existence. It became affiliated with the University of Vermont in 1821. The book is based on careful research in the archives of the college and tells the story as it was, with virtually no embellishment.

The value of the book lies in its being a case history of medical education in the United States. It describes the personal egotisms, the provincialism, the financial motivations and manipulations, the institutional competition (not always gentlemanly),

the inadequacies of the didactic lecture and private preceptor system of medical education, and the rural-urban school debate of the early nineteenth century. None of this was particularly peculiar to Vermont.

The nineteenth century saw constant attempts to reform medical education. At Vermont many of these attempts emanated from the college itself, but most came from pressures from the Carnegie Foundation, the American Medical Association, and the Association of American Medical Colleges. Often on the fringe of loss of accreditation, the college continued in existence. The ability of the college to withstand these pressures—including that from the Flexner report early in the present century—was nothing short of phenomenal. Recommendations to go out of existence or to limit the college to a two-year program were steadfastly resisted by the college. Yet there is ample evidence of the important influence of these national agencies in shaping American medical education.

The problems of the college regarding full-time versus part-time faculty, clinical resources, and relationships with the local hospitals were—and are—not unique to Vermont. The kind of town-gown difficulties to which medical schools without their own clinical facilities were prone pervaded the history of the college from the late nineteenth century on. Other problems of medical education—the budgetary competition of medical colleges with other colleges of the university, the relationships between preclinical and clinical faculties, the premedical requirements, the length of the medical program, the constant improvement of the curriculum, and the impact of federal support for medical education—are also presented in this account. The tenacity of the college and its ability to support medical education in a rural state-supported situation gives it importance in the history of medical education. It is interesting, too, that the state of Vermont boasts one of the highest ratios of physicians to population.

The book presents little analytical use of the data but touches upon some of the broader aspects of the history of medical education (the impact of the biomedical sciences and the responsibility of the medical education establishment in meeting the social needs for health care, for example). Yet it is spared from becoming an exercise in parochial chronology by the author's familiarity—he has also written *Homeopathy in America* and *American Medical Education: The Formative Years, 1765-1910*—with the broader aspects of the history of the nation's medical institutions.

An unusual feature of the book is that it has been composed with extrawide margins and that the notes have been placed in the margins. This resort to an ancient usage proved facile and pleasing and

superior to notes at the foot of the page, and, most certainly, to notes at the end of a book.

DAVID L. COWEN
Rutgers University

ELIZABETH ALDEN GREEN. *Mary Lyon and Mount Holyoke: Opening the Gates*. Hanover, N.H.: University Press of New England. 1979. Pp. xvii, 406. \$17.50.

In 1837 Mary Lyon founded in South Hadley, Massachusetts, the Mount Holyoke Female Seminary, which was destined to become the first permanent institution in our country for the higher education of women. In this scholarly volume Elizabeth Alden Green gives us both a biography of Lyon and the history of the first twelve years of the seminary she established. As emeritus professor of English and for twenty-five years director of the news bureau at Mount Holyoke, the author brings a rich background to her writing.

Mary Lyon was born in 1797 to a farm family in Buckland in western Massachusetts. What distinguished her from the other young women in the community were her eagerness and determination to secure an education. In 1814, recommended by one of her teachers for a teaching post, Mary began a life-long career in education. Compensation in her first position, "a summer term" of twenty weeks, was seventy-five cents a week and board among the patrons. As a teacher Mary improved her own education by studying at available institutions, notably coeducational Amherst Academy and Byfield Seminary for young women. At Amherst in 1818 she formed friendships that would give her the closest associations at Amherst College after it was chartered in 1825. At Byfield she was inspired by the teaching of the Reverend Joseph Emerson, a Congregational minister, who to the thoroughness of his teaching brought a high opinion of the intellectual powers of women. As Mary wrote, he "treated ladies and gentlemen essentially in the same manner." At Byfield Mary met Zilpah Grant, another pioneer teacher, with whom she would be associated through much of the decade 1824-34.

As she gained experience, Mary Lyon desired to establish a permanent, properly financed institution for the higher education of women. In Joseph Emerson's words, she looked to a time when "female institutions, very greatly superior to the present, will not only exist but be considered as important, as are now our colleges for the education of our sons." In 1834, Mary gave up her teaching and for the next three years devoted her entire time to raising money and laying plans for the new institution—writing all the appeals, raising the funds, and assembling the committee that became the board of trustees. South Hadley, Massachusetts, which

pledged \$8,000, was chosen as the location, the institution was named the Mount Holyoke Female Seminary, a five-story building was erected, eighty young ladies were admitted, and Miss Lyon and three young women began instruction on November 8, 1837.

The academic program covered three years and the curriculum was based on that of Amherst College. There was no preparatory department and all incoming students were required to take entrance examinations. The work of the regular teachers was supplemented by lecturers from Amherst and Williams. To lower costs and bring the seminary within the reach of families of moderate means, each student was assigned a specific household task. Education at Mount Holyoke was seen as a preparation for service among all classes and in foreign lands. A woman of deep Christian faith, Lyon was an ardent supporter of the American Board of Commissioners for Foreign Missions. She was to have only twelve years at Mount Holyoke, but when she died in 1849 she had made a permanent contribution to higher education.

This attractive, well-illustrated volume reintroduces Lyon to our own time. The impressive bibliography, extensive notes, and excellent index will be helpful to other researchers in the field.

E. WILSON LYON
Pomona College

JONATHAN LURIE. *The Chicago Board of Trade, 1859-1905: The Dynamics of Self-Regulation*. Urbana: University of Illinois Press. 1979. Pp. xiv, 234. \$12.50.

The Chicago Board of Trade, founded in 1848 as an informal club for local merchants, acquired a formal charter from the Illinois legislature in 1859. The charter invited the board to administer its own internal affairs as a commodities exchange, to appoint grain inspectors and weighers, and to regard its arbitration awards in members' disputes as the equivalent of Circuit Court judgments.

Jonathan Lurie draws upon a close reading of board records and papers, contemporary newspapers and periodicals, and a lengthy list of court cases to describe how the board subsequently developed and administered its delegated legal authority. The result is a book that illuminates the politics of economic self-regulation in the late nineteenth century and contributes usefully to the historical literature on the interaction of law, politics, and economic interest groups in the generation of American public policy.

In the two decades after its charter grant, the board hammered out its basic trading practices and forged an administrative framework. In aiming to provide an efficient and predictable market mecha-

nism for its members, who numbered over 1,600 in 1873, the board's directorate had to balance the need for collective discipline against its own limited leverage and the epoch's enormous speculative enthusiasms. As might be expected, internal self-discipline, "slow and often contradictory," lagged well behind speculative compulsions (p. 36). Rules against privileged trading and market corners were passed but not enforced.

Between 1874 and 1894 pressure for self-discipline mounted. Agrarian discontent stalked the land, and Lurie describes the congressional battle that agrarians waged for regulated futures trading. The agrarians lost, but the board confronted an even greater threat in economic competition from the so-called bucket shops. These small trading operations catered to commodity gamblers who objected to the board's \$10,000 initiation fee and rejected its rules on trade sizes, delivery, commission rates, and so on.

The board's struggle to close down the bucket shops was a slow, intermittent affair. Many of its own members retained bucket-shop connections, and the operations of the two institutions were nearly indistinguishable. In the long run, however, the board prevailed, and Lurie examines the court cases that marked the road to victory. The board eventually gained the right to deny all its market quotations to bucket-shop traders and to have all its own members barred from bucket-shop connections. By the turn of the century the board's functions had clearly won full legitimacy as quasi-public responsibilities.

The historical experience, moreover, held important implications for federal intervention when it did come. Lurie does not go into detail on the politics of the Grain Futures Act of 1921, the basis for all subsequent federal commodities legislation, except to note that it essentially wrote private practices into federal law. But in suggesting how the preceding history of self-regulation in a private economic institution such as the Chicago Board of Trade fundamentally shaped subsequent efforts at public regulation, Lurie has identified a key pattern in the evolution of American public policy.

ROBERT D. CUFF
York University

BRUCE BRINGHURST. *Antitrust and the Oil Monopoly: The Standard Oil Cases, 1890-1911*. (Contributions in Legal Studies, number 8.) Westport, Conn.: Greenwood Press. 1979. Pp. x, 296. \$22.95.

This book indicts American antitrust policy and its implementation on the basis of precedent-setting cases against Standard Oil, the archetype of American big business. The author concludes, "Antitrust

has never worked, and it has been used repeatedly to obscure the inability of the federal government to protect the public from the excesses of big business" (p. 222). In arriving at this verdict, Bruce Bringhurst examines the motives and impact of antitrust suits against Standard Oil at state and federal levels, culminating in the 1911 United States Supreme Court decision that ordered dissolution of the combination.

Four chapters are devoted to essentially futile efforts to invoke antitrust laws at the state level against Standard Oil. An early victory against Ohio Standard was neatly negated by reorganization of the Standard Oil Trust. When Ohio Attorney General Frank Monnett brought suit, he encountered resistance in obtaining company records, was subjected to adverse publicity at the hands of a Standard-subsidized press, and was politically pressured by U.S. Senator Joseph Foraker, who was in Standard's pocket. Monnett hurt his own credibility by alleging bribery and record-burning by Standard Oil. The case dragged on after he left office, and his successors showed little interest in taking aggressive action against the trust. One even claimed that for three years he was unable to serve a subpoena on a Standard company in Ohio!

The story was similar in Texas, Missouri, and Kansas. In Texas, for example, the appearance of successful action to oust a Standard subsidiary received widespread publicity, but the state continued to have Standard companies under new names. Kansas and Missouri wanted the investment and jobs that went with industrialization, and Standard Oil provided both. This fact, combined with the economic conservatism of judges, effective lobbying, and judicious use of political influence and wealth by Standard, created public relations triumphs and political rewards for the antitrust prosecutors but did little to change the competitive situation. When public concern reached national levels, the states seized on the claim that the initiative was being taken by the federal government as an excuse for inaction on their part.

Bringhurst shows that President Theodore Roosevelt used antitrust much as politicians had at the state level. His sense of timing, for example, led him to ignore citizen complaints against the oil combination but then to stir up and use public hostility toward Standard Oil to gain desired legislation. His efforts to amend the antitrust law to recognize "reasonable restraints" of trade failed, but that object was achieved in the judicial "rule of reason," advanced by Chief Justice White in the 1911 Standard Oil decision. One result was creation of a favorable legal climate for the merger movement of the 1920s.

The author makes his case, though it is somewhat weakened by his reluctance to concede that there

may be some virtues in bigness and that antitrust policy may have had some beneficial long-run effects on the structure and behavior of the oil industry. His attempt to cover developments down to the recent past in a single chapter is not in keeping with the in-depth treatment of the 1890-1911 period. Nevertheless, this book documents convincingly the key role of antitrust in soothing the American public about the maintenance of competition among small business units while the economy was being transformed by the rise of large-scale enterprise, with Standard Oil among its leaders.

ARTHUR M. JOHNSON
University of Maine,
Orono

MARK WYMAN. *Hard Rock Epic: Western Miners and the Industrial Revolution, 1860-1910*. Berkeley and Los Angeles: University of California Press. 1979. Pp. x, 331. \$15.95.

In recent years, a number of first-rate studies concerning the metal or hardrock miners of the American West have appeared. Among the best of these works are Richard E. Lingenfelter's *Hardrock Miners* (1974) and John Rowe's *Hardrock Men* (1974), each of which focused on particular aspects of the miners' life and work. Mark Wyman's book is a similar study, one which, although covering much familiar ground, nevertheless offers significant insights into the miners' responses to the problems resulting from the introduction of an ever-changing technology into a frontier environment.

Wyman demonstrates that the new technology was a mixed blessing. Technological innovations that were expected to make the miners more productive yet alleviate some of the hardships of the workplace often generated a host of new and complex problems for the miners—toxic gases, silicosis, electrocution, delayed and premature explosions, and so on. Industrializing the mines was achieved at great human cost as the miners learned to use machines, dynamite, and electricity deep in the earth. The new technology simultaneously revolutionized the organization of the work force, drastically altered the methods of financing mining operations, frequently imposed an absentee ownership, tied Western mining to the national and world economy, and gradually altered the institutional framework within which it was applied. These changes directly and often adversely affected the miners.

According to Wyman, the hardrock miners reacted to the new conditions in a variety of ways; for example, leaving the "country," seizing the mines, lobbying the state legislatures, resorting to violence, or forming unions. Some of these responses were obviously more significant than others. In analyzing

the responses, the author has modified some long-held views concerning the hardrock miners. For example, he rejects older interpretations of the causes of Western working-class radicalism and, instead, concludes that its origins "stemmed most immediately from a mounting sense of desperation" produced among hardrock miners by the new industrial climate (p. 227). He also concludes that the union movement among the miners was a "conservative, protective movement" (p. 173), primarily designed to maintain the high wages that had characterized Western metal mining from its beginning. Only occasionally did other issues such as union recognition assume equal importance, although Wyman concedes that the eight-hour day issue often ranked with wages as a cause of labor-management disputes. Wyman de-emphasizes the violence traditionally associated with unions of hardrock miners (for example, the Western Federation of Miners) and stresses the more typical peaceful resolution of labor-management problems in Western metal mining.

Generally, *Hard Rock Epic* is both a descriptive and analytical study of the impact of technology on the life of metalliferous miners of the West. It is thoroughly researched, drawing heavily upon primary sources and the most relevant recent scholarship concerning the hardrock men. The study is judicious and balanced with a somewhat revisionist slant on such issues as the origin and extent of the miners' radicalism. It fits well into the growing body of scholarship on Western metal mining. Historians of labor and the American West will find this volume instructive and a definite contribution to their fields of study.

GEORGE G. SUGGS, JR.
Southeast Missouri State University

JON C. TEAFORD. *City and Suburb: The Political Fragmentation of Metropolitan America, 1850-1970*. (Johns Hopkins Studies in Urban Affairs.) Baltimore: Johns Hopkins University Press. 1979. Pp. vii, 231. \$15.00.

The American suburban experience, although nearly as old as the country itself, has largely remained the preserve of political scientists and sociologists. A few historians have dared to venture forth, but the literature has been sparse. Jon C. Teaford's fine monograph thus fills one of the many gaps.

As his subtitle suggests, Teaford is interested in the position of suburban government in metropolitan America. The book is about political arrangements; it does not concern itself deeply with the great economic and social changes that transformed the way and where Americans lived. The existence

of large and growing populations on the periphery of major cities is taken for granted; what fascinates Teaford is the legal and political status of the thousands of communities these people established.

To understand why autonomous units of government proliferated after 1850, one must look first at the American commitment to local self-determination. Convinced that local affairs should be handled by local authorities, state legislatures enacted permissive incorporation laws. Unlike the approach in other countries (Teaford has a useful chapter describing the British approach) the responsibility for local government organization was left almost entirely to the local populace. So empowered, groups used the government to secure their own particularistic ends (for example, low taxes, prohibition, maintenance of social distinctions).

But by about 1880 fragmentation was being rivaled by consolidation. Benefiting from economies of scale, large cities were able to provide a wider range of services than their suburbs. Faced with a choice between autonomy and material comfort, many suburbs opted for the latter and joined the central city. Like the earlier incorporations, annexations occurred without regard to metropolitan administration; again, local self-determination reigned supreme.

After 1910 the consolidation movement all but disappeared. Improved technology and a stronger bond market allowed the larger, older suburbs to provide more services than in the past. Able to offer such amenities as transit, water, and sewerage and with particularism gaining strength as a consequence of Progressivism, the suburbs' incentives to merge with the central city evaporated.

Teaford's analysis of metropolitan fragmentation, consolidation, and suburban ascendancy through 1940 amplifies the findings of others. His research and case studies illuminate the many variations yet basic similarities in the experiences of American communities over the century. The second half of the book details the futile efforts in the 1920s and 1930s to reconcile the American traditions of dispersed power and centralized control through a federal form of metropolitan rule. The author clearly favors the idea, but he gives a dispassionate explanation of its failure. To the discomfort of those who claim the existence of a "power elite," Teaford demonstrates that the struggle for a federative metropolis pitted the business, academic, media-, and political-establishment against the farmers, factory workers, and the small-town politicians, and the latter prevailed. Pluralism was still a force.

A few caveats should be noted. Teaford accepts at face value the rationality and efficiency arguments of those favoring a federative metropolis; but power would thereby be redistributed and it should be made clearer who the winners and losers would

be. Although documented to 1970, the book covers the period since 1940 only sketchily. There is, for example, no discussion of the federal government's attempts at metropolitan reorganization, which, although not notably successful, does strike at local autonomy. Such openings to further research, however, characterize path-setting books, and this is one.

MARK I. GELFAND
Boston College

T. LINDSAY BAKER. *The First Polish Americans: Silesian Settlements in Texas*. College Station: Texas A&M University Press. 1979. Pp. xiv, 268. \$14.95.

Awareness of ethnic diversity in Texas increases constantly, thanks to efforts by such scholars as T. Lindsay Baker and the continuing work of the Texan Institute of Culture in San Antonio. Texans knew all along they counted Hill Country Germans and Southwestern Mexican-Americans in the midst of an Anglo-Celtic majority, and since the 1940s they also made room for the modern urban Yankee as a sub-ethnic species. Baker and other scholars now make them aware of a considerable contribution to Texan and United States economic and social growth in the Southwest.

Baker's group, the Poles, claim the first Polish colony in the United States as well as the first Polish Catholic Church and the first Polish schools among their contributions. These were planted in the hill country some sixty miles from San Antonio at Panna Maria (Virgin Mary) and Bandera. The migration came from Upper Silesia, then the southeastern end of Prussia, in the mid-1850s. It started because a Polish youth, Leopold Moczygamba, entered the priesthood and developed a missionary zeal that found opportunity in personnel shortages in Bishop Jean-Marie Odin's area in Texas. Odin recruited Father Bonaventure Keller, Keller recommended Moczygamba, and together they came to Texas in 1852.

Moczygamba's letters to his family, also widely read by relatives and neighbors, described abundant and inexpensive land and weather that remained pleasant and mild. They do not seem exaggerated or even read like recruiting letters—at least in the beginning. But the letters arrived among Poles dominated by German political authority, where poverty from high food prices, natural disasters, blights, droughts, epidemics of typhus and cholera, and increasing criminal activities made life unpleasant and dangerous. In 1854 major rains collapsed levees and dams, thereby flooding out two-thirds of the Silesian crops. Moczygamba's letters mentioned none of these problems and promised, perhaps by implication, much happiness. So they came.

In 1854, one hundred and fifty Poles, quickly termed "Polanders" by the Texans, crossed the ocean to establish their kind's first colony in the United States (individual Poles had of course migrated before, especially after the insurrection in 1830). The next year seven hundred friends and neighbors joined them, and five hundred more came in 1856. Then their numbers lessened as a result of things getting worse in America and better in Poland.

The immigrants represented the peasant class exclusively. They owned land in Poland and intended to own land in Texas; they paid taxes, worked hard, and eventually prospered. Their journey began with the liquidation of real and personal property, continued with a train ride to a seaport such as Bremen, dragged on with a monotonous, two-month ship passage to Galveston or Indianola, and concluded with an inland ride on a Mexican-driven donkey cart to Panna Maria or Bandera or one of the later-established settlements. Then came reality. No prepared site, no shelter, rattlesnakes, hostile Indians, occasionally inhospitable Anglos, and grasshoppers, among other hardships, wanted immediate and often continuing attention. Continental agents such as Julius Henrick Schüller helped them make the passage, and Father Moczygmba and earlier arrivals welcomed them, but hard times usually marked their migration. In fact, within a few years hard feelings against Moczygmba over their problems caused him to move to Detroit, Michigan.

Father Anthony Rossadowski and Father Julian Przysiecki carried on the Polish Catholic ministries until they also left. Following the American Civil War the Congregation of the Resurrection resumed Catholic service to the Poles. Few Poles served under Texas arms in the Confederacy, and almost none did unless conscripted. If captured, they accepted service in the Union army. Such "disloyalty" earned the Texas Poles more ill-will from the Anglos and resulted in their first real ethnic persecution in the troubled years of Reconstruction. But their problems brought forth solutions: they fought back for the first time entered politics and became more a part of the mainstream, while maintaining as much ethnic solidarity as possible at home.

They remain, as Baker observes, an island of Poland in a sea of Anglo-German-Mexican Texans. Baker handles his subject well. He writes pleasantly, and his documentation testifies to his familiarity with Polish and the Poles. His regard for that nation's people and their American descendants is as touching as it is obvious. I must wonder, though, how he got the Polish off-shoot colony of Franklin east of St. Louis on the Missouri River, why he frequently used the term southerner in reference to persecutors, or why he did not relate the Texas "Po-

landers" to the great migrations that peopled northern areas. But as a book on the Texas Poles, it is excellent.

ARCHIE P. McDONALD

Stephen F. Austin State University

PATRICK F. PALERMO. *Lincoln Steffens*. (Twayne's United States Authors Series, number 320). Boston: Twayne. 1978. Pp. 148. \$9.95.

Twice during his lifetime, Lincoln Steffens was a cult figure, once as a muckraker during the 1900s and again as an autobiographical pilgrim on the left in the 1930s. Gone for more than forty years, he still continues to fascinate. Patrick F. Palermo offers "a critical-analytical study . . . not a biography" (p. 9). Based on published writings and letters and informed by the work of other historians, Palermo provides a searching account of Steffens's intellectual pilgrimage from liberal reform to Russian revolution. Like other critics, Palermo properly approaches *The Autobiography of Lincoln Steffens* (1931) with caution, but he grasps its central reality as "the tale of a man measuring himself as a moral agent in the world" (p. 11).

Steffens was an unpromising fiction writer and a not much better political philosopher. By all accounts, including his own, he was a great journalist. Bosses, businessmen, presidents, cops, and criminals opened up to him. He was a superb interviewer and storyteller, working with the Christian metaphor and plot through which the middle classes comprehended the American scene. With investigative journalism's combination of documentation and indignation, he offered both detective story and titanic battle between boodlers and reformers. The purpose was to shame the citizenry into democratic moral action. The villain, however, was not the boodler, boss, or even big businessman, but rather privilege, the edenic temptation-apple; and the heroes were the strong men, often reformer-bosses, who fought for the people. Progressivism, Palermo explains, was spiritual, not economic, aiming at the heart, not the system; Christ, not Marx. But it failed. The decline of muckraking and progressivism, the failure of his "Boston Plan" and of his attempts to mediate both the trial of the Los Angeles Times dynamiters and the Russian Revolution, plus the Versailles treaty, "broke Steffens' liberal faith" (p. 108). Still seeking but unable to still believe in progressivism, Steffens adopted the radicalism of the Russian Revolution, Lenin rather than Theodore Roosevelt, Tom Johnson, or Robert La Follette, as the leader, and historical determinism rather than reform and Christian transformation.

Palermo does not hesitate to theorize. Perhaps his most clarifying perception is that Steffens sought to

have it both ways. In seeing himself as a "broken liberal" with democracy rejected, compassion gone, and willing to accept the "necessary" harshness of historical process, Steffens particularly seemed to enjoy attacking his former liberal allies. Yet, he never formally entered the Communist Party. Perhaps his life-long liberalism, which "disqualified him from judging the revolution" (p. 112), also kept him from joining it. Like Moses, he was not to cross over into the promised land. Justin Kaplan's easily flowing biography (*Lincoln Steffens: A Biography* [1974]) is illuminating about Steffens's life, and Patrick Palermo's briefer account offers excellent guidance for his politics. Even so, Lincoln Steffens remains more interesting than all of us who write about him; that is why we still do.

DAVID CHALMERS
University of Florida

LOUIS FILLER. *Progressivism and Muckraking*. (Bibliographic Guides for Contemporary Collections.) New York: R. R. Bowker. 1976. Pp. xiv, 200.

Louis Filler has written a thoughtful and readable interpretative bibliography of Progressivism and muckraking, following his theme through the nineteenth and twentieth centuries and basing his essay on intensive and extensive reading in literary and historical materials. He evaluates the pertinent literature that presaged the Progressive impulse and muckraking, thoroughly examines the important works of and about the Progressive era, traces its continuing importance through the succeeding decades, and finally touches upon its meaning for the present. If current writing in this vein were to reach a large enough audience to be effective, he suggests, it would have to drop its clichés about businessmen and fulfill the emotional and literary needs of the public.

Two aspects of this slender volume limit its effectiveness, at least for this reviewer. While Filler recognizes that the Progressives' "major cause appeared to be monopoly, with its challenge to American traditions of freedom" (p. 72), the sentence only appears three-fifths of the way through the book, and the reaction to trusts is casually mentioned earlier in a discussion about labor and capital. Since the economic and political threat of private economic concentration was central to Progressive thought, the de-emphasis of this theme unbalances the volume. Without this focus we cannot see the change in Progressivism by the sixties that enabled the Kennedy-Johnson administrations—little concerned about oligopolies—to give us Telstar and social reform.

The second reservation is his treatment of socialism as "a radical wing to progressivism" (p. 59), a

common, but mistaken premise. While the spread of both movements derived from a growing concern with the overweening power of big business, and socialists were willing to support Progressive palliatives while Progressives borrowed from the immediate program of the socialists, they differed fundamentally on the ownership and operation of the means of production. While Progressives were aware of the importance of economic power in society, unlike the socialists they felt that corporate power could be restrained while ownership and decision making remained in private hands, a faith not borne out by history. As a result of his definition Filler illustrates a point about Progressivism exclusively with two socialist authors, and this could be misleading.

Nevertheless, the author has penned a fine work and he is entitled to his own structure and definitions.

FRED GREENBAUM
Queensborough Community College,
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ARTHUR S. LINK. *Woodrow Wilson: Revolution, War, and Peace*. Arlington Heights, Ill.: AHM Publishing. 1979. Pp. viii, 138. Cloth \$9.95, paper \$5.95.

In 1957 Arthur Link distilled his views on Woodrow Wilson as a world leader in a slim volume, *Wilson the Diplomatist*. For diplomatic historians the contribution was of immense value. Now Link has embodied his changing convictions in a markedly different book, *Woodrow Wilson: Revolution, War, and Peace*. In his earlier volume the author recognized weaknesses in Wilson's effort to avoid or ignore the realities that frustrated his principles. In his more recent effort he has eliminated such criticism and has produced, in large measure, the ultimate defense of Wilson's leadership. The list of achievements that conclude the first essay is long and impressive. While acknowledging that Wilson's acceptance of British maritime practice served the American interest in a British victory, the author, in his second and third essays, has introduced new and valuable material, especially a long note that Wilson dictated in the autumn of 1916, to substantiate Wilson's concern for a peace without victory. In accepting war in April 1917, Wilson hoped to terminate the war quickly and to establish his leadership in the building of a new postwar order. That Wilson failed to achieve an ideal peace at Versailles the author attributes less to Wilson's failures than to the special barriers he faced in the demands and personalities of Clemenceau, Lloyd George, and Orlando. Link does not attribute Wilson's loss of power to such factors as his refusal to negotiate with the Allies while they still required American aid or his deci-

sions to introduce the peace issue into the 1918 congressional elections, to lead the American delegation at Paris in person, and to ignore Republican Senators in his selection of the peace commission.

In this new volume the author upholds Wilson's decision to protect his treaty against reservations, for League membership on Wilson's terms alone, he agrees, would have assured a responsible exercise of American power. Wilson advocated a strong system of collective security, but he never answered the conflicting questions of interest, obligation, sovereignty, and power embodied in the problem of collective security. Still, the author offers no criticism of Wilson's vision of a new order of peace based on the League of Nations. He lauds the Paris settlement for establishing "the foundation of what could have been a viable and secure world order, if only the victors had maintained the will to build upon it" (p. 103). In the 1957 volume the author noted that Wilson's hopes for collective security ultimately took the form of regional alliances such as NATO. In the new volume he views the UN as the vindication of Wilson's vision. He concludes the book with a condemnation of those who ignored Wilson's plea for collective security and thus permitted another war to come. But did the failure of the democracies lay in not building up the League, as the author suggests, or in the refusal of the victors to maintain adequate national policies? Successful collective security in the 1930s would have reflected mutual interests and not membership in the League of Nations. League membership offered no substitute for effective national action; yet the specific interests of the United States worth pursuing and the power they required did not seem to concern Wilson at all.

NORMAN A. GRAEBNER
University of Virginia

DAVID BURNER. *Herbert Hoover: A Public Life*. New York: Alfred A. Knopf. 1979. Pp. xii, 433. \$15.95.

Although over a decade of research and preparation went into this biography, it still leaves one with the impression that David Burner would have rather written a longer study about his subject and taken more time to do it. Whatever the reason, there is an unfinished quality to both the content and conceptual analysis that gives this book a sense of being hurried or incomplete. Indeed, Burner casts little light on the last third of Hoover's life between 1933 and 1964, although he does fill major gaps in our knowledge about the first third of it.

The opening chapters dealing with Hoover's formative years in Iowa, Oregon, and California, and his career as an international engineer are rich in detail. Hoover's far-flung activities before 1914 have never been so thoroughly documented. A later

chapter on his subsequent reform activities as president during the eight months prior to the Great Depression is probably the most original section. It clearly indicates that Hoover was well on his way to becoming the most progressive and successful Republican leader since Theodore Roosevelt. We learn about the "important and least-known actions" (p. 213) of the thirty-first president, including his stands on Indian rights, prison reform, land and energy conservation, the building of inland waterways, federal judiciary appointments, agricultural policy, and even his attempts at enhancing the civil liberties of blacks, women, and Jews.

One difficulty with this biography is that, aside from the first eight months of Hoover's ill-fated administration, Burner fails to offer particularly new or insightful material about the middle third of his career—the period between 1920 and 1933 when Hoover was secretary of commerce and a depression president. A more comprehensive conceptualization of his actions during these years can be found in Ellis Hawley's *The Great War and the Search for a Modern Order: A History of the American People and Their Institutions, 1917–1933* (1979). Moreover, the single chapter on foreign policy, along with a few isolated comments in other segments of the work, simply do not do justice to Hoover's systemic approach to foreign and domestic policy, especially during the years he headed the commerce department under Harding and Coolidge. This neglect of Hoover's influence on American diplomacy in the 1920s is particularly noticeable because Burner amply details the impact Hoover had on world affairs during the First World War. Crucial transitions in his thought on foreign and domestic issues between 1918 and 1921 such as the ideology of voluntarism, food diplomacy, the Russian Revolution, and international economics are given more attention in the collection of essays edited by Lawrence E. Gelfand, *Herbert Hoover: The Great War and Its Aftermath, 1914–1923* (1979).

Another analytical weakness of the biography arises when Burner compares Hoover and his brand of progressivism with the thought and actions of three other historical figures—Thorstein Veblen, Theodore Roosevelt, and Woodrow Wilson. He characterizes Hoover's foreign policy as in the tradition of Wilsonian internationalism (pp. 147, 184, 185, 313, 334, 336); his domestic policies as often like those of "his hero" Theodore Roosevelt (pp. 70, 150, 160, 178, 215, 297); and his engineering management style as most like that of Thorstein Veblen (pp. 63, 64, 67, 73, 75, 157, 210). Unfortunately, Burner does not discuss how Hoover managed to overcome the contradictions implicit in any blend of progressivism represented by these three men. It is also not made clear how their views could be squared with Hoover's seemingly contradictory

commitment to voluntarism, individualism, internationalism, nationalism, and "near" pacifism. Likewise, Burner does not elaborate upon a brief, but suggestive, comparison he finally makes between Hoover in 1933 and John Quincy Adams in 1829. In short, Hoover hovers unevenly over the landscape of his life in this biography, never completely coming into focus despite a few enticing glimpses.

One cannot help but conclude that Burner preferred to describe Hoover's early life and times. Otherwise, how do we deal with his explanation in the preface that the book does not detail his activities after 1933 primarily because of the "inaccessibility of hundreds of documents on the 1930s and after in the Herbert Hoover Presidential Library" (p. xii). In fact, the post-presidential papers represent one of the largest and least restricted of the major archival groups in the West Branch collection.

So we are left with an incomplete biography. While it does not contain the last word on Hoover, it does contribute significantly to Hooverian scholarship for the period before 1920. Arthur Schlesinger, jr.'s protestations notwithstanding (*The New York Review*, March 8, 1979), Burner documents beyond any reasonable doubt the generally progressive nature of Hoover's philosophy and actions.

JOAN HOFF WILSON
Arizona State University

RALPH D. GRAY. *Alloys and Automobiles: The Life of Elwood Haynes*. Indianapolis: Indiana Historical Society. 1979. Pp. xi, 243. \$9.00.

Falsely praised as the inventor of America's first successful automobile by many during his lifetime, Elwood Haynes (1857-1925) has been given only perfunctory mention by automotive historians. "Today he deserves a wider recognition, based not only upon a realistic assessment of his automotive work but also upon his noteworthy achievements in the field of metallurgy and for the cause of humanity," concludes Ralph D. Gray (p. 230). Handicapped by huge gaps in Haynes's correspondence files and the absence of business records for the companies with which Haynes was most intimately associated, Gray nevertheless succeeds both in establishing Haynes's true importance as an inventor and in providing some useful insights into neglected areas of the history of American technology.

Unique among the first generation of automotive engineer-entrepreneurs, Haynes was a college-educated chemist, primarily interested in metallurgy, who did graduate work at Johns Hopkins and taught school prior to playing a key role in the Indiana natural gas boom of the 1880s and 1890s as

Superintendent of the Indiana Natural Gas and Oil Company. Always more at home in his laboratory than in a business office or on a factory floor, Haynes characteristically turned over to others much of the administration of the various business enterprises with which he became associated through his inventions, and he hired Elmer Apperson to build the gasoline automobile he designed in 1893-94. Haynes-Apperson (1896-1905) became one of the first firms in the United States to manufacture motor vehicles, and Gray does an excellent job of detailing its history and that of its successor, the Haynes Automobile Company (1905-25), particularly in evaluating Haynes's contributions to automotive technology. Even more significant, however, were Haynes's contributions to metallurgy—most notably his 1912 patent on Stellite, a superior chrome alloy tool metal, and his patents on stainless steel. Gray demonstrates that these were significant technological accomplishments. They also led to the formation of both the Haynes Stellite Company (sold to Union Carbide in 1920 and now a division of Cabot Corporation) and the American Stainless Steel Company (1917-ca. 1933). A devout Presbyterian and staunch prohibitionist, Haynes remained active in church work and in the prohibition movement as well as a proponent of educational reform as a member of the Indiana State Board of Education from 1921 until his death in 1925. After World War I, he advocated American membership in the League of Nations and supported numerous anti-war organizations. And he was a leading early advocate of American adoption of the metric system.

Alloys and Automobiles is a well-researched, well-written, and informative book that deserves to be read by anyone interested in automotive history, the history of technology, or Indiana regional history.

JAMES J. FLINK
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MARCIA GRAHAM SYNNOTT. *The Half-Opened Door: Discrimination and Admissions at Harvard, Yale, and Princeton, 1900-1970*. (Contributions in American History, number 80.) Westport, Conn: Greenwood Press. 1979. Pp. xxi, 310.

Shortly after the establishment of national quotas in the immigration restriction law of 1921, President A. Lawrence Lowell urged the reduction of the proportion of Jewish undergraduates at Harvard to about 15 percent. The Harvard Faculty of Arts and Sciences, informed that Jewish enrollment had tripled since 1900 (from 7 to 21 percent), first voted that admission procedures should take into account the proportion of racial and national groups and

then rescinded the action and awaited the report of a committee. While the committee deliberated, attacks on Lowell's idea mounted among Harvard undergraduates and alumni and in the press. A rumor that Harvard planned to limit admission of Irish brought Mayor Curley's denunciation and threats to remove the university's tax exemption. The committee's report rejected any racial or religious discrimination (upholding "traditional policy") and called for firmer bars against academically weak students. This policy was voted by the faculty, and the press praised Harvard for its vindication of democracy.

Lowell now called his admission plan crude, but he had not given up. A new proposal to limit the size of entering classes offered a less direct route to a Jewish quota. An Overseers-Corporation-Faculty committee headed by Henry James successfully recommended a plan to limit the size of each class to one thousand and give the admission committee broad discretionary powers in judging "character and fitness and the promise of the greatest usefulness" (p. 109). In the wake of this decision of 1926, the admission committee required photographs on application blanks, undertook more interviewing, and asked headmasters to identify applicants' religious preferences. A Yale administrator reported after a visit to his Harvard counterparts that they were "going to reduce their 25% Hebrew total to 15% or less by simply rejecting without detailed explanation" (p. 110). Lowell had achieved his aim silently through the university's bureaucracy. The new procedures held until World War II. As late as 1942, a memorandum referred to rejection of applicants because of "the 'quota'" (p. 112).

This episode, known only partially even to those most deeply concerned, is meticulously reconstructed by Marcia Graham Synnott. She treats Yale and Princeton, as well as Harvard, policies directed against blacks, Catholics, and foreign students as well as Jews, and discrimination against enrolled students and in faculty appointments. The book ends with an even-handed summary of the Bakke case, which, though valuable in itself, deflected the author from a final appraisal of the rise and fall of ethnic exclusiveness in the Big Three. Such a conclusion would have helped a book that has its own admission problem. What data should get in and what be excluded? Synnott has erred on the side of inclusiveness, sometimes overproving her points and blurring her interpretation. Breaking new ground, she presumably sought to be prepared for any challenge about accuracy from offended institutional representatives.

This volume parallels Harold S. Wechsler's *The Qualified Student: A History of Selective College Admission in America* (1977), which stresses nineteenth-century origins of admission policies, the case of Columbia,

and the effects of open admission in New York. Both works ably recapture past attitudes and procedures, matters that could easily drift away in the mists of institutional self-promotion and selective memory.

HUGH HAWKINS
Amherst College

RANDALL K. BURKETT. *Garveyism as a Religious Movement: The Institutionalization of a Black Civil Religion*. (ATLA Monograph Series, number 13.) Metuchen, N.J.: Scarecrow Press, for American Theological Library Association. 1978. Pp. xxvi, 216. \$11.00.

The civil rights revolution and black power movements of the 1960s spawned an outpouring of popular and scholarly works about the black experience in the United States. Not surprisingly, many of these have dealt with the previously neglected subject of Marcus Garvey and his Universal Negro Improvement Association. The Garvey Movement, which flourished in the early 1920s and then gradually declined until Garvey's untimely death in 1940, was the first genuine mass movement among American Negroes. Garvey's charismatic message of heightened black consciousness, race pride, self-help, and African redemption also won for him a considerable following in Africa and among the scattered black diaspora elsewhere in the New World. Most writers on Garveyism have commented on its religious aspects, noting especially the establishment of a new black denomination, the African Orthodox Church, by a prominent Garveyite, Archbishop George Alexander McGuire. At the same time, the conventional view has held that with a few notable exceptions like McGuire, Garvey was opposed by the vast majority of the black clergy, which saw the movement as a threat to its status and influence.

Now, in this important contribution to the scholarly literature on the Garvey movement, Randall K. Burkett has convincingly demonstrated that earlier writers, including this reviewer, were wrong in their treatment of Garvey's relations with the black clergy and in many of their assumptions about the religious elements of Garveyism. The Garvey movement in its heyday in fact attracted the interest and open support of far more black churchmen—clergy and laity alike—than we have hitherto known. As St. Clair Drake points out in his perceptive introduction to the book, he grew up in close association with black ministers in the 1920s when his clergyman father served for three years as the International Organizer of the U.N.I.A., but it was not until he read Burkett's study that he realized the extent of the involvement of the black clergy with the Garvey movement.

Anyone who has tried to piece together the scattered and fragmentary records of the Garvey movement can appreciate the diligence with which Burkett has reconstructed the involvement of numerous individual black clergymen of various denominations with the Universal Negro Improvement Association. Equally valuable is Burkett's analysis of the religious elements of Garvey's philosophy and his consistent strategy of avoiding conflict with the black clergy by endorsing no official church for the movement—not even McGuire's new African Orthodox Church, which resulted in a temporary falling out between the two men. The U.N.I.A. was an avowedly Christian organization, and its symbols, catechism, and rituals gave it a quasi-religious character. But Garvey shrewdly developed its religious aspects to produce what Burkett describes as a kind of black civil religion, ecumenical in its appeal and broad enough to encompass the believer and non-believer alike. Not everyone will accept Burkett's conclusion that Marcus Garvey was the foremost black theologian of the early twentieth century (Garvey himself would probably have denied it), but there will be general agreement that this carefully researched and insightful study is a major contribution to our understanding of the Garvey phenomenon.

E. DAVID CRONON
University of Wisconsin,
Madison

ABBY ARTHUR JOHNSON and RONALD MABERRY JOHNSON.
Propaganda and Aesthetics: The Literary Politics of Afro-American Magazines in the Twentieth Century. Amherst: University of Massachusetts Press. 1979. Pp. 248. \$15.00.

Afro-American magazines, both current and past, are among the most useful artifacts in reconstructing not only the realities of life in the black community but also how blacks have perceived and defined their minority position in America. *Propaganda and Aesthetics*, while not a social history of Afro-American magazines, provides a clearly written introduction to the shifting currents of twentieth-century political and cultural nationalism as writers and critics debated whether art justified itself or should be for the purpose of propaganda and racial progress and demonstration.

Abby Arthur Johnson and Ronald Maberry Johnson focus on two types of periodicals, those giving priority to political and racial issues over cultural expression and genuine "little magazines" with a specific creative mission. The publications of the early twentieth century were primarily race periodicals, not specifically the vehicles for literature,

preoccupied as they were with outlining political alternatives to the accommodation of Booker T. Washington. Yet in printing short fiction and poetry, they openly acknowledged that art had its role in racial progress. This principle was more clearly confirmed in the 1920s as *Crisis*, *Opportunity*, and the *Messenger* did much to shepherd the Harlem Renaissance, even though they were "political" journals representing social movements. The little magazines of that period, had they survived and been published with greater regularity, would likely have developed in greater depth the debate over art versus propaganda that was only implicit in most of them. Yet the Renaissance dialogue over black esthetics reached beyond the pages of black publications, as, for example, the positions enunciated by Langston Hughes and George Schuyler in the *Nation*; unfortunately, the authors neglect to tie these discussions into their subject.

The decade of the 1930s marked a decline in the literary interests of black periodicals, although the occasional little magazine or college review permits the authors to chart left, right, and centrist variations of the art and politics theme. A more coherent pattern, labeled "the aesthetics of integration," emerged in the 1940s and 1950s, which focused on improved race relations and their portent for broader black participation in society as a whole. But by the height of the civil rights movement in 1963, this vision of a multiracial future began to be attacked in several new little magazines. The enemy—white society and its black political and cultural lackeys—was easy enough to identify and vilify, but the creation of a single esthetic vision that could define the role of black art in a revolutionary political context proved impossible to construct; editors and contributors to the various magazines "could not achieve a consensus on the meaning of separatism, nationalism, and revolution. . . . As a result, the political implications of art-for-people's sake, a slogan used as a partial definition of the black aesthetic, never became sufficiently clear" (p. 198).

As the Johnsons make clear, there never was, nor likely will be, a single black esthetic. One can expect that the debates of past generations will be repeated in the future. What one finds lacking in this volume, however, is a deeper probing. What did the black periodicals have to say about the specific roles of poetry, fiction, drama, and criticism in achieving either a revolutionary separatist or an integrated future? They slight, for example, Theophilus Lewis's extensive discussion in the *Messenger* of the role of the popular theater in race liberation. One would also like some hints as to the importance, if any, of the magazines surveyed and their literary and critical contents to the lives of the masses, as distinct from the Talented Tenth. In short, the implications

of this study go far beyond the pages and readership of the periodicals studied in this volume.

THEODORE KORNWEIBEL, JR.
San Diego State University

JOHN W. JEFFRIES. *Testing the Roosevelt Coalition: Connecticut Society and Politics in the Era of World War II*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1979. Pp. xiv, 312. \$16.50.

By now it is truism that the political revolution encompassed in the Roosevelt coalition established the Democratic party by 1936 as the dominant party in American political life. Forged through those New Deal policies and programs affecting primarily urban areas, it brought various ethnic groups, Catholics, Jews, blacks, labor, and the poor into a political coalition neatly balancing ethnic and class divisions. John W. Jeffries uses Connecticut as a case study to examine these developments because, as he argues convincingly, its society and politics revealed much about the politics and society of similar heavily urban and industrial states, let alone the United States in general. By focusing on elections from 1936 through 1946 and utilizing sophisticated analytical techniques, a wide range of primary sources, and a clear, crisp narrative style, Jeffries persuasively demonstrates how during this decade the Roosevelt coalition made the party a powerful factor in Connecticut political life.

To be sure, the Democrats carried Connecticut only in those elections in which FDR headed the ticket, losing the state in every non-presidential election. But in these elections the parameters of the coalition were not destroyed, although top Connecticut Republicans, particularly Raymond Baldwin, made inroads on it by sponsoring legislation, instituting programs, and championing ethnically balanced tickets. In addition, the coalition was challenged by foreign policy issues such as the role of Italy in World War II, the fate of Poland, and the beginnings of the Cold War. But, despite these challenges, ample evidence is presented indicating that Connecticut elections were predicated on domestic issues concerning the economy and the role of government in assuring peace and prosperity. Though issues changed and new challenges arose, Jeffries abundantly records the continuity that prevailed in the structure of Connecticut society and hence in its politics. And his analysis nicely relates Connecticut to the national political scene, developing the interaction between national issues and state politics. His conclusion, an obvious and unexceptional one given his thorough and exceptional analysis, is that while the Roosevelt coalition did not make Connecticut a solid Democratic state, one

that party leaders could take for granted, it nevertheless forged a powerful two-party system, one in which elections would be sharply contested. For the Democrats to be successful they would have to hold the coalition intact and try to broaden the appeal to voters in Connecticut towns and suburbs, while the Republicans would have to take a progressive stance and make inroads on the urban bastions comprising the core of the coalition's strength.

His conclusion is also obvious and unexceptional because it was earlier presented in a penetrating study, *New England State Politics* (1959) by Duane Lockard, a student of V. O. Key. While Lockard's two lengthy chapters on Connecticut in many ways parallel and complement Jeffries's fine study, they are nowhere cited in his lengthy footnotes (twenty-two pages) or in his otherwise comprehensive bibliographical essay.

RICHARD LOWTET
Iowa State University

NORMAN W. SPELLMAN. *Growing A Soul: The Story of A. Frank Smith*. Dallas, Tex.: SMU Press. 1979. Pp. xiii, 513. \$15.00.

This biography narrates the life of a Texas clergyman who attained the rank of bishop in the Methodist Episcopal Church, South, in 1930, who served as first president of the Council of Bishops of the Methodist Church following the merger of his denomination with its northern counterpart, and who was particularly identified with several church-related enterprises, including Southern Methodist University, Southwestern University (Georgetown, Texas), and Houston Methodist Hospital. As much a curiosity as the book's title is the fact that another Angie Smith—William Angie, Jr., brother of the subject, Angie Frank—served concurrently as a Methodist bishop beginning in 1944.

Though the volume is based on extensive research and its subject was a figure of some renown, it will neither appeal to a wide readership nor win high marks as a scholarly contribution. It is too much a recitation of worthy goals attained, particularly in the matters of membership accessions, enlarged physical plants, fund raising, and the like. *Growing A Soul* can perhaps be of greatest use to students of the Methodist bureaucracy. Clearly, Smith was a dedicated shepherd, and, clearly, he had winning manners, a pleasing appearance, organizational ability, and a gift for public address. "You look like a bishop, you walk and talk like a bishop, and above all you preach like a bishop!" an admiring fellow prelate once exclaimed (p. 342).

On the other hand, Smith's rise to eminence also seems considerably attributable to a calculating eye for enlarged opportunities, to a compatibility with

affluent, establishment-oriented laymen, to ties with powerful senior churchmen, and to a knack for shunning controversy. Amidst a furor over the Ku Klux Klan during his pastorate at Houston's First Methodist Church, in the 1920s, Smith determined that "this issue was not to enter our board meetings" or cripple the church program overall (p. 135). With both a Klan organizer and a judge who led the attack on the Klan conspicuously involved in congregational affairs, Smith tactfully sermonized (as he later explained) on "the power of God to save, Sunday after Sunday" (p. 137). Appropriately, when Bishop James M. Cannon, Jr., a sometimes reform activist and political controversialist, inquired about hospitality in Houston during the national nominating convention of the Democratic party, in 1928, he learned that accommodations were unavailable at the Smith residence. A. Frank Smith stood out among his peers as one who "wasn't tagged as being pro this or pro that" (p. 229).

A dust-jacket blurb accurately advises that the book will help illuminate "the history of Texas Methodism in particular and American Methodism in general through half a century." It seems fair to say that Smith's own estimable contributions lay principally in the spheres of spiritual enrichment, administrative expertise, tact, and the quieting of contention.

KENNETH K. BAILEY
University of Texas,
El Paso

DORIS B. MCLAUGHLIN and ANITA L. W. SCHOOMAKER. *The Landrum-Griffin Act and Union Democracy*. Ann Arbor: University of Michigan Press. 1979. Pp. vi, 288. \$22.50.

In 1959 Congress enacted the Labor Management Reporting and Disclosure Act (LMRDA), popularly known as the Landrum-Griffin Act after its authors. This law came as a result of strong public and congressional support, similar to that which twelve years earlier had prompted the Taft-Hartley Act. In the later instance, Senator John McClellan's investigations of abuses in the Teamsters Union had demonstrated a need to protect individual members from their own officials.

Depending mainly on questionnaires and interviews, this study seeks to determine (1) if the law improved internal union democracy; (2) the effect of court decisions in defining statutory language; and (3) the role of Department of Labor (DOL) officials in protecting individual rights. The book's appendixes include a reprint of the law and the procedures of the investigators as well as tabulations of the results of the questionnaires used. The au-

thors conclude that the overall effects of the law have been beneficial in furthering union democracy, and they recommend a number of changes for DOL officials in administering the policy. The study is concerned primarily with determining the opinions of those involved in the policy rather than with a history of how the law has operated. A detailed account of how this policy has affected the procedures of the Teamsters, Mineworkers, and other unions would be welcome, but that would be asking the authors for a different book.

This study was begun under a contract with the DOL. Why the University of Michigan Press subsequently published it is not clear to this reviewer. It obviously would be of benefit to the Department of Labor, both in recommending changes in the law to Congress and in revising its *LMRDA Interpretive Manual*. In fact, DOL officials have already implemented some of the suggestions. The historian of recent American labor history, however, will find it to be tedious reading and will discover little of substantive value.

R. ALTON LEE
University of South Dakota

ALFRED E. ECKES, JR. *The United States and the Global Struggle for Minerals*. Austin: University of Texas Press. 1979. Pp. xi, 353. \$18.95.

In his preface to *The United States and the Global Struggle for Minerals*, Alfred E. Eckes, Jr., informs his readers that "for purposes of organization" and because they have been or will be covered elsewhere, agricultural commodities and energy materials have been excluded from his coverage. As a result, except for occasional references, Eckes neglects that most crucial of global minerals in the twentieth century, oil. Moreover, because of the unavailability of source materials, Eckes's coverage of the 1960s and 1970s is superficial (he treats the Kennedy-Johnson years in seven pages). Finally, although sensitive to the danger of giving his work too deterministic a tone, some of his specific interpretations, especially his emphasis on raw materials as an explanation for Germany's thrust into the Soviet Union during World War II, seems strained (one also wonders why Eckes deals with such matters in a book concerned primarily with United States foreign policy.)

Indeed, Eckes's own analysis suggests that throughout much of the twentieth century, United States leaders actually ignored minerals as a major factor in the formulation of foreign policy. For example, until World War I, the mineral resource issue in the United States was seen only as a domestic one. Although World War I revealed to some the political importance of minerals, at Paris after the war Woodrow Wilson was not interested in sub-

stantive economic or natural resource issues. Likewise, before World War II President Roosevelt showed little concern with the minerals question, even blocking a materials purchase program.

To be sure, other Americans in and out of government recognized the world's resource interdependence and pushed for an official minerals policy along internationalist lines. In the 1920s, for example, Herbert Hoover sought to break up the foreign cartels controlling many of the world's mineral resources and, although Eckes mentions it only in passing, the state department made the opening of the Middle East to American oil interests a high priority. Other examples can also be cited.

As Eckes himself indicates, however, it was not until after World War II that the quest for secure mineral supplies became a high foreign policy priority (with the notable exception of oil). Faced with increasing demand for minerals, having outgrown its own domestic resources, and threatened with Soviet cannibalization of American foreign resource supplies, the United States opted for an internationalist minerals policy (one that paralleled President Truman's internationalist outlook), including a massive stockpile procurement program. As Eckes comments, by embodying this point of view the famous Paley Report of 1952 marked "a significant shift in national materials policy" (p. 198).

In other words, Eckes's own evidence seems to undermine much of his case as to the importance of minerals to American foreign policy, at least for the period prior to the 1950s, a case that, incidentally, could have been bolstered substantially had he given more attention to international petroleum policy. At least as important to American foreign policy as the proponents of an international minerals policy, it seems, were the opponents and the disinterested in policymaking positions. Eckes does not neglect these groups, but he fails to measure fully their impact on policy formulation.

Eckes appears on stronger ground in his coverage of the last two decades, although, as already indicated, his treatment of the period is necessarily thin. In the 1960s, the resource scarcity thesis (as opposed to resource exhaustion) on which the Paley Report rested was challenged by successive administrations, which were concerned about mineral surpluses and which sold off a large part of the nation's stockpiled materials. A combination of factors in the 1970s, however, including a growing world demand for raw materials, a decline in mining investment abroad, a consequent rise in prices, and continued world competition with the Soviet Union has led to a new scramble for mineral supplies and a renewed emphasis on minerals acquisition as a foreign policy priority. "The U. S. was too dependent on other nations for the commodities needed

to sustain an advanced industrial society," Eckes concludes, "for policymakers to ignore external events that might jeopardize economic security" (p. 255).

Although one can quarrel with some of its interpretation and omissions, *The United States and the Global Struggle for Minerals* does reveal that the resource concerns of the 1970s are not unique to this period.

BURTON KAUFMAN

Kansas State University

PHYLLIS R. PARKER. *Brazil and the Quiet Intervention, 1964*. (Texas Pan American Series.) Austin: University of Texas Press. 1979. Pp. xiv, 147. \$9.95.

In 1964, the Johnson administration gave moral support to, and prepared material assistance for, a right-wing conspiracy to overthrow the constitutional president of Brazil, João Goulart. Both the Brazilian conspirators and their American allies saw the hand of international communism in Goulart's efforts to mobilize urban workers and landless peasants. Afterwards they joined in chorus chanting that a coup d'état of the "democratic forces" against the legal president had been necessary to save Brazilian democracy. It is now clear that the only foreign hand violating Brazilian sovereignty was the American one. Moreover, the road to democratic salvation took a fifteen-year detour into an often nightmarish military dictatorship. In many ways the 1964 coup was a testing ground for methods employed against the Salvador Allende administration in Chile in 1973.

Anyone wishing to grasp the full extent of the disaster that scarred American diplomacy in the 1960s should contemplate the readiness of American officials to meddle in the internal affairs of its largest neighbor to the south. Phyllis R. Parker has used recently declassified American materials and interviews with Ambassador Lincoln Gordon and his military attaché, General Vernon Walters, and then Assistant Secretary of State Thomas Mann to reconstruct the steps that led to the creation of Operation Brother Sam, a stand-by supply effort, and to recognition of an illegally established government. Her analysis is thin and limited to presenting the American conspirators' views of the situation. Since the details of Ambassador Gordon's story apparently have changed over the years and seem to clash with the documentary evidence, it is pointless to use his testimony without corroboration, as Parker repeatedly does here.

The declassified material in the Kennedy and Johnson libraries is fascinating and might be worth publishing as edited, annotated documents, but their use did not justify publication of this volume.

Not only has Parker not utilized the extensive bibliography that has appeared in Brazil since 1964, but also she has ignored Jan K. Black's *United States Penetration of Brazil* (1977), which was available in dissertation form in 1975 and which covers the same ground more completely. By not insisting that she do a thorough job, the University of Texas Press did Parker and itself a disservice.

Its shallowness aside, the book has an important message that the American public needs to hear repeatedly. "There seems to have been," Parker concludes, "no conflict in the minds of U.S. officials over someone's believing in the democratic process and at the same time plotting to overthrow a constitutional democracy . . ." (p. 107). The question is, does someone who wants to overthrow democracy anywhere really believe in it? For those who were listening in 1964, the Brazilian intervention was anything but quiet. Sadly, subsequent interventions proved that too few had been listening.

FRANK D. MCCANN
University of New Hampshire

W. DAVID LEWIS and WESLEY PHILLIPS NEWTON. *Delta: The History of An Airline*. Athens: University of Georgia Press. 1979. Pp. xiii, 503. \$15.00.

This solid volume is one of the first scholarly histories of a major American airline. W. David Lewis and Wesley Phillips Newton were given full access to company records and they have written a detailed narrative account of the fifty-year history of Delta Air Lines.

The leading figure in the evolution of this pioneer southern airline was C. E. Woolman, a University of Illinois graduate who moved to Louisiana before World War I to work with the agricultural extension service. By 1925 the extension agent became involved in a crop-dusting venture in the delta country with headquarters at Monroe, Louisiana. The small business grew, and Woolman plus a few associates formed Delta Air Service, which in 1929 started offering passenger flights in a single, second-hand, six-passenger Travel Air monoplane flying from Birmingham to Dallas. Air traffic was light in the early 1930s and often crop-dusting receipts were larger than passenger revenues. Some improvement came in 1934 when Delta was granted a federal airmail contract for a route from Fort Worth to Charleston. During the remaining years of the decade airmail receipts were considerably greater than passenger income. Prosperity came with World War II. During the war the load factor increased from 43 to 90 percent, and the fleet of DC-3s increased from five to ten.

In the early postwar years Delta sought to expand through merger and in 1952 combined with

Chicago & Southern, a smaller airline serving such mid-American cities as Chicago, Kansas City, Memphis, and New Orleans. By 1953 the Delta-C & S Air Lines was the sixth largest airline in the nation with 4,200 employees, gross revenues of \$32,000,000, and a net income, after taxes, of over \$4,000,000. Three years later in 1956 Delta finally won approval from the Civil Aeronautics Board to extend its service to Washington, Baltimore, Philadelphia, and New York in "Main Street America" (p. 250). The workhorse in all this expansion was Woolman, founder of Delta and chief executive officer until his death in 1966, who kept employee morale high with the growth of "family feeling" throughout the workforce, who had the final word on new aircraft selection, who directed the CAB campaigns for route extension, and who reluctantly in 1958 agreed to allow Delta to serve alcoholic beverages in flight.

In 1960 Delta retired the last of its DC-3s, the same year that the new DC-8 jet transports took over from the DC-7s. By the time that Delta was fully accustomed to jet service, it had received CAB approval, in 1961, for new western routes from Dallas to Los Angeles and San Francisco. During the decade of the sixties gross revenues and revenue passenger-miles both more than quadrupled, while net income increased more than twelve-fold. In 1972 a merger with Northeast Airlines provided Delta with service in New England, and in 1978 it inaugurated transatlantic service from Atlanta to London. The growth of traffic and increasing prosperity continued in the 1970s. In 1977 a reporter called Delta: "Not the country's biggest airline; it doesn't have the most flights or fly the most passengers. It just makes the most money" (p. 394).

While this is generally a sympathetic corporate history, the authors do not cover up the faults and mistakes of the airline—rather full details are given of several serious aircraft accidents. In addition to their use of company records, Lewis and Newton had numerous interviews with active and retired Delta personnel and support their story with complete documentation. Dozens of pictures, nine excellent maps, and useful appendixes supplement the text. This study should appeal to both the general reader and the student of American transportation history.

JOHN F. STOVER
Purdue University

J. HARVIE WILKINSON III. *From Brown to Bakke: The Supreme Court and School Integration, 1954-1978*. New York: Oxford University Press. 1979. Pp. viii, 368. \$17.95.

J. Harvie Wilkinson III is a young white Southerner, former lecturer at the University of Virginia

law school and editor of the Norfolk *Virginian-Pilot*. His story of the Supreme Court and school integration from *Brown v. Board of Education* to the aftermath of the Bakke case is heavily narrative and, to a considerable degree, personal. During this period he sees the South (by which he means the white South) "progressing" in the area of school desegregation through four successive stages: absolute defiance, token compliance, modest integration, and massive integration. Out of the latter, Wilkinson sees a fifth stage growing—that of resegregation, at least in certain localities in the South, and more recently the North—a development that he traces from Charlotte and Richmond to Denver and Boston and on to Detroit, Pasadena, and Dayton. This he sees as a function of busing, for him an almost unqualified evil, his sequence here moving from "black take-over" through white flight to a new segregation with black school systems in the central city and white systems (plus private white schools) in the suburbs. Woven through this chronicle is a brief background on race relations leading up to *Brown*; and assessment of the *Brown* decision; an evaluation of the "protective role" played by the Supreme Court in "siring" the civil rights movement, succoring it, defending it, and (and here Wilkinson is speculative) more recently moving toward deserting it. Further, as Wilkinson constructs his picture, there is a sincere effort made, as controversial aspects of these developments occur, to present the arguments made by spokesmen on both sides of each controversy, whether this includes public figures from Supreme Court justices to governors and public officials, school board members, local editors, opinion forums, and even, in some instances, reactions of the "victims" of integration policy themselves, who are, by and large, middle- and especially lower-class whites.

The results, in many ways, tell more about the author than the events he treats, but, in the process, tell a great deal about a powerful constituency of like mind currently in central policy-making positions. While admitting freely that pre-*Brown* race relations produced oppression, ignorance, and injustice and acknowledging that in the early years of the civil rights movement there was only one side for people of good will, Wilkinson sees a second valid side emerging by the early 1970s as coercion to achieve school integration took more overt forms. For him, courts should now be asking whether the social and academic benefits actually produced by integration justify the risks and disruptions necessary to achieve it. Thus, as with the man for whom he formerly clerked, Justice Lewis Powell, whose Bakke ruling he gives a cautious endorsement, Wilkinson feels law should be used in such a way as to ensure social stability. The application of constitutional principles, when they lead to community hos-

tility, chaos, and upheaval, should be tempered in hopes of reaching a resolution on the race question more equitable to those for whom forced integration is too disruptive and traumatic. In this regard, busing raises vital moral questions and challenges fundamental principles: "the right to be left alone, to control one's own destiny, to live by one's own lights, not those of an unelected district judges or theorists whose notions of equality have become constitutional law."

Intriguingly, Wilkinson sees himself and the white South generally as having benefited from the positive forces that *Brown* unleashed. However, he also sees some of those forces as far less than positive, with a clear lesson revealed: "School integration has taught us at home what Vietnam did abroad; how much eludes the American capacity to reshape." And, for Wilkinson, continued heavy-handed attempts to reshape may well undermine the kind of progress with which he is comfortable in the race relations area.

PAUL L. MURPHY
*University of Minnesota,
Minneapolis*

G. WILLIAM DOMHOFF. *The Powers That Be: Processes of Ruling-Class Domination in America*. New York: Random House. 1978. Pp. xv, 206. \$10.00.

For years, G. William Domhoff has been arguing that a small class of rich property owners controls American business and politics, acting as a ruling class in the strictest sense of the term. Where other students of social stratification have been content merely to demonstrate the existence of an upper class, Domhoff tries to show that this class actually makes the key decisions in all the major institutions of American society. In his new book, he argues that members of the upper class dominate the "policy network," the selection of political candidates, and the formation of ideology. He takes issue with those who emphasize the importance of special interests, like Grant McConnell, and with theorists of the "power elite," like C. Wright Mills, both of whom fail to see, according to Domhoff, that the power of special interests springs from a larger pattern of class rule.

When he insists that the property-owning class "is as alive and well as it has ever been," Domhoff also takes issue with theorists of the "managerial revolution," who see the old elite as having been displaced by a new oligarchy of administrators, managers, and bureaucrats. As always, Domhoff's work provides a useful corrective to the cruder versions of this theory, according to which the managers care more about growth than profits and use their power for the good of society as a whole. But

his analysis cannot do justice to the historical developments that have made direct class rule a thing of the past. One does not have to accept either the idea of a managerial revolution or the pluralist theory of "veto groups" and "countervailing power" to accept the general validity of David Riesman's observation that "explicit class leadership" died with McKinley. The general crisis of authority is the most obvious sign of its obsolescence, the exhaustion of ruling class ideologies, the inability of elites to provide a coherent justification of their own power, the retreat from confrontations over principles, the resort to psychological manipulation in place of ideological coercion.

When Domhoff tries to show that the ruling class retains a coherent and fully worked out world view, he describes an ideology that now finds support only on the extreme right. *Laissez-faire* liberalism, he claims, continues to enjoy a "near-monopoly of American political thought." In fact, the ideology of "individualism, free enterprise, competition, equality of opportunity and a minimum of reliance upon government" long ago gave way to a therapeutic, permissive style of leadership that dispenses with appeals to authority, sides sentimentally with the underdog and the outcast, and seeks to co-opt dissension through programs of affirmative action and "innovative" social change. That such programs leave the existing distribution of wealth untouched does not establish the existence of a ruling class in any important sense of the term; for as Domhoff himself concedes—though reluctantly, for the sake of argument—we cannot "infer power from the distribution of wealth, income, health, education, and other benefits."

The United States today presents the curious spectacle of a capitalist society in which the capitalist class plays an altogether negligible role. No theory has yet managed to capture the ironies and contradictions of such a situation, and Domhoff's ruling-class theory does not even come close.

CHRISTOPHER LASCH
University of Rochester

CANADA

RICHARD J. DIUBALDO. *Stefansson and the Canadian Arctic*. Montreal: McGill-Queen's University Press. 1978. Pp. xii, 274. \$18.95.

Richard J. Diubaldo's study of Vilhjalmur Stefansson's early career in the Canadian arctic raises important questions about Stefansson and about science and government generally. Diubaldo's purpose is not to trace Stefansson's exploits in lavish and stirring detail but, rather, to discover why "a man whose northern work and ideas about Canada's

northern destiny had fired the imagination of Canadians" became "unofficially ostracized from Canada" in the 1920s and is largely forgotten today. Given Stefansson's accomplishments—the accumulation of large amounts of invaluable anthropological detail, the mapping of much of the arctic, the discovery of major islands claimed for Canada, and the development of techniques for living in the Arctic—his current obscurity in a country ransacking its past for hero-adventurers does seem puzzling. Diubaldo's study shows why it should not be.

Stefansson promised so much. His background had the material of which legends are made. Born in Gimli, Manitoba, of Icelandic parents who lost two children to the hardships of pioneer prairie life, Stefansson had only twenty-seven months of primary education but still completed his education as a fellowship student in anthropology at Harvard. From the beginning, Stefansson had a sense of the advantage of his background and an arrogant awareness of how he might use it. Of Harvard he wrote, it "is the place for the poor man who knows he is made of the right stuff, who has found by experience that he has more inferiors than superiors in ability and courage." And in 1899 "Willy" Stephenson became Vilhjalmur Stefansson.

A Harvard scientist of romantic northern background was naturally lured to the Canadian arctic at the beginning of this century. For their part, Canadians were drawn to Stefansson because science seemed to offer the means of establishing their claims upon an arctic region that common sense told them was too harsh for human habitation and exploitation. In the first years of the new century, common sense had already lost much ground to the claims of science. The Canadian west, which had long resisted settlement and development, was booming because of the railway, the new strains of wheat, and so many other "scientific" innovations. If science had made the west habitable and even prosperous, why could it not do the same for the north? By 1908 Stefansson was writing in *Harper's* that "A reasonably healthy body is all the equipment a white man needs for a comfortable winter among the arctic Eskimos." The white man, of course, wanted to hear this, and he gave Stefansson support for the next expedition. Stefansson saw the advantage of publicity, and it became for him, Diubaldo writes, "the key, or at least a short cut, to success." Yet, when the key opened the door, the path led only to disillusionment and failure. Although publicity won Stefansson entry into the prime minister's office, it also created suspicion of his motives and, justly, his scientific integrity; when Stefansson's expeditions did not produce the results the public and the politicians had been led to expect, his detractors gained the upper hand. The central issue of arctic development and exploration was lost in the

controversy about Stefansson himself. What Stefansson the publicist promised, the scientist always knew was impossible to attain. Canadians began to look elsewhere as Stefansson's scientific colleagues dissected what was left of his arctic dream.

Diubaldo has written an important book based upon thorough research in manuscript sources. He has not always used his material as effectively as he might, and he does not portray Stefansson "in human terms" as the dust-jacket suggests he will. Perhaps only a novelist could. Diubaldo is not a novelist; this book does show that he is a good historian.

JOHN ENGLISH
University of Waterloo

ESCOTT REID. *Time of Fear and Hope: The Making of the North Atlantic Treaty, 1947-1949*. Toronto: McClelland and Stewart. 1977. Pp. 315. \$13.95.

This important contribution to the historiography of the North Atlantic Treaty Organization appears at a time when historians, as opposed to political scientists, have just begun to turn their attention to the subject. Escott Reid, a Canadian diplomat then in the early stages of a distinguished career, was second in command in the Department of External Affairs in Ottawa from April 1947 to March 1949. His chief was Lester Pearson, one of the principal architects of NATO. In this capacity Reid was an active participant in the diplomacy leading to the North Atlantic Treaty. Although he does not use the Achesonian term, "present at the creation," he implies that he was not only more intimately involved than Acheson had been but also a creator of the treaty. In the summer of 1947 he believed (p. 31) that his may have been the first public statement advocating a collective defense system for the Western powers.

His insider's knowledge, however, does not make this just a personal memoir. With careful scholarship he assembled his notes, analyzed the files of the Canadian foreign office, and exploited interviews with such leading American policy makers as John D. Hickerson and Theodore C. Achilles to reconstruct and evaluate the negotiations among the twelve nations of NATO. The result is a full treatment of the complicated negotiations that began with the Brussels Pact of March 1948 and proceeded through four stages ranging from secret negotiations at the Pentagon among the United States, Canadian, and British representatives in the spring to discussion with the Western powers in the summer, to a draft treaty in December, and lastly to working out a final text between January 10 and March 28, 1949. The critical issue was always the extent of America's pledge to frightened Europeans, and the breakthrough was the Vandenberg Resolu-

tion of June 1948. Much of the delay in the completion of negotiations derived from an expectation that no serious American commitment would be concluded until President Dewey was in the White House.

As he traces the course of negotiations, Reid enlivens his book with personal comments. We know his heroes—Hickerson, Achilles, Robert A. Lovett, Arthur H. Vandenberg, Gladwyn Jebb, and Lester Pearson. Although there are no enemies as such, George F. Kennan and Charles E. Bohlen of the United States and Hume Wrong, the Canadian ambassador, are criticized for insufficient recognition either of the imperatives of a multilateral commitment or of the importance of an Atlantic community. Acheson's ability and incisive style Reid balances against his "arrogance of expression," which irritated ambassadors (p. 150). Among the many virtues of the book is a vivid presentation of a Canadian point of view as its diplomats opposed an American unilateral commitment to Europe that would isolate Canada. Ultimately, the friends of the alliance triumphed, just as hope triumphed over fear in 1949.

There are some problems with the work. Although his writing is clear and occasionally stylish, the organization of the material frequently produces repetition. And, although he is justified in seeing himself as a founding father, even creators have lapses. Reid misplaces the date of the signing on one occasion (p. 95) and recalls a meeting with Dulles in the early 1960s (p. 245) on another. Elsewhere (p. 232), he admits that his failings as a diplomat include overzealousness and perfectionism. This should not be an excuse, however, for a reviewer to apply these traits to a criticism of a book that every scholar in the field should read with appreciation.

LAWRENCE S. KAPLAN
Kent State University

JOHN P. SCHLEGEL. *The Deceptive Ash: Bilingualism and Canadian Policy in Africa, 1957-1971*. Washington: University Press of America. 1978. Pp. xviii, 463. Paper \$14.50.

The Deceptive Ash is John P. Schlegel's doctoral dissertation at Oxford, apparently published here without significant alteration. From the standpoint of presentation and format, therefore, it demonstrates the limitations of theses as a genre and may suggest the advantage of indulging in a slight pause between the savoring of a new degree and appearing in print. The text is littered with typographical errors. Sentence structures often emerge in convoluted, and sometimes garbled, form. Verb tenses are occasionally mixed, and at least two sections of

footnotes (notes 119 to 124 on page 187 and 91 to 98 on page 367) are missing. Value judgments frequently appear extravagant or gratuitous. The doctoral candidate's understandable anxiety to establish thematic consistency sometimes leads to excessive repetition. The central argument—that Canada's policies in Africa from 1957 to 1971 were significantly affected by domestic politics—fits well enough with the current preoccupations of many political scientists in foreign policy analysis but may strike those who are detached from the discipline as a rather tiresome statement of the blindingly obvious. There is also a slight tendency to exaggerate the importance of Africa in Canadian affairs, both domestic and foreign—a tendency for which a more distant and leisurely perspective on the subject might well have been the cure. A thesis, in short, does not instantly make a polished book.

Readers who are prepared to overlook such blemishes will find in Schlegel's volume an extraordinarily useful examination of Canada's developing relationships with sub-Saharan Africa, a subject hitherto confined for the most part to a periodical literature of ephemeral quality. For reasons of economy, the book concentrates on Ghana, Nigeria (with special reference to the period of the civil war), "la francophonie," Tanzania, and Southern Africa, but this serves well enough to cover the most important ground, and from it the author has certainly been able to identify the principal features of Canadian policy. With the help of extensive interviewing he has also captured the flavor of much of the pertinent domestic Canadian politics. Given the "contemporary" character of the subject matter and the subtlety of the nuances involved, this is a considerable achievement, and perhaps especially so for a non-Canadian studying at Oxford.

American readers may be particularly interested in chapters 5, 6, and 7, which deal with "la francophonie" and, hence, with the complex and volatile dispute between Ottawa and Quebec (exacerbated by the interventions of France) over the role of the provinces in the conduct of foreign policy. The analysis, although somewhat qualified in the concluding chapter, could leave the uninitiated with the mistaken impression that the issue has been essentially resolved, when in fact it is still festering at the bureaucratic level and is almost certain to become visible again in the near future. A great many details, moreover, remain to be told. Nevertheless, Schlegel's treatment provides as clear a background on the problem as can be found anywhere in readily available English-language sources, and it may soon have a current, as well as historical, relevance for students of Canadian affairs.

It seems unfortunate that Schlegel did not delay his pursuit of a publisher long enough to permit a thorough revision of his text, and hence the creation

of a more mature and finely tuned result. Even as it stands, however, the book is a useful preliminary survey of a subject that thus far has received only sporadic attention in the literature of contemporary foreign policy analysis.

DENIS STAIRS
Dalhousie University

LATIN AMERICA

MARIA PARADOWSKA. *Polacy w Ameryce Potudiniowej* [The Poles in South America]. Wrocław: Ossolineum. 1977. Pp. 315. 55 Zł.

In this work Maria Paradowska provides many new insights into Polish immigration to Latin America, a subject much neglected by scholars outside Poland. Polish scholars have published much of the correspondence that went from Latin America to Poland in which immigrants described conditions in their new home. Although Paradowska indicates her intention to cover long periods of time and all aspects of life, her own ethnographic interests sometimes dominate the book. In preparing her book, Paradowska studied the writings of those who journeyed to Latin America, whether to emigrate or to report for newspapers in Poland. The most valuable part of the book is the extensive bibliography, which appears both in the back and scattered throughout the text. I am surprised, however, that she did not use the wealth of manuscript material available in the area since she acknowledges that rich materials about Poles in Latin America are available in foreign archives and libraries.

Paradowska's book starts long before the coming of Poles to settle in the territories of the Spanish empire; she begins with a short history of the area before the arrival of the Spaniards and Portuguese. Herein lies one of her weaknesses, for she covers too wide a range of topics and too many years. She analyzes Indian culture and lifestyle in a short section that is too general and that lacks innovative writing and research. Yet her account of the first news of Latin America in sixteenth-century Poland is interesting; she cites printed works from as early as 1506. Also of interest is the reference to Jan Dantyszek, who was sent by the Polish king to Spain. There he met with Hernando Cortez and the two men exchanged three "memorials" that are now lost.

The author also turns her attention to the question of whether the Poles discovered America. Many articles have already appeared on this subject and the quarrel about whether Jan Szkolny arrived before Columbus continues. Paradowska spends much time discussing reasons behind the emigration, immigrants' trials and difficulties in the new

lands, and individual and mass emigration up to the present. Also included are names of individuals who contributed to Latin American national development, biographical sketches, and histories of particular countries. There are some photographs and she does footnote some of her work.

This book suffers chiefly from the lack of manuscript material and adequate footnoting and from excessive generalization and superficiality in the coverage of some periods. Yet, for all its weaknesses, Paradowska's work does contribute knowledge in an area just being discovered, European immigration to Latin America.

BERNADINE PIETRASZEK
De Paul University

PIERO GLEJESSES. *The Dominican Crisis: The 1965 Constitutional Revolt and American Intervention*. Translated by LAWRENCE LIPSON. Baltimore: Johns Hopkins University Press. 1978. Pp. xiii, 460. \$22.50.

Politics in the Dominican Republic, especially the civil war of 1965 and the accompanying U.S. intervention, have received so much attention from scholars and journalists that it is difficult to imagine anything fresh being said. Yet Piero Gleijeses has written an original and interesting book, bringing new information, insights, and interpretations to a well-known subject. He succeeds in his attempt to present the crisis of 1965 from Dominican points of view, as opposed to the "abundant literature . . . largely by American authors and primarily treating the American side of events" (pp. xi-xii).

This book is an expanded and revised version of the author's doctoral dissertation at the University of Geneva, translated from the original French. It is meticulously researched and impressively full of fascinating detail. In addition to a documentary base, Gleijeses interviewed a large number of participants in the events analyzed. The study is straightforwardly organized in chronological order. Chapter 1 provides sweeping historical background from the Spanish "discovery" through dictator Rafael Trujillo's assassination in 1961. Chapters 2 through 4 further set the stage, treating various aspects of the period from Trujillo's death to the Constitutionalist coup on April 24, 1965, against the Triumvirate government. Five subsequent chapters deal in virtual hourly detail with events of April 24-28, 1965. Chapter 10 sketches occurrences to June 1, 1966, and the presidential election of Joaquín Balaguer; chapter 11 summarizes the author's previously elaborated conclusions. Three appendixes are attached. One deals with the U.S. role in Trujillo's assassination, and another with the evolution of Dominican far-left parties until early 1962. The third appendix reproduces a contemporary Art Buch-

wald column satirizing U.S. interventionist policy in 1965. Ninety-seven pages of notes are included; much of the valuable commentary buried here would be better placed in the text. A twenty-four-page bibliographic essay is useful. The index is restricted to names of participants.

Glejeses analyzes the complicated mix of Dominican factions and U.S. involvement, sorting out personal interests and various policy motivations. His sympathies are clearly, sometimes emotionally and even passionately, with the Dominican left. He does not disguise his hostility to Dominicans identified with the status quo and to U.S. officials (John Bartlow Martin receives especially harsh criticism). But Glejeses does not fall into the trap of viewing the Dominican crisis in terms of heroic Constitutionalist pitted against villainous Loyalists, as did some authors of earlier works on the subject. He recognizes the simplicity of this approach and the remarkable complexity of Dominican politics, finding opportunism and idealism on both sides. While specialists will quarrel with some of Glejeses's provocative interpretations, he has made a major contribution to our understanding of the unique and enigmatic Dominican Republic.

G. POPE ATKINS
U. S. Naval Academy

W. F. ELKINS. *Street Preachers, Faith Healers, and Herb Doctors in Jamaica, 1890-1925*. New York: Revisionist Press. 1977. Pp. 99. \$44.95.

W. F. Elkins has compiled in this volume what he calls "historical vignettes" on Jamaica. The book is one volume of what seems to be "studies in the Caribbean series." Although the book is weak in scholarship (most of his documentation being derived from newspaper articles in general and the *Jamaican Gleaner* in particular), the content is well written.

Elkins tried to present in simple style the historical development of the island from 1890 to 1925, in which he showed the tension existing between British colonial rule of law and order and the gradual development of political self-awareness in the Jamaican peasant community. This self-awareness emerged within what he called the "revival milieu," which came about as a synthesis of Afro-Jamaican possession cults; the Native Baptist tradition, which entered the island during slavery under the leadership of George Liele, an American slave; and Christian missionary religion. The combination of all three culminated in the Great Revival of 1860-61, in which the present religious milieu had its origin.

Elkins characterized revival movements as an "illusory inward escape from mundane concerns," but, at the same time, these movements retained a cer-

tain characteristic for political activities, which he called "a slumbering radicalism" that was very much "this-worldly." The revolutionary dimension of this movement precipitated the Sam Sharpe Rebellion of 1831, commonly known as the "Baptist War," and the Morant Bay Rebellion of 1865 under the two Native Baptist leaders, Paul Bogle and George William Gordon. Both of these rebellions brought about drastic changes in British colonial rule. The first contributed to the abolition of slavery, the second to the withdrawal of independence of Jamaica from the Plantocracy to Crown Colony.

Elkins further discussed various healers, both men and women, who emerged between 1890 and 1920, as curers of illness both by supernatural means and by the use of herbs and the emergence of the revival leader, Prophet Bedward, and his work at Mona River close to the University of the West Indies. He also traced the origin of the two most prominent sects in Jamaica, the Revival Zion and the Pocomania. The emergence of the Salvation Army through the works of W. Raglan Phillips and the rise of Ethiopianism, the forerunner of the present-day Ras Tafari movement, are also considered.

Despite weak scholarship in documentation, the book is delightful reading, neatly organized, and insightful to readers who have no knowledge of this rather exciting period in Jamaican history.

LEONARD E. BARRETT
Temple University

LAURENS BALLARD PERRY. *Juárez and Díaz: Machine Politics in Mexico*. (The Origins of Modern Mexico.) DeKalb: Northern Illinois University Press. 1978. Pp. xx, 467. \$25.00.

Laurens Ballard Perry's book is a long and detailed narrative of the political and military history of the Restored Republic (1867-76). The era was one of intense factional conflict and electoral corruption under Presidents Benito Juárez and Sebastián Lerdo de Tejada, which culminated in a three-way struggle for the presidency in 1876 among the incumbent Lerdo, the president of the Supreme Court and vice-president, José María Iglesias, and the successful rebel, Porfirio Díaz. The book is the product of intensive research in the Díaz archives (which the author himself organized for public use) as well as in the papers of Juárez, Justo Benítez, and Fernando Iglesias Calderón and in some major newspapers. Perry's volume supplements the works of Frank Knapp and Daniel Cosío Villegas and adds much new information on state politics and on electoral practices; moreover, it may become the definitive account of the complicated events of 1876.

The triumph of Díaz came not by default but as a result of a careful military plan and the use of guerrilla tactics learned in the struggle against Maximilian.

The central argument of the book is that the real conditions of Mexico in 1867 made it impossible for Juárez and Lerdo to follow the ideals of "liberal republicanism" as enunciated in the Constitution of 1857 and the Laws of Reform. Federalism, a limited presidency, and separation of powers gave way to "executive centralism"; democratic electoral procedures to fraudulence and machine politics; social democracy to rural abuse. Perry argues further that the gap between liberal ideals and actual political practice gave stimulus to a rebellious opposition, culminating in the Díaz victory. Juárez and Lerdo, though publicly clinging to liberal theories, contradicted them in practice and thus were little different from President Díaz after 1876. Perry adheres to the theme of continuity from Restored Republic to Porfiriato, suggested by Emilio Rabasa (1912) and Knapp (1951) but denied by Cosío Villegas (1955). In fact, much of the author's discussion seems a refutation of Cosío's contention that the Restored Republic represented the high point of liberal constitutional government in Mexico.

The strengths of the book—its impressive research and its wealth of detail—are nonetheless undercut by conceptual weakness and confusion. The author's argument rests on demonstrating the inadequacies of the "liberal model" as a guide for post-1867 governments, and yet he never really plumbs the complexities of political liberalism. For him, it seems to mean solely the limitations on central government authority outlined in the Constitution of 1857. What Perry ignores is that political liberalism also entailed the secularization of society and the destruction of corporate inequality. This was the program of the Reforma, one recognized by the reformers themselves as anticonstitutional because it necessitated the strengthening of the state. The "executive centralism" of Juárez and Lerdo after 1867 had its direct antecedent in the ideals and practice of the Reforma. Political liberalism in Mexico, as in France and Spain, has always contained two contradictory "models," constitutionalism and the secular state. Juárez and Lerdo (and Díaz) could thus be centralists and also good liberals. The author seems insensitive to the subtle interaction between theory and practice in the political system of the Restored Republic (and in the Porfiriato) and particularly to the effects of liberalism as an all-embracing myth following the heroic liberal age. Moreover, Perry's account shows traces of the traditional Anglo-American judgmental attitude toward Latin politics, making it reminiscent of the old studies of the Spanish colonial system that emphasized the breach between the written law and its

observance. Despite these flaws, the book will stand as an important reference for the politics of the years 1867-76.

CHARLES A. HALE
University of Iowa

GUILLERMO HERNÁNDEZ DE ALBA and JUAN CARRASQUILLA BOTERO. *Historia de la Biblioteca Nacional de Colombia*. (Publicaciones del Instituto Caro y Cuervo, number 38.) Bogotá: The Institute. 1977. Pp. xviii, 447.

The Biblioteca Nacional in Bogotá is the oldest national library in the Americas, indeed, is older than any but about a half dozen national libraries in Europe. With the expulsion of the Jesuits in 1767, their property was turned over to the crown. The society's library in Santafé de Bogotá contained 4,182 volumes. Largely at the instance of the creole Francisco Antonio Moreno y Escandín, the Real Biblioteca of Bogotá opened its doors in 1777. The first of sixty-three librarians through the bicentennial was Anselmo Álvarez. Among them have been such brilliant names as Miguel Antonio Caro, Daniel Samper Ortega, and Tomás Rueda Vargas.

Here is the first chronologically complete history of the first two centuries of the library. Eduardo Posada, the noted Colombian bibliographer and historian, published *Narraciones: Capítulos para una historia de Bogotá* in 1906, with one chapter on the library, the best available study to date. The present work uses many documents and much iconographic material that could not find a home in Posada's relatively short work. The authors have used original documents (manuscript and printed), new printed editions of sources, and many portraits, photographs, and facsimiles (74 plates in all). This book is mainly a framework, however, and the critical analysis of the past history of the Biblioteca Nacional and its prognosis for the immediate future is a chore that remains to be done.

The story of the Latin American national libraries, even the fairly large ones in Mexico and Rio de Janeiro, is, in general, pathetic. They have received nothing like the support given to European national libraries, even in the smaller jurisdictions. They have been political booty for men of letters and other persons on the right side (as have many state libraries, even today, in the U.S.). The present work reflects this situation, but it is an eligible point of departure for future studies, particularly with the indication of source material. There are only bibliographical footnotes, not a bibliography.

There is a section on special collections. Like most of the recent U.S. "research" libraries, the collections are spotty, with isolated cimelia. Most valuable are the collections on Colombian literature and history, of which a catalogue would be useful. A few

of the other special collections also deserve to be made better known through catalogues.

The gradually increasing interest in the library history of Latin America is most encouraging. Perhaps more than most institutions, libraries can profit from the examination of the past, for the physical evidence of achievements and mistakes is on all sides and must be handled on a continuing basis. Here is a cornerstone for Colombian library history.

LAWRENCE S. THOMPSON
University of Kentucky

JONATHAN C. BROWN. *A Socioeconomic History of Argentina, 1776-1860*. (Cambridge Latin American Studies, number 35.) New York: Cambridge University Press. 1979. Pp. xiv, 302. \$24.95.

A Socioeconomic History of Argentina, 1776-1860 is actually a study of agriculture and commerce within the province of Buenos Aires. The author ably relates the provincial economy to the riverine, interior, and other littoral provinces and to the world market. Jonathan C. Brown enters the dependence theory controversy by stating that it provides an unsatisfactory explanation of Argentine development. He concludes that the more realistic explanation of Argentine economic growth is found in staple theory, which proposes that the production of raw materials and food crops rather than creating a neocolonial situation provides the basis of development.

Brown's research is impressive both in utilization of Argentine archival sources and in examination of printed materials. He imaginatively utilizes information relating to commerce and agriculture that has been ignored and concisely summarizes relevant literature, thereby presenting for the first time a whole picture of the nineteenth-century agricultural and commercial operations within the province of Buenos Aires.

This, however, is a disappointing book because the title leads one to expect a great deal more. In no sense is this work a socioeconomic history of Argentina. Brown provides no clear picture of Argentine mining and industry. He fails to describe the financial infrastructure. He presents rather limited information on immigration and the family and nothing on religion, education, or social classes. Furthermore, one does not find information on agriculture within the riverine and interior provinces, although Brown's discussion of the links to the province of Buenos Aires and the benefits that the other provinces received from their association supports his analyses of staple economic growth. The author favors the thesis that the independence movement did not immediately bring about the decline of the riv-

erine provinces. The closer the provinces to Buenos Aires, the greater the economic growth.

Brown should have titled his work "The Developments of Agriculture and Commerce of the Province of Buenos Aires, 1776-1860." He describes the growth of Buenos Aires trade from contraband to legitimacy, changes in trade patterns with the coming of independence and increased technology, the river and cart routes, and the expanding frontier of the province of Buenos Aires. Of particular interest are accounts of the provincial links to Potosí and Asunción and descriptions of the European industrial market for Argentine wool, hides, tallow, and meat. Brown also provides the first detailed information on *estancia* management through the examination of the Anchorena family operations. He challenges traditional theories of land patterns by proving decreasing size of holdings in the nineteenth century and arguing for the significance of the small holder and the ease of land purchase. The nineteenth-century watercolor and oil painting illustrations are excellent as are the thirty-four tables and six maps and graphs.

Jonathan Brown has made a contribution to the economic history of Latin America, and his work should be consulted by those working on nineteenth-century trade and agriculture. However, a socioeconomic history of Argentina remains to be written.

VERA BLINN REBER
Shippensburg State College

ROBERT J. ALEXANDER. *Juan Domingo Perón: A History*. Boulder, Colo.: Westview Press. 1979. Pp. xiii, 177. \$15.00.

The death of Argentine President Juan D. Perón in July 1974 put an end to the career of that country's most controversial personality of the present century. Robert J. Alexander, who published one of the earliest studies on Perón's first period of rule (*The Perón Era* [1951]) has now produced what is apparently the first book in English to cover Perón's entire political career.

Alexander's new work is not a biography in the usual sense. The personal side of Perón's career is treated in a chapter entitled "The Young Perón" and briefly in other chapters, especially one devoted to his second wife, Evita, but there is no attempt to delve deeply into the psychological factors that may have explained Perón's actions. Rather, what Alex-

ander has done is to present in this slender volume a narrative and interpretive study of Argentine history from 1943 to 1974. The bulk of the volume deals with the years leading up to Perón's ouster from power in 1955, but the last forty-three pages treat the years in exile and the return to power in the 1970s as well as offer an overall assessment of Perón's role.

For those unfamiliar with recent Argentine history, this book can be recommended (save for the reservation noted below) as providing a useful summary and interpretation of the Perón-dominated years. Alexander, who visited Argentina thirteen times between 1946 and 1974 and conducted perhaps a thousand interviews, offers explanations for many crucial developments of those years, and he does not hesitate to take firm positions on such perennially intriguing questions as to whether it was Perón or Evita who was the more important in their political partnership. Alexander's judgments about Perón reflect his own social democratic preferences, but this book is far more sympathetic to an understanding of Perón's economic and social policy objectives than his earlier volume. His conclusion that Perón has a tragic figure rather than a great one and that he directed his great political talents more often to destructive than to constructive ends will seem persuasive to many who try to understand the tragedy of recent Argentine history.

In the preface to his work, Alexander describes it as "an interpretive essay rather than a scholarly treatise" and therefore "unencumbered by the paraphernalia of footnotes and other scholarly impedimenta" (p. xi). The absence of scholarly apparatus, however, is no excuse for the numerous errors of fact that appear in the text. The author is quite cavalier in his use of dates (see pp. 1, 12, 18, 25, 56, 104, 107, 117); he is also mistaken in a number of matters of varying importance from the role of General Aramburu during and after the 1955 revolt to the details of the 1962 election that led to President Frondizi's ouster. In what is perhaps the most notable confusion, Alexander describes as occurring on a single day, June 15, 1955, events that happened two years apart on April 15, 1953, and June 16, 1955 (p. 104). These are the kind of lapses one might expect to find in the first draft of a book of recollections, not in an edited volume presented to the public under the title of a history.

ROBERT A. POTASH
University of Massachusetts,
Amherst

Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

PAUL GORDON LAUREN, editor. *Diplomacy: New Approaches in History, Theory, and Policy*. New York: Free Press. 1979. Pp. xvi, 286. \$25.00.

PAUL GORDON LAUREN, *Diplomacy: History, Theory, and Policy*. GORDON A. CRAIG, *On the Nature of Diplomatic History: The Relevance of Some Old Books*. ALEXANDER L. GEORGE, *Case Studies and Theory Development: The Method of Structured, Focused Comparison*. MELVIN SMALL, *The Quantification of Diplomatic History*. OLE R. HOLSTI, *Theories of Crisis Decision Making*. SAMUEL R. WILLIAMSON, JR., *Theories of Organizational Process and Foreign Policy Outcomes*. RICHARD SMOKE, *Theories of Escalation*. PAUL GORDON LAUREN, *Theories of Bargaining with Threats of Force: Deterrence and Coercive Diplomacy*. ROBERT JERVIS, *Systems Theories and Diplomatic History*. ROGER V. DINGMAN, *Theories of, and Approaches to, Alliance Politics*. SAMUEL F. WELLS, *History and Policy*.

PAWEŁ CZARTORYSKI *et al.*, editors. *Science and History: Studies in Honor of Edward Rosen*. (Studia Copernicana, number 16.) Wrocław: Ossolineum; distributed by Neale Watson Academic Publications, New York. 1978. Pp. 553. \$35.00.

PAWEŁ CZARTORYSKI, Foreword. PAUL OSKAR KRISTELLER, *The First Printed Edition of Plato's Works and the Date of Its Publication* (1484). MARSHALL CLAGETT, *Francesco Maurolico's Use of Medieval Archimedean Texts: The *De sphaera et cylindro**. CHARLES B. SCHMITT, *Filippo Fantoni, Galileo Galilei's Predecessor as Mathematics Lecturer at Pisa*. VASCO RONCHI, *Two Thousand Years of the Struggle between Reason and the Senses*. NICHOLAS PASTÖRE, *"In His Eye, or Rather In His Mind."* ERNEST NAGEL, *The Supremacy of Method*. A. I. SABRA, *An Eleventh-Century Refutation of Ptolemy's Planetary Theory*. PEARL KIBRE, *"Astronomia" or "Astrologia" Ypocratis*. OLAF PEDERSEN, *The Decline and Fall of the Theorica Planetarum: An Essay in*

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

I read Walter J. Brunhumer's review of my *Leibniz and Confucianism: The Search for Accord* (AHR, 83 [1978]: 1040-41) with dismay. I accept his criticisms regarding the lack of development of Leibniz's personality and the weaknesses in my argument concerning the reasons why the search for accord failed. But I find it harder to accept his omission of reference to chapters 4 and 5, which, as other reviewers have correctly noted, constitute the core of the book. These chapters trace the transmission of Chinese concepts from the Chinese texts through the missionary translators to Leibniz, and this material is the basis for saying that my book is one of the "first works to begin to document the relationship between the two worlds" of China and Europe that began in the sixteenth and seventeenth centuries (*Choice*, 15 [May 1978]: 318-19).

I do not fault Professor Brunhumer for his treatment of European intellectual history. I fault him for his avoidance of Chinese intellectual history. From his dubious postulation of Chinese parallels to the European faith-reason tension, it appears that he has little understanding of China. In fairness, the problem is not the reviewer's alone. Many historians continue to resist merging areas of expertise, a resistance that is reflected in the AHR's review editors giving the book to a European historian, rather than finding one with a dual (European-Chinese) expertise, and publishing the review under the "Europe" instead of the "General" category. (In doing so, the AHR ignored the Library of Congress classifications on the second page, of which three out of four refer to China.) The problem is that, unless we begin to merge areas of expertise, we are never going to appreciate the rich confluence of histories, such as occurred between Europe and China, which

is increasingly crucial to an understanding of the modern world. Some of the intellectual fences that historians have built between Europe and China are not part of history.

Writing a disagreeable letter over a review of one's own book is surely not the best way to open minds to a new approach. Furthermore, by writing it I realize that I wave the banner of self-interest. But an important issue is involved. And, since I did try first to write a book that would prod historians in that direction and since the primary objective of the book has been omitted from the review, perhaps this letter may be received more in the spirit of scholarly emendation than of authorly revenge.

DAVID E. MUNGELLO

Leibniz-Archiv

Hannover, West Germany

PROFESSOR BRUNHUMER REPLIES:

In his letter as in his dissertation, David E. Mungello displays a distressing tendency to jump to conclusions on the basis of misunderstood evidence. Apparently persuaded that it is his mission to expose the self-evident evils of "sinological torque" (Western misconceptions of Confucianism), and overlooking the fact that its contrary (asinological torque?) is equally unlikely to advance the cause of an East-West entente, he gratuitously and presumptuously makes his own failure to penetrate to an even rudimentarily cogent interpretation of Leibniz's ethical thought into "The Failure of Leibniz' Philosophy." That the alleged "unresolved tension" therein, which better-equipped scholars have not found at all irresolvable, was the underlying reason for the collapse of the "search for accord" is the very thesis of the dissertation, which obviously concerns not *merely* Chinese thought or *merely* Leibniz and Western thought but precisely the connection between the two. Inevitably, then, this thesis attracted prime attention in all of the (remarkably few) reviews that have appeared, and it has escaped severe criticism only in *Choice*, whose reviewer seems unaware of the work, among others, of Needham, Rowbotham, Merkel, Dunne, Bernard, Zempliner,

Franke, Demiéville, Grimm, and, especially, Donald Lach.

The burgeoning literature on the relationship between Maoist and "classical" Confucianist China makes manifest the superficiality of regarding Chinese philosophy as a tensionless and static repository of wisdom unaffected by external circumstance. "Oh the patience of the ageless Chinese with their external values!" Jonathan Spence has sighed. "One grows a bit weary." But Dr. Mungello, though he wishes to assist our understanding of the modern world, is oblivious to annoying historical complexities while smugly pontificating about pioneering dual "areas of expertise." Comparative studies in East-West philosophy are actually less fashionable today than they were thirty years ago—by which time, incidentally, I had already long since completed an extended and intensive program in the navy's Oriental Language Institute, which first sparked my interest. His dissertation advisers should have warned Mungello that a leap from the seventeenth to the twentieth centuries that blithely ignores all that intervened, whether East or West, predictably produces problems owing to certain limitations on how long underfueled generalizations can remain airborne. It is interesting that Thomas Metzger—whose *Escape from Predicament: Neo-Confucianism and China's Evolving Political Culture* (1977), however controversial it may be, exemplifies the ferment that is induced by writers of subtle intellect who really *can* handle the intricacies of a multiple approach—has dwelled upon the "rationalistic bias" of Neo-Confucianism, which he has found fairly bristling with ambiguities. The inclination of Confucianists to consider polarities as complementary rather than antithetical does not change the fact that "tensions existed between the poles in question [and] that some men gravitated to . . . one pole rather the other," wrote Benjamin Schwartz in 1959; and in 1974 Julia Ching discerned in the diverse approaches of Chu Hsi and Lu Hsiang-shan a "parallel" to the Dominican-Franciscan dispute over the nature of Christian spirituality. (Need one add that a parallel is not an identity?)

More space is given in my review to the portions of the dissertation that I am charged with ignoring than is allotted them by the reviewer whose phrase ("core of the book") the author borrows! Readers of the *Review* were clearly informed as to what is central in the book—something to which attention can be drawn by less mechanical means than reciting chapter numbers. Dr. Mungello's statement on this point is therefore, to put it mildly, deliberately misleading. In fact, virtually every sentence of his communication, which evidently was hastily composed, contains some species of misrepresentation. The letter is not merely disagreeable; it lacks integrity, for which reason its motivation is rendered all the more

palpable by its author's rather too overt protestations.

WALTER J. BRUNHUMER
Western Michigan University

TO THE EDITOR:

I have met two or three intellectual giants in my thirty-odd years of academic work; one of them is Karl August Wittfogel. Perhaps David Felix, author of the review of G. L. Ulmen's biography of Wittfogel, *The Science of Society* (AHR, 84 [1979]: 1026-27), has never met any. Though Felix declares that "Wittfogel deserves . . . a sympathetic but critical study that would evaluate his achievements credibly," he seems chiefly concerned to attack the subject of the study. Credibility for his own review is difficult to claim, since he first declares that Wittfogel merely "enlarged upon a casual and ignorant aside of Marx about the 'Asiatic mode of production'" (thereby raising the question of whether Felix knows how much Marx wrote about Asia and Russia, using the concept in question) and then declares Wittfogel's views to be "overwhelmed . . . by the grand architectonics of Marx"—which presumably pertain to Europe alone, as Marx himself insisted his famed sequence of slavery, feudalism, and capitalism did. *Oriental Despotism*, writes Felix, was "almost immediately abandoned by serious scholarship"; a few lines later, Wittfogel's exploration "has encouraged fruitful scholarly inquiry." Wittfogel is said to have "provided no data on the location, size, management, and relation to government and society of the hydraulic works"; I refer Felix, as a start, to *Zweiter Abschnitt, Wirtschaft und Gesellschaft Chinas* (1931), followed by much more.

Wittfogel—whatever the extent of his debt to the "ignorant" Marx and also, as Felix says, to Weber—has raised more forcefully than anyone else, living or dead, the fundamental issue of whether the world outside of Europe and Japan has been dominated by social and political formations sharply different from those of the West and thereby has suggested cogent reasons why communism has succeeded and Western democracy has apparently failed in many countries of the area. (If such a view is "Eurocentrism," Felix had better explain why.) My sentence is an inadequate characterization of Wittfogel's work, but it is a much less inadequate one than is offered by Felix's unhappy review.

DONALD W. TREADGOLD
University of Washington

TO THE EDITOR:

Joe Gray Taylor's review of Peyton McCrary's *Abraham Lincoln and Reconstruction: The Louisiana Experiment* (AHR, 84 [1979]: 1161-62) states of McCrary's conclusion that Lincoln was aligning himself shortly before his death with congressional Radical Re-

publicans on Reconstruction policy, "So far as this reviewer is aware, this contradicts the conclusion of all other reputable scholars who have studied Lincoln's attitudes."

In my opinion, a close examination of Professor McCrary's documentation illuminates the existence of a pride of "reputable scholars" who agree with McCrary. I do, at least, and I do not feel lonely.

HAROLD M. HYMAN
Rice University

ERRATUM:

In Lewis A. Tambs's review of *The Bandit King: Lampião of Brazil* (*AHR*, 84 [1979]: 1512), the author of the book, Billy Jaynes Chandler, is incorrectly given an affiliation with Texas A&M University. In fact, Professor Chandler is a member of the department of history at Texas A&I University at Kingsville. The editors regret the error, which is the fault of the *AHR* and not Professor Tambs.

THE EDITORS

Recent Deaths

SOLOMON M. LUTNICK, associate professor of history at Queens College, died on September 13, 1979, after a long illness that he approached with the bravery and good humor so characteristic of him. Professor Lutnick was born in New York City on September 6, 1928, and spent all of his academic life there. He received his A.B., M.A., and Ph.D. degrees from Columbia University, being awarded the latter in 1960. At Columbia he studied with Professor Richard B. Morris and specialized in the American Revolution, a field in which he displayed unending interest. After teaching briefly at Hunter College, Professor Lutnick came to Queens College in 1956 and remained there until his death.

Dr. Lutnick is best remembered for his book, *The American Revolution and the British Press, 1775-1783*, published in 1967 by the University of Missouri Press. The book traced the split in English thought during the troubles with the American colonies, pointing up the enormous sympathy the British people held for their countrymen overseas. More recently, Professor Lutnick was interested in Edward Gibbon and before his death was collecting material for a biography of that great historian.

As a teacher, Professor Lutnick can only be described as superb. Approaching American history with an enthusiasm and depth seldom matched, he attracted large numbers of undergraduates to his classes. During his career he taught close to six thousand students, leaving his imprint on almost every one of them.

Dr. Lutnick will be remembered as a devoted scholar, a fine teacher, and a warm human being.

STANLEY P. HIRSHSON
*Queens College,
City University of New York*

JOSEPH HENDERSHOT PARK, professor emeritus of history and dean emeritus of the Graduate School of Arts and Science at New York University, died on October 6, 1979 at the age of 89 in the Morristown Memorial Hospital in New Jersey. Born in Port Murray, N.J., he attended Williams College for two

years and went to Columbia University for his A.B. in 1912, A.M. in 1913, and Ph.D. in 1920. He joined the faculty of New York University in 1915, saw overseas service in World War I, and returned to rise to full professor by 1928. He became chairman of the department of history at University College and executive secretary of the Graduate School of Arts and Science in 1940. In 1943 he was named dean and head of the graduate department of history.

Park's major publications were *The English Reform Bill of 1867* (1920) and *British Prime Ministers of the Nineteenth Century: Policies and Speeches* (1950). He contributed a number of articles on English history and on antiques and ceramics. Internationally known as an authority on Wedgwood chinaware, he also bred new types of flowers, notably peonies.

Few persons, if any, at New York University enjoyed the affection and respect that Park did. Students, faculty, clerical personnel, and administrators turned to him for guidance and sympathy. His wisdom, integrity, compassion, and cheerful outlook seemed as unfailing as his energy. Old-fashioned in dress and lifestyle, he was open to new ideas and methods and was thus an ideal official to preside over the great expansion of the department of history and the graduate school after 1945. Without being a crusader, he broke barriers of race, religion, and sex discrimination. The growth of the university's prestige owed much to his stewardship.

On a typical day Park commuted for an hour and a half from New Jersey by rail, ferry, and subway to Washington Square to attend to his graduate duties. At noon he would take the long subway trip to University Heights, where he spent perhaps his happiest hours teaching large classes in English history. Then he would return to the Square for administrative labors that grew heavier each year. He presided over every oral examination in his department, and he served on almost every committee in the university. No bureaucrat, he carried important information scribbled on slips of paper in his pockets. Yet he seemed to know everything and to forget nothing. Superlatively effective though he was as an

administrator, Park loved teaching best, and his students responded enthusiastically to his learning, wit, and incomparable personality.

After retiring in 1955 Joseph Park continued to be active as a gardener, farmer, and collector. Until the last he played the organ at a rural Methodist church near his native Port Murray. Nearly a quarter-century after his retirement his contributions to New York University are widely recalled with admiration and gratitude.

JOHN E. FAGG
New York University

FRANCE VINTON SCHOLES died in Albuquerque, New Mexico, on February 11, 1979, shortly after celebrating his eighty-second birthday on January 26. Born in Bradford, Illinois, he received the A.B. from Harvard University in 1919, the A.M. in 1922, and the Ph.D. in 1943. He taught history at Massachusetts Institute of Technology in 1919–20, at the University of New Mexico in 1925–26, and again in 1928–31, and at Colorado College in 1926–27. He had gone to the Southwest as a health seeker, and for him, like many who found a cure for tuberculosis there, New Mexico became his beloved adopted homeland.

His long career as one of the most distinguished research scholars of colonial New Spain and the Hispanic Southwest began in 1927–28 when Harvard awarded him the Woodbury Lowery Fellowship for travel and archival research in Mexico and Spain. In 1929–30 he was director for the Library of Congress of a project for photo-reproduction of documentary resources in Mexican archives. His extraordinary flair for fruitful archival investigation soon become evident and led to many spectacular finds then and later. By the time he joined the staff of the Division of Historical Research, Carnegie Institution of Washington, as head of the Post-Colombian History Section in 1931 he had discovered so much important new documentation for the history of New Mexico and the Spanish Southwest that it has not been fully exploited to this day. His own pioneering publications on seventeenth-century New Mexico remain valid and indispensable for the study of the period, and he continued to add to them throughout his life.

Carnegie, however, gave his research a new and equally profitable direction. Under Alfred Vincent Kidder, also a New Mexico expert, the Division of Historical Research undertook a major interdisciplinary program of study of Maya culture in all its aspects. During his fifteen years with the Carnegie Institution, Scholes's research again resulted in enormous quantities of new material and major finds to illuminate and revise traditional notions of the Maya past. His work and publications in this

field are invaluable, and his famous colleague, Maya archeologist J. Eric S. Thompson, paid fitting tribute when he numbered Scholes "among the giants of this century." The scope and depth of his interests were not limited to the Spanish Southwest and the Maya area. He also collected masses of material concerning colonial New Spain proper, some of which saw publication. By the time he died his collection of source material for the life and times of Hernán Cortés may well have been the most complete extant. Unfortunately, he was unable to finish the life of the conqueror with which he had hoped to crown his career as a historian.

In 1946 he returned to the University of New Mexico, where he served as dean of the graduate school, 1946–49, and first academic vice president, 1948–56. He was a fellow of the American Academy of Arts and Sciences and a corresponding member of the Academy of American Franciscan History, which honored him with the Serra Award of the Americas in 1956. After his retirement he was a Bollingen research fellow 1962–65, and visiting professor at Tulane University, 1968–69.

To France Scholes the life of the scholar was a joyous adventure. He was a dedicated perfectionist who loved and respected his profession and was rigid in his belief that complete honesty and objectivity are essential. Whatever the drudgery—and archival research is often a dreary, dusty task—he always found it fun and exciting. But Scholes was more than an outstanding investigator and writer, he was an inspiring teacher. One of his greatest achievements was to pass his integrity and enthusiasm on to his colleagues and students. In their gratitude for his guidance and assistance the same themes emerge over and over again: "A great historian, but also a friend of inexhaustible kindness and generosity, who made the practice of history an uninterrupted joy for himself and others." "I would hope that our contemporaries and successors will approach scholarship and teaching with the same breadth of vision, tolerance, and urbaneness, not to ignore simple generosity." "He represented pure and dedicated scholarship at its highest level." "He was truly an inspiration to me (as he was to so many others) and I resolved then and I resolve now to try harder to be the kind of professor and scholar he would have me be." "Papa Scholes" will not soon be forgotten by those who enjoyed the privilege of his company.

ELEANOR B. ADAMS
*University of New Mexico,
Albuquerque*

DAVID M. VIGNESS, professor of history at Texas Tech University, died July 16, 1979. Born in La Feria, Texas, on October 12, 1922, he served as a lieutenant

ant in the United States Navy in the Pacific Theater during World War II. He received his bachelor's (1943), master's (1948), and doctor's (1951) degrees from the University of Texas at Austin. From 1951 to 1955 he served as head of social sciences at Schreiner Institute in Texas. He then joined the faculty at Texas Tech University, where he chaired the department of history from 1961 to 1978, an important period of growth and diversification. He served as a Fulbright Lecturer at the University of Chile and the Catholic University of Santiago in 1957-1958. During the spring of 1979 he taught as a visiting professor at the University of New Mexico.

David's scholarly interests spanned the history of nineteenth-century Mexico, the Southwestern borderlands, and Texas. His published works included several articles and two books, *Documents of Texas History* (1963), edited with Ernest Wallace, and *The Revolutionary Decades* (1965) in the *Saga of Texas* series. Honors for his scholarship included election as a fellow of the Texas State Historical Association and receipt of the H. Bailey Carroll award for the best article in the *Southwestern Historical Quarterly*, 1971-72.

His participation in several professional organizations included the presidency of the Southwestern Council of Latin American Studies, membership on the executive council of the Southwestern Social Science Association, and the vice-chairmanship of the Texas Committee for the Humanities. Furthermore, he served on the editorial boards of several scholarly journals.

David also participated in the work of many university committees, in a number of community organizations, and as an active member of the Presbyterian Church. Perhaps he will be remembered most of all, however, as a kind, even-tempered man who as a chairman and as a professor always found time to counsel with faculty and students.

ALWYN BARR
Texas Tech University

SERGIIUS O. YAKOBSON, scholar, author, librarian, specialist in Russian history and Slavic languages, ranking Slavist in the Library of Congress, and foreign policy adviser to the Congress for over 30 years, died on November 13, 1979, at the George Washington University Hospital.

Born in Moscow in 1901, Yakobson received his early academic training at the Lazarev Institute for Oriental Languages in Moscow. Following graduation in 1918, Yakobson fled with his family to Germany in the wake of the Bolshevik Revolution. There, he earned a Dr. Phil. degree at the University of Berlin in 1926 majoring in history and with a minor in Slavic languages, literature, and philosophy. For the next seven years Yakobson was a re-

search associate in the Prussian Privy State Archives, but as a Jew he was forced to leave Germany in 1933 and take up residence in England. With the assistance of Arnold Toynbee, he was appointed Honorary Lecturer in Russian History at the University of London and Chief Librarian at the University's School of Slavonic and East European Studies. During his London years covering the period 1933-1940, Yakobson contributed to publications of the Royal Institute of International Affairs and to British scholarly journals; he lectured at Oxford, Cambridge, Nottingham, and Cardiff; and along with his many scholarly activities he enjoyed a professional association with such notable British scholars as Sir Bernard Pares and Toynbee.

In 1940, Yakobson came to the United States and a year later was appointed a Rockefeller Fellow in Russian History at the Library of Congress. Thus began an association with the Library that was to last for 38 years. During most of that time Yakobson served simultaneously in the Library's Congressional Research Service as Senior Specialist in Russian Affairs and in the Slavic and Central European Division as its Chief. Upon retirement in 1971, he was appointed the Library's Honorary Consultant in Slavic Studies, a post he continued to hold until his death.

As a scholar in Slavic studies recognized internationally, Yakobson combined a professional career of service in the United States government and to the scholarly community. During World War II he was second in command of a confidential White House project on migration and settlement. As Chief of the Slavic Division, he was the primary force in developing and expanding the Library's Slavic collection. As Senior Specialist in Russian Affairs, he contributed enormously to the proceedings of the Congress by his written work and oral consultations with members and staff. And in addition to his official duties he served the scholarly community directly: by lecturing at some of the nation's leading universities; by presenting research papers at national and international meetings on subjects of scholarly concern; by actively serving in such eminent organizations as the Joint Committee on Slavic Studies of the American Council of Learned Societies, the American Historical Association, and the American Association for the Advancement of Slavic Studies—(In 1972, the AAASS presented Yakobson its National Award in recognition for his contribution to Slavic studies); and finally publishing the results of his extensive research. Listed among Yakobson's many publications both by the government and in the private press is his chapter on "Russia and Africa" included in *Russian Foreign Policy: Essays in Historical Perspective* published by the Yale University Press in 1962. This essay, leavened by years of research and reflections on Af-

rica, has now been recognized as a pioneering effort in tracing Russia's historical interest in Africa.

In his long professional career, Yakobson was thus a working scholar motivated by a high sense of service to his government and to the scholarly community, applying his skills with a dignity, dedication, and integrity that won for him the respect and gratitude of those he served. In a life that had been shaped by training and experience in three great

cultural traditions of the world—the Slavic, the Germanic, and the Anglo-Saxon—he was unique as a “scholar-in-government” for he could and did draw upon the best of all three traditions and thus made a distinctive contribution to the vital center of American life.

JOSEPH G. WHELAN
*Congressional Research Service,
Library of Congress*

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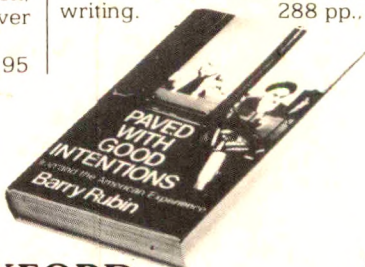
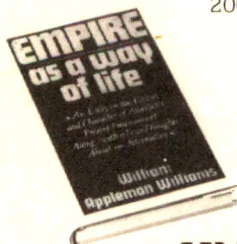
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PETER KOLCHIN, an associate professor of history at the University of New Mexico, is working on a comparative study of American slavery and Russian serfdom. He is the author of *First Freedom: The Responses of Alabama's Blacks to Emancipation and Reconstruction* (1972), "The Process of Confrontation: Patterns of Bondage in Nineteenth-Century Russia and the United States," *Journal of Social History*, 11 (1977-78), and other articles.

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The Case for Comparing Histories

RAYMOND GREW

HISTORICAL COMPARISON HAS BEEN WIDELY PRAISED by many of the best minds of the historical profession. Marc Bloch, whose prestige and influence have increased since his death, called for historians to work comparatively in a famous article published over fifty years ago,¹ and Lord Acton argued for historical comparison fifty years before that.² Indeed, the pedigree of that approach could be extended back through William Robertson and Adam Smith to Montesquieu, Vico, and Machiavelli and on to Polybius and Herodotus. Auguste Comte, that often disowned father of social science, considered comparison to be the highest form of observation and, when undertaken through "the historical method," believed it "the only basis on which the system of political logic can rest" as well as the method that would provide "philosophic character to sociology in a logical as well as a scientific sense."³ And Marxism, perhaps even more today than in the past, is a powerful stimulus to historical comparison.

Admittedly, however, for many professional historians comparative study evokes the ambivalence of a good bourgeois toward the best wines: to appreciate them is a sign of good taste, but indulgence seems a little loose and wasteful. In part, such hesitance reflects some of the admirable if modest qualities most widely respected and fully shared within the historical profession—caution, accuracy, unpretentiousness, and respect for the integrity of documents and for the particular. In part, it reflects doubt not so much about comparison as a mode of analysis as about what it is that historians should compare. For most historians, the models that come readily to mind are troubling.

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¹ Bloch, "Pour une histoire comparée des sociétés européennes," paper delivered at the Sixth International Congress of Historical Sciences, held in Oslo, August 1928, printed in the *Revue de synthèse historique*, 46 (1928): 15–50, and reprinted in Bloch, *Mélanges historiques*, 1 (Paris, 1963): 16–40. For an English version, see Bloch, "Toward a Comparative History of European Societies," trans. Jelle C. Riemersma, in Frederic C. Lane and Jelle C. Riemersma, eds., *Enterprise and Secular Change: Readings in Economic History* (Homewood, Ill., 1953), 494–521.

² "The process of Civilization depends on transcending Nationality. Everything is tried by more courts, before a larger audience. Comparative methods are applied. Influences which are accidental yield to those which are rational." Note of Lord Acton, University of Michigan, Add. MSS 4908. Sylvia Thrupp used Acton's statement to begin the first issue of *Comparative Studies in Society and History: An International Journal* in October 1958.

³ Comte, *Cours de philosophie positive*, in *Auguste Comte and Positivism: The Essential Writings*, ed. Gertrud Lenzer (New York, 1975), 247, 249.

"COMPARATIVE HISTORY"—a term so compromised that, throughout the program for the Ninety-Third Annual Meeting of the American Historical Association in December 1978, it was properly wrapped in quotation marks—suggests first of all the comparison of civilizations, comparison on the grandest scale in the manner of Spengler, Sorokin, and Toynbee. These great efforts, themselves part of Europe's encounter with the rest of the globe in the era of two world wars, remain stimulating and provocative for many. But their search for the morphology of history is not intimately related to what most practicing historians really do, very few of whom would agree that "the most important task of the scientific study of history will be the comparative study of the development of whole civilizations." In his intelligent book Philip Bagby went on to declare, "The actual technique of describing cultural ideas and values is a fairly simple one. It consists of listing all or, at least, a number of the forms of cultural behaviour, the speech, and actions in which they are expressed, and the artifacts which result from such behaviour."⁴ And any reader knows immediately that this student of A. L. Kroeber is not a historian.

When civilizations are compared primarily in terms of certain central topics, the historian is on more familiar ground. Comparison, then, as in the *grands thèmes* of international historical congresses, merely treats familiar concerns on a broader scale. Comparative religion or the relations of state and society are subjects of great classics of historical literature, especially in German, and, even when segregated as philosophy of history, such sweeping interests have animated whole schools of study. If as historians we like to call these approaches philosophy, it is because they seem, even after more than a century of remarkable works that deny the distinction, to rest less on what historians call research than on what the very learned already know (in short, on the state of our culture), on the wise discernment of patterns, and on the generation of fresh hypotheses.

A third well-recognized mode of comparison treats not whole civilizations or major aspects of them but historical processes. These can range enormously in scope, from comparative study of the "alternative modes of production"⁵ to the pace and degree of industrialization in a single industry; from the comparison of revolutions,⁶ a subject of undying popularity, to the comparison of processes still more closely defined, such as the spread of modern educational systems.⁷ This sort of historical comparison is especially congenial to economics, sociology, and some schools of anthropology; and, as with the now much-criticized concept of "modernization," such comparisons lead to exchanges of methods and data, in-

⁴ Bagby, *Culture and History: Prolegomena to the Comparative Study of Civilizations* (Berkeley and Los Angeles, 1963), 190, 192.

⁵ Immanuel M. Wallerstein has called for this sort of comparative study; see his "Modernization: Requiescat in Pace," in L. Coser and O. Larson, eds., *The Uses of Controversy in Sociology* (New York, 1976), as quoted in Barbara Hockey Kaplan, ed., *Introduction to Social Change in the Capitalist World Economy* (Beverly Hills, 1978), 10-11.

⁶ For some recent approaches, see the articles by Perez Zagorin, Theda Skocpol, and Elbaki Hermassi in *Comparative Studies in Society and History*, 18 (1976).

⁷ See Michalina Vaughan and Margaret Scott Archer, *Social Conflict and Educational Change in England and France, 1789-1848* (Cambridge, 1971).

terest in common problems, and a shared vocabulary that have lasting benefits.⁸ Many of the most often-cited works of recent historical comparison belong in this category, although, significantly enough, most have not been written by scholars professionally trained as historians.⁹

Yet the comparison of historical processes also evokes resistance, even suspicion, among many historians. Much of that suspicion may result from methodological naiveté, intuitive nominalism, or sheer prejudice; but weightier concerns informed by an awareness of the common nineteenth-century roots of all of the social sciences also seem to be involved. Nothing is more awkwardly old-fashioned than optimism no longer felt—in this case about the nature of social knowledge, the validity of universal laws, and the meaning of history. Furthermore, our professional distaste for easy determinism deserves serious consideration on methodological grounds. Historical determinism has been one of the great spurs to comparison among Hegelians and materialists and from Buckle, Tylor, Spencer, Maine, and Morgan to the present day,¹⁰ for determinism implies the existence of destinies and definable laws that comparison should be able to uncover. But determinism can also place an early limit on the power of comparison to uncover surprises or display variety and, thereby, to redefine traditional problems.

The nineteenth-century evolutionists tended in practice to compare stages of evolution or other analytic abstractions, and this dependence on definition is further reason for historians' discomfort. Sensitive to the differences that chronology makes, historians are bound to wonder whether a process remains the same when it lasts for decades in one instance and for centuries in another or when it occurs in different eras altogether. Are two sets of events comparable because both are called revolutions or are the crises, revolutions, and cycles assembled for analysis mere homonyms (a kind of comparison that Marc Bloch warned against), a humorless misunderstanding of a historical pun? The answer, of course, is that the validity of any comparison rests on careful definitions against which the elements compared must be systematically tested. This obvious methodological solution explains why so much of the effort in works comparing historical process is taxonomic, why so much of the actual comparison is of categories and classifications.

These problems are reduced if institutions are the unit of comparison; and much of the best work, especially in premodern history, is of this sort. But institutions that go by common names of church or party or bank may, in fact, perform quite different functions in different societies or at different times. Structural-functionalism in anthropology and sociology has helped clarify this

⁸ For some assessments of the continuing debate on modernization and its implications for comparison, see the April 1978 issue of *Comparative Studies in Society and History*, which was devoted to that question, and note the earlier articles mentioned in the Editorial Foreword, *ibid.*

⁹ Among others, see the works of S. N. Eisenstadt, Michel Foucault, Samuel P. Huntington, Barrington Moore, Charles Tilly, and Immanuel Wallerstein and of the Committee on Comparative Politics of the Social Science Research Council.

¹⁰ See J. W. Burrow, *Evolution and Society: A Study of Victorian Social Thought* (Cambridge, 1966); and Maurice Mandelbaum, *History, Man, & Reason: A Study in Nineteenth-Century Thought* (Baltimore, 1971).

problem and has been effectively used especially by historians of political parties and economic development.¹¹ Ultimately, however, social functions, like most analytic abstractions, raise for historians the taxonomic discomfort of categories that seem to lie outside the field of study (the workday historian is likely to complain of "imposing" something on the past). The emphasis on structural analysis now more in vogue lessens the abstractness, for structures are more easily tied to specific behavior. Studies of the crowd, for example, were received into the historical canon primarily as ideology¹² until Marxian concern for class differences led historians to seek a firm relationship among particular crowds, their behavior, and specific conditions. Then the comparison of crowds became especially fruitful. Yet the search for structures is vulnerable to a subtle determinism that invites merely secondary interest in *mentalités* or cultural values, and historians are wisely sensitive to modes of analysis that too readily limit or exclude. If the comparison of processes calls attention to the problems of definition, so the comparison of structures calls attention to the dangers of abstractness.

Methodological sophistication in matters of morphology and taxonomy or typology can be of great value in historical analysis, and I assume no historian so benighted as to denounce the effort in advance would have chosen to read this far. But such analytic tools require—it is their very purpose—fitting bits of history into categories already established, and conceptually that is uncongenial for most historians, even though in practice we do this all the time. It may not be more "artificial" to define a topic in terms of the available historical sources, traditional political boundaries, or conventional chronology; but historians, like everyone else, notice more readily that to which they are unaccustomed. To start with fixed categories is to admit to approaching history from the outside. To the historically sensitive, however great their gift for theory, this is not a comfortable way to work. "Events strikingly similar but occurring in a different historical milieu," wrote Karl Marx in 1877, "lead to completely different results." And, he added, "By studying each of these evolutions separately and then comparing them, it is easier to find the key to the understanding of this phenomenon; but it is never possible to arrive at this understanding by using the *passe-partout* of some universal historical-philosophical theory whose great virtue is to stand above history."¹³ The common impression that historical comparison requires just such an approach—that it consists in comparing groups or events, institutions or ideas that in the abstract have been labeled as comparable—may in large part account for the scarcity of systematic comparison in historical writing. Significantly, the comparisons most admired and accepted tend to be of the sort that emerge in the works of Jacob Burckhardt, R. R. Pal-

¹¹ S. N. Eisenstadt has seen both Claude Lévi-Strauss and Talcott Parsons, however, as providing bases for the comparison of types; Eisenstadt, *Essays on Comparative Institutions* (New York, 1965), 47. Parsons has conveyed a view of history remarkably similar to the grand histories of civilization; see his *Societies: Evolutionary and Comparative Perspectives* (Englewood Cliffs, N.J., 1966) and *The System of Modern Societies* (Englewood Cliffs, N.J., 1971).

¹² Or they were received as works that could safely be ignored, as in the case of the striking *aperçus* of Elias Canetti; see his *Crowds and Power*, trans. Carol Stewart (New York, 1972).

¹³ Marx, Letter published in a Russian newspaper, as quoted in Edward Hallett Carr, *What Is History?* (New York, 1961), 82.

mer, or Fernand Braudel, studies whose scope was not determined by categories of comparison but that used a framework already familiar and acceptable and also large enough to include space for comparative analysis.

The willingness to compare societies rests, most attractively, on the assumption of a certain universality in the human condition, on the belief that, just as individuals and societies can learn from each other, we can learn by comparing human behavior in similar or different contexts. One of the more admirable of our own cultural traditions is thus reinforced in our times by easier communication and the methods of social science. And I believe one sees an increasing tendency among Western scholars to acknowledge, at least in their footnotes, the relevance of the African or Indian or Chinese experience to the understanding of our own behavior. Heirs of the nineteenth century, when national differences seemed fundamental, and nowadays surrounded by an intense ethnic consciousness, we have a special need for cross-national comparisons. Significantly, they remain more common in medieval than in modern history, in the study of Europe than in that of the United States. In the writing of American history it is surprising, as John Higham has commented, not that comparison "should at last have begun to flourish, but that it should have been so long delayed."¹⁴ So great, then, is the need that "comparative history" is often taken to mean comparison between nations,¹⁵ which has the paradoxical effect of reinforcing the tendency to consider nations the major unit of analysis. That, in turn, has discouraged comparison by those who prefer to work in primary sources; for it makes comparison seem to require an equally intimate knowledge of at least two societies, two languages, two distinct traditions of record-keeping and interpretation. Few historians are willing to abandon the benefits of a specialization that has enormously expanded historical knowledge and sophistication.¹⁶ It is therefore important to stress that many of the benefits of comparison can result from studies in which all of the cases fall within a single nation but exemplify differences in time or space or social group or even in which the cases compared have not been investigated at equal depth. Comparative analysis, like the architect's compass, is no less useful a tool of design when the legs on which it stands are of unequal length.

¹⁴ Higham, *Writing American History: Essays on Modern Scholarship* (Bloomington, Ind., 1970), 165. He has suggested that the vigorous effort in American studies to be interdisciplinary may have got in the way of intercultural analysis. Like so many others, historians of the United States have been transfixed by the national state. Thus, comparative analysis is central to the historiography of the colonial period but uncommon thereafter except for certain classic questions like slavery (which we would like to see as foreign anyway) and such challenges to the state as regionalism and civil war.

¹⁵ For a discussion of the development and difficulty of such ambitions, see Stein Rokkan, "Comparative Cross-National Research: The Context of Current Efforts," in Richard L. Merritt and Stein Rokkan, eds., *Comparing Nations: The Use of Quantitative Data in Cross-National Research* (New Haven, 1966), 3-25. Terence Hopkins and Immanuel Wallerstein have distinguished three kinds of "plurinational" study, but David S. Landes and Charles Tilly have seen comparison more broadly and predicted a central place for it within the historical discipline; see Hopkins and Wallerstein, "The Comparative Study of National Societies," reprinted in Amitai Etzioni and Frederic L. Dubow, eds., *Comparative Perspectives: Theories and Methods* (Boston, 1970), 183-204; and Landes and Tilly, eds., *History as Social Science* (Englewood Cliffs, N.J., 1971).

¹⁶ The benefits of using primary materials are often overstated. In practice, greater historical understanding is threatened more by mindlessness than by error, for inaccuracies are easier to identify and correct than are misleading assumptions. The great advantage of working from primary sources is less any increased assurance that the facts are right than the increased opportunity for independent, original analysis that is at the same time sensitive to historical context.

BECAUSE, AMONG PROFESSIONAL HISTORIANS, COMPARISON is more widely admired than consciously practiced, it may be useful to set forth a fairly simple and unpretentious case for thinking about history comparatively, one that need not arouse inherited doubts and suspicions. Some of the simpler advantages of comparison are more apparent in the other social sciences. For those dealing with contemporary issues in another society, the dangers of ethnocentrism, mis-translation of culturally imbedded meanings, and false assumptions of comparability are immediate. Historians—usually analyzing a world different from the one in which they live, experienced in the culture they study, and guided by scholarly (and cultural) lore—often avoid the most blatant forms of these errors without once shuddering at the peril they missed. And when, a generation later, the historian in turn is recognized to have reflected his own times, history—the most cumulative of the social sciences in knowledge if not in method—seems hardly to have been harmed. Yet comparison, for the historian too, can help prevent the misinterpretation of other cultures.¹⁷

That comparison is central to any scientific method has become a truism;¹⁸ and logicians like to point out that to declare anything unique, supposedly a penchant of historians, is by implication to compare it to a class of things to which it purportedly belongs. All historical narrative rests on some assumptions as to what constitutes normal behavior and what others have done in similar circumstances. In addition, the most consistently fruitful source of new insight into the past is probably the historian's own experience of the present. (How revealing it is that we speak of a scholar's gaining or providing a fresh perspective.) Striving for objectivity in our finished analyses, we tend to admit our grounding in the present a bit shamefacedly; but it can, like hindsight or the fact that the historian is always something of an outsider, be a welcome asset, an aid to good comparison. Similarly, the historian's preoccupation with change is a commitment to comparison. Thus, the need to compare and the habit of doing it are a large part of the practice that makes history a discipline, that enables an "experienced" historian to judge a piece of scholarship in a distant field of history. This is done by assessing the significance of the problem posed, the importance of the argument made, and the quality of the evidence presented largely by a series of comparisons both with other scholarly works and with other sets of historical problems. In short, history can be substituted for sociology in Émile Durkheim's dictum, "Comparative sociology is not a particular

¹⁷ Maurice Mandelbaum has made this point, significantly adding that this "need not involve an abandonment of the historian's ideographic approach"; Mandelbaum, *The Anatomy of Historical Knowledge* (Baltimore, 1977), 14. And Barrington Moore, Jr., has noted that comparisons "can lead to asking very useful and sometimes new questions . . . , can serve as a rough check on accepted historical explanations," and "may lead to new historical generalizations"; Moore, *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966), xiii.

¹⁸ In his admirable discussion of Claude Lévi-Strauss and Talcott Parsons, Guy E. Swanson began by noting that all research entails comparison and pungently added, "Thinking without comparison is unthinkable"; Swanson, "Frameworks for Comparative Research: Structural Anthropology and the Theory of Action," in Ivan Vallier, ed., *Comparative Methods in Sociology* (Berkeley and Los Angeles, 1971), 141, 145.

branch of sociology; it is sociology itself, in so far as it ceases to be purely descriptive and aspires to account for the facts."¹⁹

In the broadest sense, then, comparison is unavoidable. The question is not so much whether historians should make comparisons but whether the study of history benefits when those comparisons are made consciously and sometimes even systematically. Deliberately used, comparison can aid historians at four stages of their work: (1) in asking questions, (2) in identifying historical problems, (3) in designing the appropriate research, and (4) in reaching and testing significant conclusions.

In even a single piece of research, the historian may use many quite different comparisons in quite different ways. First, the initial stage of truly original historical research is the focused curiosity that comes with the asking of fresh questions. And comparison is probably their most consistent source, suggesting by examples that what looks like change may be continuity or that things seemingly unrelated may be connected. To look at other cases is to see other outcomes. By considering them, the historian wins some freedom from the tyranny of what happened and develops that awareness of alternatives—of a missing revolution, of banks that were not formed, of parliaments that did not meet, of populations that failed to increase—that underlies some of the most provocative of historical questions. We are easily blinded by the obvious. As Bloch noted of German and French ways of dividing property in the Middle Ages, either pattern studied by itself would seem natural; only comparison establishes that there is something to be explained.²⁰ When the historiography of one time and place develops a rich set of analytic categories, their very effectiveness tends to foreclose "inapplicable" questions—until, that is, the historical imagination is stimulated anew by the surprises that follow from applying to familiar issues some categories of questions evolved in another context. No less a thinker than Charles Darwin, one of the most effective users of comparison, expressed admiration for the brilliance of Henry Thomas Buckle (although soon adding, "I doubt that his generalizations are worth anything"),²¹ and we should consider the nature of that brilliance. Confident in historical laws, Buckle was eager to seek evidence everywhere and so willing to rub one fact against another that he expanded the range of historical curiosity to touch in a few pages on climate and crime, ballads, philology, the price of wheat, the influence of the clergy, the causes of dueling, the impact of the theater, and the importance of colonies. When comparison has uncovered interrelationships usually overlooked, its effect

¹⁹ Durkheim, *Rules of Sociological Method*, ed. E. F. Catlin, trans. Sarah A. Solovay and John H. Mueller (Glencoe, Ill., 1964), 139. Daniel P. Warick and Samuel Osheron have discussed the uses of comparison and noted the debate in sociology as to whether comparative sociology is a separate field. They decided it is not, citing Oscar Lewis's comment, "Comparison is a generic aspect of human thought rather than a special method." Warick and Osheron, "Comparative Analysis in the Social Sciences," in Daniel P. Warick and Samuel Osheron, eds., *Comparative Research Methods* (Englewood Cliffs, N.J., 1973), 6–11.

²⁰ Bloch, "Pour une histoire comparée," 39.

²¹ Buckle's biographer in turn attributed Darwin's skepticism to "a certain genial density"; John MacKinnon Robinson, *Buckle and His Critics* (London, 1895), 25; and Buckle, *Miscellaneous and Posthumous Works*, 1 (London, 1872).

is also to increase awareness of the differences between cases—a result often more fruitful than the initial impression of similarities. In all of these ways, comparison can lead to good historical questions. Now it may not matter how these questions are arrived at or what prejudices or misconceptions have stimulated this new awareness; but most of us need more than a falling apple. In adopting historical comparison we can make use of, for our purposes, the best thought in other fields.²²

The purpose of posing questions is to identify historical problems that merit further research, a second stage of the historian's work. Once certain clusters of questions are identified as interrelated in ways that constitute a problem or problems for which solutions should be sought, comparison can be used with somewhat greater deliberation to determine which historical elements from an infinite variety ought to be included. Here, too, comparison is a means to breaking out of the trenches dug by received conceptions, but it should bring to bear the experience of other times and places and the work of other scholars in defining the unit of study—justifying, in effect, these particular cuts across history's seamless web. Conceived comparatively, the historical problem is then more likely to determine the scope of the research—investigation that may require chronological, geographical, and social dimensions that are quite unconventional. (Acton and Bloch particularly stressed the need to use comparison to overcome the national particularism built into European historiography.) It is important to understand that the first essential of sound comparison is well-defined categories of analysis and that these, simply by being too broad or too narrow for the problem at hand, can make it difficult to uncover anything new through comparison.

In a third stage, the establishment of a research design, comparison can be used to suggest by example the applicability of evidence and methods that might otherwise be overlooked, although they have worked well elsewhere, and to search for critical variables. As most of its advocates have insisted, systematic historical comparison should often stimulate new attention to the details of individual cases and to local studies.²³ Truly original historical analysis usually strives to build its arguments from the inside out, and we should not allow the daring use of sweeping comparisons (more often ventured in our sister social sciences) to create a misimpression that comparison only moves from the abstract to the particular. To ask historians to work comparatively is not to uproot them from their sources (nor to embrace the desiccation that would be likely to fol-

²² Hugh Stretton has argued for comparison as a means of extending the investigator's experience and stimulating the imagination; but Arend Lijphart has criticized this position in favor of the conventional view that comparison serves primarily to test hypotheses; see Stretton, *The Political Sciences: General Principles of Selection in Social Science and History* (London, 1969), 69, 245–47; and Lijphart, "The Comparable Case Strategy in Comparative Research," *Comparative Political Studies*, 8 (July 1975): 159–60.

²³ See Bloch, of course, "Pour une histoire comparée," 40. But Lijphart has cited such sociologists as Neil J. Smelser and Juan Linz to the same effect; "Comparative Research," 67–68. And the list could easily be extended. Arthur L. Stinchcombe has provided an extended argument rejecting the idea of some tension between facts and generalization, declaring that "it is the details that theories in history have to grasp if they are to be any good" and that "social theory without attention to detail is wind"; Stinchcombe, *Theoretical Methods in Social History* (New York, 1978), 115–16, 124, 21–22.

low). Whereas concern for detailed accuracy can bury significance under trivia, comparison can make the apparently trivial significant—an asset few of us can afford to ignore. As the problem is defined and a design for research established, strategies of comparison become more formal. Proper concern for the complexity of a particular context may invite comparison of its major elements with many different cases; the task is then to see wholes and compare parts²⁴ (for all of the majesty of his effort, Arnold Toynbee often exemplified the dangers of doing the reverse).

When research leads to the recognition of general patterns, formal hypotheses, or full-fledged theories, these can again be tested by comparison—this time in ways as methodologically systematic as possible. This fourth and most formal stage of comparison is the one most written about in works on method. But comparison can be claimed to be the historian's (or the social scientist's) form of experiment in the multiple tasks of preliminary soundings and research design as well as verification. These multiple uses of comparison form a continuum, as do the differences in scale from one instance seen comparatively to many and from comparisons within one society to those that move across countries, cultures, or time. The value of any historical comparison rests not on the importance of the cases compared or on their number or scope but on the quality of the discernment, of the research, and of the general statements that result.

This is not to say that all valuable historical work must be consciously comparative. The absolutely essential effort to determine what happened and in what sequence or to assess the validity of suspect evidence need not involve historical comparison, except in the broad sense that judgment always has some comparative elements. Nor is the scholar working on a single case so likely to use comparison when the problems posed have been fully set by historiographical tradition. Invaluable contributions have, nevertheless, been made in these ways.

The case for comparison should conclude, however, by noting that the various levels and kinds of conscious historical comparison contribute to the endless task of building a science of society, and they do so most importantly in a relatively straightforward way. The slow, concrete construction of a common vocabulary enables students of different societies working from different disciplines to speak to each other.²⁵ Sometimes we can even work from a shared set of questions to create the kind of dialogue that is the essence of science. We must not claim too much, for the projects devoted to interdisciplinary research during the last thirty years give reason for modesty as well as hope. Neither need we reject

²⁴ Robert F. Berkhofer, Jr., has argued that in a final sense a reconciliation of the two approaches so far appears "analytically unattainable," but his standards are very high; in any case, the *effort* seems central to much of the best historical writing. See Berkhofer, "Clio and the Culture Concept: Some Impressions of a Changing Relationship in American Historiography," in Louis Schneider and Charles M. Bonsean, eds., *Culture in the Social Sciences* (Cambridge, 1973), 99–100. Guy Swanson has cited Lévi-Strauss to the effect that the "comparative method consists precisely in integrating a particular phenomenon into a larger whole" and that research should be "so conducted that theory is at every point in touch with observations"; "Comparative Research," 146.

²⁵ For a well-developed case for this sort of cross-disciplinary work, facilitated by the "federated" nature of American academic departments, see Bernard S. Cohn, "History and Political Science," in Otto von Mering and Leonard Kasdan, eds., *Anthropology and the Behavioral and Health Sciences* (Pittsburgh, 1970), 89–111.

the vision of working to establish a common questionnaire,²⁶ of sharing the excitement of the hunt as well as the published reports of the kill.

IN LIGHT OF SUCH BROAD YET GENTLE CLAIMS for the benefits of comparison, it is possible to say something about when historical comparison works best. For instance, it is my impression—to select two fields safely distant—that the results of comparison have been more striking and consistent in the history of art than in the history of literature. Literature, like history itself, is so easily understood as encompassing most of life that problems of focus and selectivity can be overwhelming, reinforcing a tendency we all share to let the traditions of academic monographs rather than independent analysis shape our topics. Yet there are many works of comparative literature every historian might envy. If most of us cannot assume the brilliance or adopt the erudition of Renato Poggioli or Northrop Frye,²⁷ we can learn from the skill with which they use the concept of established literary forms (much as historians use the idea of institutions) and mastery of rhetoric (much as sociologists use methodology). In only one respect do they enjoy an inherent advantage denied most historians: they may assume that their readers are already familiar with some of their primary sources. At more ordinary levels of scholarship, however, comparison in art history seems more often to have led to new discoveries. Perhaps because painting and sculpture, which are more removed from common discourse, are more abstractly understood, because the artistic medium itself is more expressly limiting yet universal, and because its functions and symbolic structure are more formal, the elements of comparison can be more readily defined with regard to art than to literature.

Or, closer to home, the comparisons of national character, once so fashionable, have been largely abandoned because too many different things were compared at once, leaving rules of evidence so loose that the relationship of doubtful assumption and descriptive example was circular. Demography, on the other hand, enjoys its current triumph in large part because universal elements in human biology and numerous cases permit the supreme comparability and subtle analysis of quantification, with which it is possible to distinguish significant social differences. Nevertheless, historical studies of the family have proved effective when focused on particular, well-defined problems, less so when too broadly aimed.

Similarly, the magnificent sweep of Jungian categories is only on occasion and with difficulty tied to the kind of evidence with which historians deal. Psychohistorical studies of individuals, however, are likely to leap from the abso-

²⁶ The term is once again Marc Bloch's, who meant by it the set of questions a Continental judge puts to a witness rather than a form to be filled out or a tool of survey research.

²⁷ See, for example, Frye, *Anatomy of Criticism* (New York, 1967); and Poggioli, *The Theory of the Avant-Garde*, trans. Gerald Fitzgerald (Cambridge, 1968). Fitzgerald commented in his "Translator's Apology" that "it is this assembly of points into patterns that makes his [Poggioli's] book such a stimulant and a pleasure"; *ibid.*, ix. Studies of political cartoons have offered a striking use of comparison in *Comparative Studies in Society and History*.

lutely particular to Freudian categories that group many cases without necessarily inviting historical comparison. When, however, psychological analysis points to social roles in specific contexts (families, generations, children, or true believers), then very fruitful historical comparisons can follow.

On the whole, historical comparison seems most effective at a kind of middle range.²⁸ The term is imprecise, but obviously comparison is most enlightening when the choice of what to compare is made in terms of general and significant problems, the elements compared are clearly distinguished, and attention is paid to the intricate relationships between the elements compared and the particular societies in which they are located. These criteria are most likely to be met when there are models or theories that can be concretely applied, when the evidence is extensive and rooted in its historical context (which often means that it has been generated with just these problems in view), and when the cases are delimited. Then one seeks explanations and generalizations but not universal laws. The search is for patterns of behavior and circumscribed hypotheses, and it is as likely to result in the recognition of unexpected connections between aspects of society previously thought to be unrelated as in general theory. In practice, the importance of such findings will more often be measured by the stimulus they give to subsequent research than by formal validation.

As a faintly empirical appraisal of the sort of work currently being done in this middle range, I have attempted to analyze the last five hundred manuscripts submitted to the quarterly journal *Comparative Studies in Society and History*. Obviously, any single publication develops its own traditions and is unlikely to be very precisely representative; furthermore, although these five hundred authors come from some thirty countries, nearly two-thirds of them either are citizens of the United States or are teaching at American universities. Among these manuscripts, there is surprisingly little difference between the kinds of topics chosen by historians and those favored by sociologists, political scientists, and anthropologists. Nor is there, incidentally, any general difference between the topics of that small fraction of manuscripts selected for publication and all of the rest: the printed articles tend simply to provide more carefully developed arguments (or models) and to rest on more extensive and suggestive research. Paradoxically, the ones rejected are often among the most deliberately comparative, turned down because readers felt a rather mechanical application of someone else's categories had added little either to general theory or to the understanding of particular cases.

For what it is worth, I think I see in these five hundred manuscripts support for seven general statements about the current practice of historical comparison.

1. Matters colonial constitute much the favorite subject matter, a sobering reminder of the extent to which all of the social sciences are influenced by contemporary politics and of how much the impe-

²⁸ This echoes, of course, Robert K. Merton, "Theories of the Middle Range," in his *Social Theory and Social Structure* (New York, 1952). Richard H. Brown has suggested, however, that Merton merely meant testable theories; Brown, *A Poetic for Sociology* (Cambridge, 1977), 13.

tus to comparative study was stimulated by the West's imperial encounters with other societies (although Greece and Rome, ancient Egypt, and China have exercised a similar attraction). And a more intrinsic reason probably underlies this emphasis: the colonial experience offers a degree of analytic control not usually available to social scientists; new influences and pressures can be identified and their assimilation, distortion, or rejection can be traced. One can, perhaps with misleading ease, identify the source of social change. A similar set of advantages, I think, makes the study of ethnic or religious minorities and of migration popular; the subject may appeal to academic sensibilities, but the cases themselves, while speaking to the most universal social issues, have a closed quality that allows a distinction between internally generated and externally stimulated change and that makes more visible the confluence of old customs and ideologies with new social and political needs (the complex experience of Israel may have stimulated more good comparative study than that of any other modern nation). In historical comparison, cases that in themselves allow a kind of experimental control, that segregate certain variables from the larger social flow, offer particular advantages.

2. Articles on currently fashionable topics (such as women's history, birth control, crime, terrorism, or social services) are so frequent as to suggest something more than the latest style; comparative analysis of historical cases is especially valuable in building new sets of questions into a field of study and in connecting these questions to the established literature. Social history in general has been associated with comparative study for many reasons. It has emphasized the material circumstances that condition people's lives, and these are relatively easy to compare. It has sought to analyze social structures and, in doing so, has effectively used categories, concepts, and theories developed first by economists, sociologists, and anthropologists, making comparison at least implicit at every stage of research. Social history has welcomed quantitative methods, which facilitate comparison (and also its misuse); and relating specific phenomena to a larger society is an intrinsically comparative effort. Social historians, attentive to classes that have not left a rich, written record of their culture, are readily pushed to comparison when little encumbered by knowledge of the complexity of the lives led by anonymous people who have been seen from the first as an abstract type. Most simply, social historians have welcomed comparison as they saw themselves building a new subdiscipline. This may well mean that attention to comparison within social history will decline as it becomes an established field, an increasing number of whose scholars will choose to pursue in a single context questions already well developed.

3. Whenever a body of theory is already well established, it becomes easier to discern the significant elements of a particular case; empirical studies of slavery, millenarianism, land tenure, or revolution, for example, thereby gain strength. The use of theory has also become more sophisticated, and the suggestion that some general theory exists, which might be applied to real social experiences, no longer is likely in itself to seem exciting (another reason comparative study is needed). Rather, theoretical ideas are used more familiarly to illumine specific research and in somewhat scholastic logical exercises.
4. Comparative study offers a special stimulus to—and benefits by—relating categories of analysis usually left separate: peasants or workers and the larger culture, ethnic consciousness and economic structure are examples. For all of these reasons, historical comparison seems likely to flourish in the next decade or two among those analyzing the intersection of social structure and culture, the relationship of high to popular culture, and the employment of economic and political power in international relations. Important theoretical questions are at stake in each topic, and concrete comparative analysis is needed to determine where the critical connections and problems are.
5. Most of the articles submitted to *Comparative Studies in Society and History* give full attention only to a single historical case; the next largest group treats several cases across time but within a single society, and such articles are followed by those that are essentially theoretical. The minority that give equal attention to cases from several different societies are not necessarily those that most effectively use comparison to make new discoveries.
6. Although most of the authors submitting manuscripts seem to believe that social structures are more important than social attitudes, or at least are more respectable as a subject for analysis, attitudes and ideas frequently receive more space. Many explanations leap to mind, but it is possible that, whereas our vocabulary for the analysis of social structures tends to underscore the similarities between cases, it is the differences (which are more readily expressed in terms of attitudes) that are more interesting.
7. Finally, the established interests of social scientists—mobility, political behavior, law, education, religious movements, and, even more suspect, modernization or revolution—can still, through systematic comparisons, provide the occasion for fresh and exciting new understanding.

THESE CONSIDERATIONS OF COMPARATIVE STUDY IN HISTORY—of the ways in which it is commonly understood and the somewhat different ways in which it is

more often effectively practiced—bring us back to questions of method. To have dealt with that more directly might have embarrassingly shortened this essay: there is no comparative method in history.²⁹ “The comparative method” is a phrase as redolent of the nineteenth century as “the historical method” with which it was once nearly synonymous. For historians, at least, the idea of comparison still needs to be demystified. Abstract discussion of historical comparison makes it seem so demanding in terms of knowledge and method and so taxing and complicated a procedure that only the most immodest could feel capable of attempting it and all of the rest are excused from trying. Indeed, scholars reluctant to break the framework of received opinions have an interest in adopting that view. But historical comparison is no more attached to a single method than is the discipline of history itself.

John Stuart Mill’s oft-cited distinction between the Method of Agreement and the Method of Difference is a contribution to logic, but it leaves the way open to different kinds of comparisons according to whether one is exploring new questions, defining a problem, isolating a single variable, identifying general patterns, or testing a hypothesis. The criteria for selecting which elements to compare, for checking the internal logic of the analysis, or for determining the relevance of the evidence used are not fundamentally different for comparison than for social analysis of any sort. There are not even general rules, except those of logic, and they apply differently according to one’s purpose, as to whether it is better to compare cases that are similar or contrasting, neighboring or distant, synchronic or diachronic.³⁰

To call for comparison is to call for a kind of attitude—open, questioning, searching—and to suggest some practices that may nourish it, to ask historians to think in terms of problems and dare to define those problems independently, and to assert that even the narrowest research should be conceived in terms of the larger quests of many scholars in many fields. To call for comparison, however, says almost nothing about how to do any of this well.³¹ In the United States, the excitement of comparison may be found more often in classrooms than in print and in the colleges where a few teach broadly than in the universities where many research narrowly. It may even be that this use of comparison is stimulating some of the venturesome work of younger scholars.

²⁹ “So the methodological problems facing ‘comparativists’ are the same as those facing all social-scientific investigators”; Neil J. Smelser, *Comparative Methods in the Social Sciences* (Englewood Cliffs, N.J., 1976), 3. Historians will find the discussions of method useful in Smelser’s book and in Stinchcombe’s *Theoretical Methods in Social History* (and especially enjoy comparing the two discussions of comparison in de Tocqueville) as well as in Vallier’s *Comparative Methods in Sociology* and Etzioni and Dubow’s *Comparative Perspectives: Theories and Methods*.

³⁰ Arend Lijphart’s valiant effort to define a comparative methodology for political science (a discipline in which it might be expected that comparison would be more readily controlled than it is in history) does not seem to me to be convincing; see Lijphart, “Comparative Politics and the Comparative Method,” *American Political Science Review*, 65 (1971): 682–93, and “Comparative Research.” Also see Theodore W. Meckstroth, “‘Most Different Systems’ and ‘Most Similar Systems,’” *Comparative Political Studies*, 8 (July 1975): 132–57; and Henry Teune, “Comparative Research, Experimental Design, and the Comparative Method,” *ibid.*, 195–99. On the response to Mill by Émile Durkheim and Max Weber, see Smelser, *Comparative Methods in the Social Sciences*, 62–67, 141–43.

³¹ “The choice of the phenomena to be explained is the responsibility of the historian, not the comparative method”; William H. Sewell, Jr., “Marc Bloch and the Logic of Comparative History,” *History and Theory*, 6 (1967): 213; also see *ibid.*, 213–18.

To admit that comparison contains no special method is not to say that methodology is unimportant. The search for problems is more than blind intuition and will benefit from the most formal methods applicable. Nor is the search for patterns mad empiricism, for it should welcome models and hypotheses that are as well developed as possible. Methods behavioral, quantitative, inductive, and even deductive, formal models, and theories of change can all be applied to comparative study and used to guide the historian in determining when comparisons support generalization and when generalization can be stretched to theory. Even the historian's concern for whether a hypothesis really fits a given context can be a disciplined test.³²

Not only is comparison not a method, but "comparative history" is a term better avoided. In his famous call for comparison, Marc Bloch warned he would put forth no "new panacea," and it is worth noting that he spoke of *histoire comparée*, not *histoire comparative*. For historians to think comparatively, to compare histories, is to do what we already do—a little more consciously and on a somewhat broader plane. It is not to embrace some new type or genre of historical investigation. Bloch cited Fustel de Coulange's aphorism that a day of synthesis requires years of analysis, quickly adding that "analysis can be used for synthetic purposes only if it intends from the outset to contribute to such a synthesis."³³ That warning points to the essence of historical comparison—the conduct of research and the presentation of findings in the context of a larger continuing discourse about the nature of society.

There is thus no paradox in insisting that the study of a single case can be comparative.³⁴ The use of common terms and recognizable categories assumes comparison. The search for patterns implies comparison by seeking regularities in the behavior and issues common to a certain set—a kind of group, institution, or form of social organization—or common to a certain process.³⁵ In searching for patterns the historian is following François Simiand's injunction of seventy-five years ago to study classes of problems. Elegant solutions to problems carefully posed invite further comparisons, the testing of patterns and hypotheses that is most generally recognized as a form of experiment.³⁶ At its best, the practice of comparison can lead toward that *histoire générale* for which Lucien Febvre argued and also establish agenda for concrete, new research. At its least, comparison is an effective device for removing the inexcusable blinders of parochialism.

³² See Donald T. Campbell's article, all the more impressive for its revision of his earlier argument, "Degrees of Freedom" and "The Case Study," *Comparative Political Studies*, 8 (July 1975): 178–93.

³³ Bloch, "Pour une histoire comparée," 16, 38. Presumably, the analogue of the term was *grammaire comparée*.

³⁴ William H. Flanigan and Nancy H. Zingale made this point in "The Comparative Method in Political Science," paper delivered at the annual meeting of the American Historical Association held in San Francisco, December 1978; so did E. A. Hammel in "The Comparative Method in Anthropological Perspective," a paper at the same session. An earlier version of this essay completed the panel at that meeting.

³⁵ Alexander Gerschenkron has called the industrial history of Europe "a unified and yet graduated pattern"; Gerschenkron, *Economic Backwardness in Historical Perspective* (Cambridge, Mass., 1966), 1.

³⁶ And these can be used to test "presumptive causal linkages"; Walter P. Metzger, "Generalizations about National Character: An Analytic Essay," in Louis Gottschalk, ed., *Generalization in the Writing of History* (Chicago, 1963), 90.

Articles and books need not do all of this explicitly, and, happily, it is impossible to list all possible comparisons relevant to any single case. But authors and readers who think in comparative terms enlarge the communication on which science rests. Even their tacit comparison eases the task of integrating concepts and methods from different disciplines by providing common categories of analysis and a common focus, the most effective counterweight to the penalties of specialization. Within the framework of comparison a dialogue and even a kind of teamwork can then cut across our disciplinary divisions. Arthur Wright once said that for the Chinese the discovery of comparative history and comparative sociology "was revolutionary in its effects on historical studies."³⁷ We would do well to work to make that revolution permanent in its historical home.

³⁷ Arthur F. Wright, "On the Uses of Generalization in the Study of Chinese History," in Gottschalk, *Generalization in the Writing of History*, 46.

Antebellum Planters and *Vormärz* Junkers in Comparative Perspective

SHEARER DAVIS BOWMAN

EVER SINCE PLANTATION MAGNATES IN COLONIAL VIRGINIA began to think of themselves as transplanted English country gentlemen, the friends, foes, and scholarly interpreters of the South's slaveholding planters have, on occasion, suggested their comparability to Europe's traditional feudal aristocracies. In view of the longstanding scholarly debate over the character of the Old South's planter class and proslavery ideology, historians have been surprisingly slow to make systematic comparisons of antebellum planters and Europe's surviving landed aristocracies of the same epoch. Although Peter Kolchin has focused on the *pomeshchiki* estate owners and the proserfdom thought of pre-1861 Russia as a standard of comparison,¹ the Junker gentry of eastern Prussia are frequently cited as the most appropriate historical analogue to the planters of the Old South.² The Junkers were certainly the most formidable landed elite in nineteenth-century Europe, and, in "Old Prussian" ideology, their apologists presented Europe's most intellectually rigorous and politically effective "aristocratic" opposition to the "French ideas of 1789."

By comparing antebellum planters with pre-1848 (*Vormärz*) Junkers, I hope to contribute a new perspective to the ongoing controversy over whether the planters were a genuinely "conservative" class and whether Southern proslavery

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¹ Kolchin, "In Defense of Servitude: American Proslavery and Russian Proserfdom Arguments, 1760-1860," *AHR*, pages 809-27, below, originally presented as a paper at the Ninety-Third Annual Meeting of the American Historical Association, held in San Francisco, December 28, 1978.

² See, in particular, Dietrich Zwickler, *Der amerikanische Staatsmann John C. Calhoun, Ein Kämpfer gegen die "Ideen von 1789": Studien zur Vorgeschichte des amerikanischen Bürgerkrieges*, Historische Studien, no. 280 (Berlin, 1935), pt. 2, chap. 1: "Gleichzeitige Ideen in Deutschland?" esp. 110-12; and the tantalizing comments in Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966), chap. 3: "The American Civil War: The Last Capitalist Revolution," esp. 115, 127. For pointed criticism of Moore's handling of the comparison, see Eugene D. Genovese, "Marxian Interpretations of the Slave South," in Barton Bernstein, ed., *Towards a New Past: Dissenting Essays in American History* (New York, 1968), 117-20, and *The World the Slaveholders Made: Two Essays in Interpretation* (New York, 1969), 228-30.

thought was a genuinely "conservative" ideology. The starkly contrasting views of two prominent American scholars illustrate the nature of the controversy. Louis Hartz has belittled the conservative authenticity of the planters and of proslavery thought by stressing America's lack of a feudal heritage, the South's inescapable attachment to the American liberal tradition (its "prejudices of liberty and equality"), and the middle-class, capitalist character of Southern society.³ Eugene D. Genovese, however, has argued for the genuine conservatism of the planters and of proslavery thought by insisting upon the "precapitalist" character of the Old South's "paternalistic" master-slave relationship and the consequent "prebourgeois" outlook of antebellum planters—"the closest thing to feudal lords imaginable in a nineteenth-century bourgeois republic."⁴ Hartz and Genovese seem to agree, however, that the planters could not simultaneously be both capitalists and authentic conservatives, a judgment that this essay will question.

Before addressing the problematical concept of conservatism and its applicability to planters and Junkers, it is necessary to establish some structural and attitudinal similarities between the two landed elites and some fundamental dissimilarities between their historical environments.⁵ My analysis of Southern planters concentrates on the four antebellum decades from the Missouri controversy of 1819–21 over slavery's westward expansion until secession and the outbreak of the Civil War in 1861. In discussing the Junkers, my focus is on the period from Baron vom Stein's edict abolishing serfdom in 1807 to the revolutions of 1848–49, which brought about the final liquidation of surviving servile dues. My essay will conclude with a comparison of the attitudes and behavior of planters and Junkers during the political crises of 1860–61 and 1848, since the particular "conservative" qualities of each landed elite manifested themselves with special clarity at these times of intensified stress and conflict.

JUNKERS AND PLANTERS CAN BE TERMED "gentlemen farmers," though not in the sense of English landlords living off tenant rents. They were gentlemen farmers in that they were freed not only from the necessity of manual labor but also from the need to serve as full-time supervisors of work in the fields.⁶ The term

³ Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought since the Revolution* (New York, 1955), pt. 4: "The Feudal Dream of the South," esp. 146–54, 189–97.

⁴ Genovese, *The Political Economy of Slavery: Studies in the Economy and Society of the Slave South* (New York, 1967), chap. 1: "The Slave South: An Interpretation," esp. 31. Genovese has developed this interpretation further; see his *The World the Slaveholders Made*, esp. 96–99, 121–26, and *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), esp. 3–7, 661–63.

⁵ As Marc Bloch pointed out in 1928, "In order to have historical comparison, two conditions must be fulfilled: a certain similarity or analogy between observed phenomena—that is obvious—and a certain dissimilarity between the environments in which they occur"; Bloch, "Toward a Comparative History of European Societies," trans. Jelle C. Riemersma, in Frederic C. Lane and Jelle C. Riemersma, eds., *Enterprise and Secular Change: Readings in Economic History* (Homewood, Ill., 1953), 496.

⁶ As Peter Laslett has explained, "The primary characteristic of the gentleman was that he never worked with his hands on necessary, as opposed to leisurely, activities"; Laslett, *The World We Have Lost: England before the Industrial Age* (1966; 2d ed., New York, 1971), 30. To inject the English term "gentleman" into a discussion of Prussian history is admittedly problematic, especially since the German language has no precise equivalent.

"Junker" (derived from the Middle High German for "young nobleman") identifies both noble and nonnoble owners of legally privileged estates (*Rittergüter*) in Prussia's six eastern provinces. Except for part of Brandenburg and most of Saxony, these provinces lay to the east of the Elbe River and are, therefore, referred to collectively as "East Elbia." Although the eleven thousand five hundred *Rittergüter* (literally, "knights' estates") in East Elbia varied in size even more than Southern plantations, the majority were larger than the 379 acres (600 *Morgen*) specified by government statisticians as the minimum for a "large landed estate." In 1851 the average *Rittergut* encompassed between 1,300 and 1,400 acres, had perhaps twenty to twenty-five resident families providing full-time agricultural labor, and employed a manager and often field foremen as well.⁷

The term "planter" denotes the larger and more elastic elite of agricultural slaveowners whose holdings in land and slaves were large enough that the job of superintending labor in the fields usually fell to white overseers or black foremen (drivers). This imprecise definition includes some slaveholders with fewer than the twenty slaves specified as the minimum for classification as a "planter" by census officials in 1860, who counted forty-six thousand planters; but the imprecision reflects the social fluidity of the antebellum planter class more accurately than does the census-takers' standard.⁸ One historian has suggested that the "accurate definition of a planter" should also include ownership of "a minimum of between 500 and 1,000 acres."⁹ Here, again, a more flexible standard is necessary, even to accommodate some substantial slaveholders like Charles Whitmore of Mississippi, who owned about thirty-five slaves and just over 300 acres near Natchez during the 1840s and 1850s, and Robert Christian of the Alabama

My purpose is simply to emphasize that freedom from the necessity of arduous physical labor and the concomitant freedom to enjoy civilized leisure were valued in the rural societies of both the South and eastern Prussia as prerequisites to social pre-eminence and, therefore, to popular recognition of elite status.

⁷ In 1851, over eleven thousand five hundred estates were classified as *Rittergüter* in East Elbia. I computed the number of estates and their average acreage from statistics in C. F. W. Dieterici, *Handbuch der Statistik des preussischen Staats* (Berlin, 1861), 318. The largest *Rittergut* encompassed 46,002 acres (72,904 *Morgen*; one *Morgen* equaled 0.631 acres) and was located in the province of Prussia, while the smallest *Rittergut*, located in Silesia, encompassed about six-tenths of an acre. As Dieterici, a government statistician, explained, "The status of a *Rittergut* is independent of the estate's size; it is quite possible for a *Rittergut* owner to sell arable land and fields and to retain for the house and small estate remaining the status of a *Rittergut*"; *ibid.*, 320-21.

⁸ Planters with more than thirty slaves tended to have the most efficient and complex operations and generally had both overseers and drivers, while planters who owned fewer than thirty slaves tended to rely on their sons or slave foremen. "Masters who had at their command as few as half a dozen field-hands were tempted to improve their social status by withdrawing from the fields and devoting most of their time to managerial functions." Kenneth M. Stamp, *The Peculiar Institution: Slavery in the Antebellum South* (New York, 1956), 35-38. One such small-scale but ambitious cotton planter was James Monette, of Morehouse Parish in northern Louisiana. During the fall of 1853 Monette had six to eight male and female pickers working in the fields, and his crop for that year amounted to a very respectable fifty bales, or 22,305 pounds. James Monette Day Book and Diary, 1848-1863 (typescript), Library of Congress. The most thorough student of antebellum agriculture, Lewis Cecil Gray, has distinguished between upper-class planters who owned fifty or more slaves and the much larger group of middle-class planters who owned ten to fifty slaves. Thus, according to Gray, there were nearly one hundred and ten thousand "planters" in 1860, out of a total of three hundred and eighty-five thousand slaveowners. Gray, *History of Agriculture in the Southern United States to 1860*, 1 (1933; reprint ed., New York, 1958), 481-83. My own definition of planters would place the number between Gray's figure and the census figure of 1860, but definitely closer to the latter. Although planters composed a small minority of slaveholders as well as of the South's white population, they owned well over half of the almost four million slaves in 1860.

⁹ Clement Eaton, *A History of the Old South* (3d ed., New York, 1975), 390.

Black Belt, who had twenty-five slaves and 400 acres in 1857.¹⁰ The planter class was a more fluid elite than the Junkers, both geographically and socially, as the increase in the number of "Southern" slave states from eleven to fifteen during the antebellum decades demonstrates. Of course, intraregional variations existed among planters and plantations in the South, just as they did among Junkers and *Rittergüter* in East Elbia. Not only geography but also notable economic and cultural differences separated Virginia and Louisiana, for example, or Pomerania and Silesia. But plantations throughout the South, like *Rittergüter* throughout East Elbia, displayed certain fundamental similarities as social institutions that transcended intraregional distinctions.

Although the legal and racial status of slaves on a plantation was certainly quite different from that of the laborers on a Junker estate (before as well as after the end of hereditary bondage in 1807), there were significant parallels between the productive purposes to which menials on plantations and *Rittergüter* were put and between the ways in which they were governed. Each work force was subject to the personal, nearly despotic, authority of the owner, and each worked to produce cash crops for foreign and domestic markets. While Southern planters were growing cotton or tobacco for shipment to Liverpool or New York, for example, East Elbian Junkers were producing wheat or wool for shipment to London or Berlin. At mid-century most plantations and *Rittergüter* also achieved a high, cost-efficient level of self-sufficiency in basic foodstuffs as well. The functional and structural analogies between the plantation and the *Rittergut* are crucial to a comparative study of planters and Junkers, because these estates and their work forces constituted the foundations of their owners' wealth, political influence, social status, and, in many instances, even their self-esteem.¹¹

During the first half of the nineteenth century, moreover, the owners of *Rittergüter* became what the owners of plantations had always been—a socially mixed class of agrarian entrepreneurs. After Stein's edict of October 1807 declared an end to both serfdom and the nobility's de jure monopoly on the ownership of Junker estates, any wealthy and ambitious "commoner" could legally purchase a *Rittergut* and the aristocratic privileges tied to it. By 1856, 45 percent belonged to untitled commoners, including one who was the son of a rural cottager.¹² But the purchase of Junker estates by many nonnobles is not as signifi-

¹⁰ Mark Swearingen, "Thirty Years of a Mississippi Plantation: Charles Whitmore of 'Montpelier,'" *Journal of Southern History*, 1 (1935): 201; and the Robert Christian Diary, 1854–57, "Fair Oaks," Perry County, Alabama, Alabama State Department of Archives and History, Montgomery, Ala. Both Whitmore and Christian were cotton planters.

¹¹ For the importance of his land and work force to a planter's public stature and self-esteem, see James L. Roark, *Masters without Slaves: Southern Planters in the Civil War and Reconstruction* (New York, 1977), esp. 35. For a comparable perspective on the Junkers, see Heinrich Heffter, *Die deutsche Selbstverwaltung im 19. Jahrhundert: Geschichte der Ideen und Institutionen* (Stuttgart, 1950), 132.

¹² I have computed the percentage of *Rittergüter* owned by commoners from figures in Karl Friedrich Rauer, *Hand-Matrikel der in sämtlichen Kreisen des preussischen Staats auf Kreis- und Landtagen vertretenen Rittergüter* (Berlin, 1857), 451. The cottager's son was Johann Gottlieb Koppe, a widely respected agronomist who worked his way up first as the efficient manager of a noble-owned *Rittergut* and then as the innovative lessee of a crown estate. In 1842 he bought two *Rittergüter* in Brandenburg, and shortly thereafter he was appointed to the *Landes-Oeconomie-Collegium*, a fifteen-member committee under the Ministry of the Interior and the most prestigious governmental agency for agricultural affairs in Prussia. See "Johann Gottlieb Koppe," *Allgemeine Deutsche Biographie*, 16 (Leipzig, 1882): 693–97.

cant as the influence that the free trade in these estates had on the character of the Junkers as a landed elite, especially during the depression years of the 1820s. Titled Junkers who held on to their estates were usually able to do so only because they adopted the innovative, cost-conscious spirit of the nonnoble newcomers to their ranks.¹³ Captain Carl von Wulffen-Pietzpuhl of Brandenburg, writing in 1845 about the educational value of model farms for "the advancement of Prussia's practical agriculture," declared that "the most rational" farmer managed to use "land and soil most effectively" and that "the most important aspect of rational agriculture" could be "reduced to the art of producing the cheapest dung."¹⁴ Mid-century Junkers, like antebellum planters, understood that a profitable commercial agriculture was essential in the long run to their survival as landed gentlemen. Both elites can be evaluated as "agrarian capitalists" in that they were impelled by the need and desire to earn substantial profits from their capital investments in producing agricultural commodities.¹⁵

Describing antebellum planters and pre-1848 (*Vormärz*) Junkers as agrarian capitalists, while also recognizing that they generally valued the status and lifestyle of a landed gentleman as ends in themselves, does not entail any inconsistency or contradiction. Alfred Huger of South Carolina complained about "the fools and swindlers" on New York's Wall Street and described himself as "a 'Plantation Man' " who treasured "the quiet and retirement of Plantation Life." Yet in 1856 he offered sound advice about establishing a successful farm to the son of an old friend off to settle in turbulent Kansas: look for "good land selected where a town is most likely to be established, and the facility of reaching the point where crops are to be sold"; in particular, "look always to a water course—the shortest way to market is the shortest way to Wealth."¹⁶

¹³ For the most compelling presentation of this argument, see Hans Rosenberg, "Die Pseudodemokratisierung der Rittergutsbesitzerklasse" (1958), in his *Probleme der deutschen Sozialgeschichte* (Frankfurt a/M., 1969), 14, 17, 21–22. For British historian David Spring's recent misinterpretation of Rosenberg's argument, see Spring, "Landed Elites Compared," in Spring, ed., *European Landed Elites in the Nineteenth Century* (Baltimore, 1977), 5, 19 n. 20. And, for reaffirmations of Rosenberg's interpretation, see Reinhart Koselleck, *Preussen zwischen Reform und Revolution: Allgemeines Landrecht, Verwaltung, und soziale Bewegung von 1791 bis 1848* (Stuttgart, 1967), 512–14; and Hanna Schissler, *Preussische Agrargesellschaft im Wandel: Wirtschaftliche, gesellschaftliche, und politische Transformationsprozesse von 1763 bis 1847* (Göttingen, 1978), 165–67, 199–201.

¹⁴ Hauptmann von Wulffen auf Pietzpuhl, "Gutachten betreffend die von Herrn Franz zu Eggenstadt bei Seehausen . . . mitgetheilte Denkschrift: Über die Notwendigkeit eigenthümlich gestellter Musterwirtschaften behufs der Förderung der praktischen Landwirtschaft Preussens," *Annalen der Landwirtschaft in der königl. Preussischen Staaten*, 5 (1845): 60.

¹⁵ In his April 1979 presidential address to the Organization of American Historians, Eugene D. Genovese reiterated his argument that the slave South had a "precapitalist" economic system, that "the liberation of entrepreneurship historically accompanied the free market, especially in labor power, and [that] entrepreneurship, like science, technology, education, and investment in 'human capital' in general, arose as a function of freedom and everywhere suffered in the absence of freedom"; Genovese and Elizabeth Fox-Genovese, "The Slave Economies in Political Perspective," *Journal of American History*, 66 (1979): 22, 15. This perspective seems to confuse the historical conditions prerequisite to the development and flowering of "industrial capitalism" with the entrepreneurial, market- and profit-oriented behavior common to all capitalists. I agree with Jürgen Kocka's definition of capitalism as "an economic system that rests predominantly on private property and private disposition over capital and that facilitates production and exchange of commodities for the purpose of profit." As Kocka has suggested, legally free workers under contract and receiving wages are a defining characteristic of "modern industrial capitalism." Kocka, *Klassengesellschaft im Krieg: Deutsche Sozialgeschichte, 1914–1918* (Göttingen, 1973), 149 n. 14.

¹⁶ Huger to R. Bunch, October 20, 1857, to William Porcher Miles, January 23, 1958, and to John Vanderhorst, August 1, 1856; Alfred Huger Letterpress Books, 1853–1863, Perkins Library, Duke University, Durham, N.C.

Although landed Junkerdom was a more fluid and entrepreneurial class in 1850 than in 1800, even on the eve of Stein's edict of 1807 the Junkers and their serfs were no more a historical legacy of noncommercial seigniorialism than were Southern planters in the era of Jefferson and Madison. *Rittergut* owners in regions bordering the Baltic Sea used serf labor to produce grain for export to Western and Southern Europe a century and a half before the founders of colonial Virginia's plantation gentry began shipping tobacco to England; and serfdom east of the Elbe River only became established in the late fifteenth to the mid-seventeenth centuries. Like plantation slavery in the colonial South, East Elbian serfdom evolved primarily as a combined entrepreneurial and political maneuver to secure a labor force suitable for the production of cash crops on large estates. As German historian Georg Friedrich Knapp observed in 1891, "The modern big business concern of the *Rittergut* began with the forced labor of unfree persons, just as the modern plantation enterprise began with the forced labor of the unfree; but the planter obtained Negroes in Africa and made them slaves; the *Rittergut* owner did not reach so far, for he seized his peasants and made them serfs."¹⁷ Thus, both Junkers and planters established themselves as landed elites long before the onset of the Industrial Revolution, during the "early modern" centuries that witnessed the emergence of what Immanuel Wallerstein has termed a "capitalist world-economy" centered in Britain and the Low Countries.¹⁸ But the planters' and Junkers' fortunes as agricultural entrepreneurs were revitalized during the late eighteenth and early nineteenth centuries by the rise of English industrial capitalism and the growth of English demand for foreign wheat, cotton, and wool. In East Elbia this revitalization of large-scale, cash-crop agriculture involved the abolition of serfdom, while in the American South it brought the expansion of slavery.

During the three *Vormärz* decades prior to the Revolution of 1848, the make-up of the Junkers' labor force and organization of production on their estates underwent far-reaching transformations that could have had no parallel in the very different historical context of the Old South. The Stein-Hardenberg agrarian reforms, instituted piecemeal by the Prussian bureaucracy between 1807 and 1821, compelled some inveterately traditionalist Junkers—and allowed others more flexible in their attitudes—to make a gradual transition from serf labor to a contractual work force. These agrarian reforms entitled the *Rittergut* owner to demand from his freed and "regulated" (*reguliert*) peasants, as compensation for the abolition of serfdom and compulsory labor, long-term commutation payments or the cession of peasant land. Under Hardenberg's retrogressive Declaration of 1816, however, the majority of former serfs were made ineligible for the regulation of their servile obligations, because either the size of their landholdings or the longevity of their titles to the land did not meet the legal requirements. Very often these unregulated peasants, already suffering from the

¹⁷ Knapp, "Die Erbuntertänigkeit und die kapitalistische Wirtschaft," in his *Die Landarbeiter in Knechtschaft und Freiheit* (2d ed., Leipzig, 1909), 57.

¹⁸ Wallerstein, *The Modern World-System: Capitalist Agriculture and the Origins of the European World Economy in the Sixteenth Century* (New York, 1974), 88–91, 99–100.

loss of their former rights to use the common lands, found it impossible to support themselves on their own small farms while having to pay state taxes and perform services for their former lords. Consequently, they sold out to the *Rittergut* owner but usually continued to work for him as cottagers on annual contracts (*Insten* and *Lohngärtner*).

These cottagers became the most important group of workers on Junker estates during the middle decades of the nineteenth century, although Junkers also employed contractual servants (*Gesinde*) and often hired extra day-laborers for the harvest or major agricultural improvements (just as some Southern planters hired free laborers to undertake projects like ditching and secured outside free or slave labor at harvest time). The East Elbian cottager and his family generally received a small house on a half acre of garden land suitable for potatoes and vegetables and the right to graze a few animals on the lord's meadow—the sort of compensation, one might say, that many slaves also received from their masters. But the cottager family received, in addition, the yield from several acres of the Junker's crop land, a fraction of the Junker's grain threshed by the cottager during the fall and winter months, and a small monetary wage. In return, two and sometimes three members of the cottager's family were required to work year-round on the *Rittergut* for six days per week (minus occasional vacations) and from sunrise to sunset.¹⁹

The Stein-Hardenberg reforms, by enabling the Junker to consolidate his heretofore scattered fields and to absorb former peasant and common land into his own estate, both facilitated and spurred the Junkers' transition to more productive, complex, and labor-intensive systems of crop rotation and soil fertilization adapted from English practices. The rapid growth of East Elbia's population during *Vormärz* provided the labor necessary to bring new land under the plow and to cultivate new field crops (especially root crops) at a time when agrarian technology was not yet able to substitute mechanical for human and animal power on a large scale.²⁰ In the antebellum South, human labor was never so cheap or plentiful as in prerevolutionary Prussia, despite the increase in the slave population from one and a half million in 1820 to nearly four million in 1860. But Negro slavery enabled the planters to avoid the labor shortage that confronted Northern farmers and, at the same time, increased the amount of money that could be profitably invested in plantation agriculture.²¹ Moreover,

¹⁹ The following works are especially informative on cottagers: Oberamtmann Proseleger, "Über den Zustand der landwirtschaftlichen Verhältnisse in Graudenz Kreis, 1843," *Annalen der Landwirtschaft*, 8 (1846): 74–78, reprinted in Georg Friedrich Knapp, *Die Bauernbefreiung und die Ursprung der Landarbeiter in den älteren Theilen Preussens*, 1 (2d ed., Munich, 1927): 337–40; Max Weber, *Die Verhältnisse der Landarbeiter im ostelbischen Deutschland*, vol. 55 of *Schriften des Vereins für Sozialpolitik* (Leipzig, 1892), 11–18; and Günther Franz, "Landwirtschaft, 1800–1850," in Hermann Aubin and Wolfgang Zorn, eds., *Handbuch der deutschen Wirtschafts- und Sozialgeschichte*, 2 (Stuttgart, 1976): 302–04.

²⁰ "The immense increase in Prussia's population from roughly ten to sixteen million between 1815 and 1848 came mostly from the rural districts of the East Elbian provinces, where the agrarian reforms made possible, and at the same time encouraged, that growth"; Koselleck, *Preussen zwischen Reform und Revolution*, 503. As Günther Franz has pointed out, human and animal power predominated in East Elbia at least until the post-1850 development of the steam plow; "Landwirtschaft, 1800–1850," 292.

²¹ Heywood Fleisig has suggested that slavery, by relaxing the labor constraint that Southern planters would have faced under a free-labor system, may have served to retard industrialization in the South by in-

slavery, unlike serfdom, had never presented obstacles to accelerated innovation and profitability in Southern agriculture, for slaves had neither formal ties to nor claims upon the land and, therefore, presented no hindrances to the planter's free disposition over his property or to a "rational" concentration of land in fewer hands.

If antebellum planters were, on the whole, slower than *Vormärz* Junkers to institute improved crop rotations, stall feeding of livestock, and manuring or marling, the fundamental explanation is threefold: in the South fertile land was cheaper and more abundant, fewer mouths had to be fed from the same plot of land because population density was much lower,²² and plantation labor was scarcer and dearer, which encouraged most planters to concentrate their efforts on immediately profitable staples rather than on diversified agriculture. Yet slavery was well suited to the plantation economy's need for a supply of gang laborers whose geographical mobility made them highly responsive to constant, often rapid, shifts in demand along an advancing frontier.²³ Furthermore, slaves, as chattel, were themselves a lucrative capital investment during the antebellum decades. Hence, Thomas Affleck's widely used *Cotton Plantation Record and Account Book* included a section on "The Duties of an Overseer," which stipulated that the primary component of "a *fine crop*" was "an increase in the number and a marked increase in the condition and value of the negroes."²⁴

BOTH PLANTATION and *Rittergut* had a commercial-political character—a cash-crop business combined with a semi-autonomous community over which a single individual wielded inheritable and salable patrimonial power. In other words, plantation and *Rittergut* constituted what an antebellum Virginia academic termed "an *imperium in imperio*—a government within a government" or what a German historian has more recently called "a formidable 'private law state.'"²⁵ The nearly despotic authority that an antebellum planter or *Vormärz*

creasing the amount of money that could profitably be invested in farming; Fleisig, "Slavery, the Supply of Agricultural Labor, and the Industrialization of the South," *Journal of Economic History*, 36 (1976): 581–83. Hanna Schissler made a comparable argument for East Elbia, claiming that agricultural growth during *Vormärz* benefited from population growth but hindered industrialization "because agriculture itself absorbed labor power, attracted entrepreneurial talent, and needed capital"; *Preussische Agrargesellschaft im Wandel*, 187.

²² The population per square mile in the coastal planting states (South Carolina, Georgia, Florida, Alabama, Mississippi, and Louisiana) was 15.25 in 1860 and 20.93 for the central slave states (Virginia, North Carolina, Kentucky, Tennessee, Missouri, and Arkansas) in the same year; U.S. Census Office, *Preliminary Report of the Eighth Census, 1860* (Washington, 1862), 8. In 1858 the Baltic provinces of Prussia, Posen, and Pomerania in East Elbia—the least populous by far of Prussia's eight provinces—contained approximately 113 persons per square mile; I have computed this figure from statistics in Georg von Viebahn, *Statistik des zollvereinigten und nördlichen Deutschlands*, 2 (Berlin, 1862): 164.

²³ Edgar T. Thompson, "The Plantation: The Physical Basis of Traditional Race Relations" (1939), in his *Plantation Societies, Race Relations, and the South—The Regimentation of Populations: Selected Papers of Edgar T. Thompson* (Durham, N.C., 1975), 89–90.

²⁴ Affleck, "The Duties of an Overseer," reprinted in the *American Cotton Planter*, 2 (1854): 356.

²⁵ William A. Smith (President of Randolph Macon College), *Lectures on the Philosophy and Practice of Slavery* (Nashville, Tenn., 1856), 154; and Hans Rosenberg, *Bureaucracy, Aristocracy, and Autocracy: The Prussian Experience, 1660–1815* (Boston, 1958), 43.

Junker exercised over those who lived and labored on his land could and did serve as an invaluable instrument of direct economic coercion;²⁶ but in the minds of most planters and Junkers maintaining the subordinate position of their laborers assumed supra-economic significance as a social, political, or—in the case of the South—racial necessity as well. To be sure, the legal source of a planter's authority over his slaves was not the equivalent of a Junker's over his cottagers and servants. A planter's autocratic power derived from his ownership of slaves as personal property, as chattel, not from his ownership of any particular tract of land on which the slaves worked. By contrast, the authority of a Junker, whether nobleman or commoner, derived from his ownership of the *Rittergut*, which entitled him to exercise, either in person or through a personally designated representative, such powers of local government as police authority, lesser justice, and supervision over church and school affairs. Thus the Junker was a *Gutsherr*, a lord of an estate, while the planter was a master of slaves.²⁷

Idealistic, romantic spokesmen for the two landed elites tended to portray government on both plantation and *Rittergut* as a familial, patriarchal relationship between "benevolent fathers" and "obedient sons."²⁸ James H. Hammond of South Carolina, in a pair of widely quoted letters written in 1845 to the president of the British Anti-Slavery Society, described antebellum plantation practices as "our patriarchal scheme of domestic servitude" and maintained that few social ties "are more heartfelt, or of more beneficent influence, than those which mutually bind the master and the slave."²⁹ F. A. Ludwig von der Marwitz of Brandenburg argued in 1811, in response to Stein's condemnation of serfdom as "the last vestige of slavery," that "serfdom was rather a patriarchal bond that joined the peasants to the nobleman" and that the *Rittergut* constituted a "small patriarchal state" governed by "a member of the family."³⁰ A few iconoclastic planters and Junkers, however, vigorously decried such apologetic incantations of paternalism. East Elbia's closest equivalent to the South's James G. Birney, the Alabama lawyer-planter who became a vocal abolitionist in the 1830s, was probably the Silesian Count zu Dohna-Kotzenau, whose strictures of the status quo won him the title "the red count." In a short book published in 1847, the purpose of which was to stir up public concern about the deplorable conditions

²⁶ The issue is clear-cut regarding the planters. On the Junkers, see Koselleck, *Preussen zwischen Reform und Revolution*, 539–53; and Schissler, *Preussische Agrargesellschaft im Wandel*, 167–77.

²⁷ The Junker's *Gutsherrschaft*—literally, the authority of an estate—extended over the inhabitants of the small rural district (*Gutsbezirk*) within which his own *Rittergut* was located. After the Stein-Hardenberg reforms the Junker's authority became less significant for the peasant villages outside the boundaries of his own estate and more important for the cottagers and servants who lived and labored on the *Rittergut*. Koselleck, *Preussen zwischen Reform und Revolution*, 544–49. The Junker's patrimonial judicial authority was abolished in 1849, but his far more exploitable right to exercise local police power survived until 1872.

²⁸ Robert M. Bigler, *The Politics of German Protestantism: The Rise of the Protestant Church Elite in Prussia, 1815–1848* (Berkeley and Los Angeles, 1972), 147. Stampp observed of slavery that "ideally it was the relationship of parent and child"; *The Peculiar Institution*, 327.

²⁹ Hammond, "Two Letters on the Subject of Slavery in the United States, Addressed to Thomas Clarkson, Esq.," in Hammond, *Selections from the Letters and Speeches of the Hon. James H. Hammond* (New York, 1866), 184. These letters were published before the Civil War under the title "Slavery in the Light of Political Science," in E. N. Elliott, ed., *Cotton Is King, and Pro-Slavery Arguments* (Augusta, Ga., 1860), 634–746.

³⁰ Marwitz, "Kritik des Steinschen Testaments," in Friedrich Meusel, ed., *Friedrich August Ludwig von der Marwitz: Ein märkischer Edelmann im Zeitalter der Befreiungskriege*, 2, pt. 1 (Berlin, 1913): 240, 242.



Figure 1: James Henry Hammond, Southern planter, in the uniform of the commander in chief of the South Carolina Militia, a rank he held as governor of South Carolina. Photograph from a copy of a painting by Gerald Foster, ca. 1842, reproduced courtesy of South Caroliniana Library.

of the Prussian working classes, Dohna-Kotzenau responded to traditionalists who extolled the pre-1807 master-serf relationship:

It is ironic that this relationship has been labeled patriarchal. Its participants were not united into a family by indissoluble bonds; the dependents were chained indissolubly only to the authority of the *Rittergut*, whose owner could sell the estate to a new lord without hindrance and, with it, the dependents. The children awaken one morning and discover that their father has sold them, that they have a new father. Is this the subject for a family portrait? . . . Nor have I spoken here of a lord who was tyrannical toward his dependents but of a kind, considerate lord who lightened the labor obligations of his subordinates when possible and who maintained them in misfortune; but I believe I have shown that he could only be a kind lord and that he stood in too high and isolated a position to be called the father of a family.³¹

Applying the logic of Dohna-Kotzenau's argument to the master-slave relationship in the Old South raises noteworthy semantic objections to any scholar's use of the terms "patriarchal" or "paternalistic" to characterize the institution of slavery. In the first place, their etymologies have imbued these adjectives with strong connotations of familial attachment and fatherly concern, which seem to

³¹ Hermann Graf zu Dohna-Kotzenau, *Die freien Arbeiter im preussischen Staate* (Leipzig, 1847), 10–11.



Figure 2: Friedrich August Ludwig von der Marwitz, Prussian Junker, in the uniform of a Prussian cavalry general (ret.), and marshal of the Brandenburg Provincial Landtag. Photograph from a painting by Franz Krüger, ca. 1827, taken from Walter Görlitz, *Die Junker* (1964), and reproduced courtesy of C. A. Starke Verlag.

survive all social scientific efforts to provide them with ethically neutral definitions.³² More significant, to evaluate slavery as an institution with language that recalls the planters' own flattering appraisal of "domestic servitude" is to compound the difficulties inherent in the historian's obligation to distinguish clearly between the images historical actors had of themselves and the reality of their historical situation.³³ Although many a planter, like many a Junker, viewed his

³² Anthropologist John W. Bennett has explained that "the derivation of the term 'paternalism' from a Latin-English kinship term suggests its root meaning: a type of behavior by a superior toward an inferior resembling that of a male parent to his child—in most cases, a son." Hence Bennett has found it necessary to distinguish clearly between "benevolent" and "exploitative" modes of paternalistic action. Bennett, "Paternalism," in *International Encyclopedia of the Social Sciences*, 2 (2d ed., New York, 1968): 472, 472-77.

³³ In *Roll, Jordan, Roll* Eugene Genovese made a concerted but tortuous effort to identify "paternalism" as the definitive characteristic of Southern slavery, while distinguishing between "paternalism" as the planters understood it and "paternalism" as the slaves themselves allegedly understood it. Nonetheless, the term "paternalism" still seems incongruous with: a system of labor relations, the logic of which, Genovese claimed, "pushed the masters to break their slaves' spirit and to reconstruct it as an unthinking and unfeeling extension of their own will." Only "the slaves own resistance to dehumanization compelled the masters to compromise in order to get an adequate level of work out of them." *Roll, Jordan, Roll*, 317. My argument on this issue also puts me somewhat at odds with another formidable scholar—Max Weber, whose research on nineteenth-century Junkers and their labor force has been so valuable to German historians. In 1906 Weber observed, "In the first half of the last century the Junker was a rural patriarch"; Weber, "Capitalism and Rural Society in Germany," in Weber, *From Max Weber: Essays in Sociology*, ed. H. H. Gerth and C. Wright Mills (New York, 1946); 382.

position as that of a kindly paterfamilias tending to the welfare of his faithful "people,"³⁴ the actual interaction between a planter and his laborers seldom approximated this patriarchal ideal—especially when the "people" were field hands rather than house servants or skilled artisans. The Mississippi planter Cornelius Vanhauten composed a will in 1850 that indicates how the financial constraints of chattel slavery could inhibit the paternalistic intentions of even an unusually benevolent slaveholder: "4th. I direct and empower my Executors to sell all my Negro Slaves with the exception of Joe hereafter to be disposed of at private sale to humane masters provided it can be done to advantage near the valuation fixed by my appraisers but in no event to separate families."³⁵

Although it seems inappropriate to characterize the relations between either planters and their slaves or Junkers and their cottagers as "patriarchal" or "paternalistic," the need of both landed elites to obtain productive labor from sentient human beings, combined with the ethical dictates of Christianity, induced both planters and Junkers alike to seek the cooperation of their workers through more positive incentives than fear and compulsion. In fact, landowners distinguished more by self-interested practicality than by altruistic benevolence often stressed that considerate treatment of slaves or estate laborers could yield substantial dividends in terms of worker efficiency and loyalty. In 1842, Judge John Belton O'Neill of South Carolina claimed to members of his state's agricultural society, "You cannot succeed with negroes, as operatives, as you desire to do, *unless you feed and clothe [them] well*. —Make them contented, and then 'Massa' will be, as he ought to be, the whole world to them."³⁶ And, in 1849, an East Prussian estate owner noted in the Berlin agricultural journal, *Annalen der Landwirtschaft*, "In general, the cottagers, if they are well treated and also supervised, are capable and not incompetent workers who perform more work on their poor diet than one ought to expect; one must seek only to maintain their good will and fresh spirit. Without supervision and with bad treatment they accomplish little and do not take adequate care of property."³⁷

Southern planters, of course, had yet another and often more compelling incentive to treat their slaves with at least a modicum of benevolence—the huge capital investment that plantation labor, unlike *Rittergut* labor, constituted. A slaveholder could expect to reap substantial capital gains from the increase in both the market value and the number of his slaves. Andrew Flynn of Mississippi went so far as to advise his overseer that "the children must be very partic-

³⁴ Franz Rehbein (1867–1909) worked on a Pomeranian *Rittergut* as a boy, and his autobiography, originally published in 1911, provides vivid testimony about the self-consciously patriarchal bearing of the estate owner toward his "people" (*Leute*) as late as the 1880s; see Rehbein, *Das Leben eines Landarbeiters*, ed. Karl Winfried Schafhausen (Darmstadt, 1973), 52. In regard to planters, Genovese has stated, "The slaveholders' vision of themselves as authoritarian fathers who presided over an extended and subservient family, white and black, grew up naturally in the process of founding plantations"; *Roll, Jordan, Roll*, 74. Much the same argument could be made for the Junkers.

³⁵ Vanhauten, Manuscript Will (Draft), October 9, 1850, in the Richard Abbey Papers, Mississippi State Department of Archives and History, Jackson, Miss.

³⁶ O'Neill, "Judge O'Neill's Address Delivered before the South Carolina Agricultural Society, December 29, 1842," *Southern Cultivator*, 1 (1843): 109.

³⁷ Gutsbesitzer Lüdersdorf auf Christoplack, "Die landwirtschaftlichen Verhältnisse des Labiau Kreises, in Reg.-Bezirk Königsberg," *Annalen der Landwirtschaft*, 13 (1849): 129–30.

ularly attended to, for rearing them is not only a duty, but also the most profitable part of plantation business." Hence, there was some truth in the contention of Dr. William C. Daniell of Georgia that planters were "the only capitalists whose system embraces humanity to the laborer consistently with economy of investment."³⁸

PLANTERS AND JUNKERS ARE COMPARABLE primarily because of certain similarities between the antebellum plantation and *Vormärz Rittergut* as commercial enterprises and political communities. There were, however, vast dissimilarities between their respective historical environments. Though the plantation and the Junker estate were both hierarchical and authoritarian institutions, the larger society within which the planters defended slavery was less hierarchical and more fluid, less authoritarian and more democratic, than the larger society within which the Junkers defended the privileges of *Rittergut* ownership. Successful, constructive use of the comparative method certainly does not require proving that analogous phenomena were of greater historical significance than the differences between their environments. As Marc Bloch stated, the comparative method may contribute primarily to the isolation and analysis of "the 'originality' of different societies."³⁹

The Prussian state during *Vormärz* was a hereditary monarchy whose power rested primarily upon a strong standing army and an extensive civil bureaucracy. Since the mid-seventeenth century the maintenance and expansion of the army—an absolute necessity for the monarchy in view of its precarious position on the plains of north-central Europe—had encouraged the growth of a professional state bureaucracy, which in time became an independent power in its own right and, under the Civil Code (*Allgemeines Landrecht*) promulgated in 1794, even obtained its own legally privileged status in Prussian society. Eighteenth-century Junkers generally found service as army officers more congenial than employment as bureaucrats, in part because of their rustic life-style and poor education and in part because of the ease of stepping from exercising patrimonial authority over peasants on the *Rittergut* to wielding military command over peasants on the battlefield. Frederick William I and Frederick the Great made the officers' corps a virtual Junker preserve, thereby serving the interests both of less affluent Junker families and of Prussian monarchs anxious to secure the loyalty of their parochial landed aristocracy. Within the civil service, however, the regular recruitment of bureaucrats from outside Junker ranks fostered the development of socioeconomic and political values different from and sometimes even antagonistic to those of landed Junkerdom. Although Prussia's military humiliation at the hands of Napoleonic France in 1806 provided the immediate impetus for the Stein-Hardenberg reforms, the reform statutes were

³⁸ Flynn, 1840 Plantation Rules, Andrew Flynn Plantation Book (microfilm), in the Southern Historical Collection, University of North Carolina, Chapel Hill, N.C.; and Daniell, "An Address" [to a convention called to organize an Agricultural Association of the Slaveholding States, Montgomery, Ala., May 2, 1853], *American Cotton Planter*, 2 (1854): 66.

³⁹ Bloch, "Toward a Comparative History," trans. Riemersma, 507.

conceived and implemented by a bureaucracy that contained many educated "commoners" and non-Junker noblemen (such as ministers Stein and Prince von Hardenberg, for example, who were originally from Nassau and Hannover). These reformers tended to look upon the "boundness" of the peasantry and the Junkers' traditional corporate privileges as antiquated obstacles to the growth of Prussian state power, the increased vitality of Prussia's economy, and the evolution of a social order founded on the classical liberal principle of "careers open to talent."

Nonetheless, the reformers' respect for the sanctity of private property and their distrust of political democracy prevented them from attempting any wholesale reconstruction of Prussian society, much as the victorious North's regard for property rights and distrust of the Negro freedmen prevented the federal government from attempting a wholesale reconstruction of Southern society after the Civil War. Indeed, as has been suggested above, the Stein-Hardenberg reforms had the effect of strengthening the economic vitality of *Rittergut* agriculture prior to 1848 and thereby "made it possible for the aristocracy, as a class, to maintain and strengthen itself."⁴⁰ This economic rejuvenation was, moreover, accompanied by a partial social and political reconciliation between landed Junkerdom and the upper ranks of the bureaucracy, as increasing numbers of Junker sons attended universities and entered the civil service and as senior officials realized that the smoldering movement for civil equality and constitutional government posed a threat to their own privileged status under the Civil Code as well as that of the Junkers.⁴¹ The extent of this Junker-bureaucrat rapprochement became apparent during the revolutions of 1848–49, when the higher echelons of the civil service rejected democracy and radicalism with as much fervor as did most Junkers, while the army and its aristocratic officers' corps constituted the rock of monarchical loyalty on which the wave of revolution eventually broke. Thus, the Junkers' "privileged position next to the bureaucracy and the military in the Prussian power syndicate,"⁴² in conjunction with their shrewd adaptability as agrarian entrepreneurs, enabled them to be a tenacious and formidable landed elite throughout the nineteenth and early twentieth centuries.

The Junkers' involvement with military and bureaucratic absolutism and their skill as agrarian capitalists did not preclude their holding fast to the traditional corporatist view of society as a hierarchical and "organic" structure of distinct social groupings—the *Stände* ("estates")—topped, of course, by the large landowners.⁴³ This conception of Prussian society was a legacy from the pre-ab-

⁴⁰ Koselleck, *Preussen zwischen Reform und Revolution*, 85.

⁴¹ "The influx of the impoverished lesser nobility into middle and upper bureaucratic positions also strengthened the bureaucracy's feudal perceptions"; Walter Görnitz, *Die Junker: Adel und Bauer im deutschen Osten* (3d ed., Limburg a/L., 1964), 229. Also see, for a clear and concise analysis, John R. Gillis, "Aristocracy and Bureaucracy in Nineteenth-Century Prussia," *Past & Present*, no. 41 (1968): 107–19.

⁴² Hans-Jürgen Puhle, "Aspekte der Agrarpolitik im 'Organisierten Kapitalismus,'" in Hans-Ulrich Wehler, ed., *Sozialgeschichte Heute: Festschrift für Hans Rosenberg zum 70. Geburtstag* (Göttingen, 1974), 547.

⁴³ For a concise definition of *Stand* from a traditional perspective, see "Stand" in *Dr. Johann Georg Krünitz's ökonomisch-technologische Encyclopädie*, cont. Johann Wilhelm David Korth, 169 (Berlin, 1838): 604–05. Also see Robert M. Berdahl, "The *Stände* and the Origins of Conservatism in Prussia," *Eighteenth-Century Studies*, 6 (1973): 298–321.

solulist conditions of the fifteenth to seventeenth centuries, when the members of the landed aristocracy (*Ritterstand*) tended to look upon their Hohenzollern prince as *primus inter pares*. During *Vormärz* the corporatist view of society served to justify the Junkers' privileged representation in and political dominance over both the county and provincial assemblies (*Kreistage* and *Landtage*), which were important to *Rittergut* owners primarily as institutional counterweights to the "mechanistic" power of the bureaucracy.⁴⁴ Even after the revolutions of 1848–49 introduced quasi-parliamentary constitutionalism to Prussia, Junker spokesmen like Friedrich Julius Stahl argued that the landed interest was entitled to privileged electoral representation in the national legislature as the country's most important occupational estate (*Berufstand*).

In the American South slavery and the plantation regime did instill some military values in planter society, and many planter sons pursued careers as officers in the U.S. Army. But the geographical situation of the American colonies and states, like that of England, obviated the need for a strong standing army; and the absence of such an army precluded the pre-industrial development of a centralized bureaucracy such as military absolutism fostered in Prussia. Antebellum planters actively participated and firmly believed in republican and parliamentary rather than monarchical and authoritarian government. Most planters—even those in older, more "aristocratic" areas like the Chesapeake Tidewater and the lower Mississippi Valley—adapted successfully, if unwillingly, to the democratization of political style and structure that occurred during the era of Jackson and the second (or Jacksonian) party system. From the mid-1830s to the mid-1850s the national two-party system flourished in all of the Southern states except Calhoun's South Carolina, although both Whig and Democratic politicians in the South were anxious to present their respective parties as "sound" on slavery.⁴⁵

But the antebellum South increasingly became a minority section within the federal Union, while pre-1848 East Elbia remained the preponderant heartland of the Prussian monarchy. The 1850s witnessed an accelerating decline of the South's electoral strength within the federal government, as the free states rapidly outstripped the slave states in both number and population. During the mid-1850s the sharpening sectional conflict over slavery's westward expansion into the territories and the rise of an antislavery party in the North served to destroy the Jacksonian party system; the Democratic Party seemed to be the South's only hope of protecting slavery within the Union. Even the "Democracy" was torn apart during the spring and summer of 1860 by fierce quarreling between the party's Northern and Southern wings over the status of slavery in the territories. Until Lincoln's election in November, however, most planters abjured secession and believed that loyalty to the Constitution—properly inter-

⁴⁴ On the importance to Old Prussian conservatism of a corporate organization of society in general and of the *Kreistage* and *Landtage* in particular, see Carl Wilhelm von Lancizolle, *Ueber Königtum und Landstände in Preussen* (Berlin, 1846), esp. 242–50, 461–70. On the county and provincial assemblies during *Vormärz*, see Hefter, *Die deutsche Selbstverwaltung im 19. Jahrhundert*, 129–32.

⁴⁵ See, in particular, William J. Cooper, Jr., *The South and the Politics of Slavery, 1828–1856* (Baton Rouge, 1978), esp. 50–74.

puted—was the surest defense of both their property rights (ensured by the Fifth Amendment) and their political power (dependent on the three-fifths rule and Southern representation in the Senate). The election of a Republican president, on a platform that most Southerners (and certainly most planters) saw as threatening the ultimate abolition of slavery, drove the seven states of the Lower South from the Union; and Lincoln's "coercive" call in April 1861 for seventy-five thousand troops to subdue the "rebellion" provoked the secession of four more states in the Upper South.

It is important to emphasize that the splits in the Southern electorate *before* the November presidential election—among supporters of Breckinridge, Bell, and Douglas—and the splits *after* the election returns—among separate-state secessionists and the several varieties of "cooperationists"—reflected disagreement over what political strategy would serve best to protect slavery, not disagreement over the fundamental necessity to preserve the institution. As Dwight Lowell Dumond demonstrated decades ago, "At the South, the members of all political parties were dedicated to the preservation of the institution of slavery."⁴⁶ In order to understand how planters and plantation slavery were so compatible with political democracy in the Old South, it is valuable to consider some geographic, demographic, and racial differences between the South and East Elbia.

Geographic and demographic conditions in the antebellum South provided many more opportunities for upward social mobility than could exist in *Vormärz* eastern Prussia. East Elbia contained much less uncultivated land than did the Southern states, and it did not have an expanding frontier. With a total land area not as large as that of Louisiana and Mississippi combined, East Elbia in 1849 supported a population nearly as large as that of all fifteen slave states in 1860.⁴⁷ The continuing availability of unsettled land in the Old South, in conjunction with the profitability of cotton-growing, gave whites opportunities for social as well as geographical mobility that were impossible in pre-1848 Prussia. Just as ambitious yeomen farmers could realistically hope to become small slaveholders, ambitious lesser slaveholders could strive to become gentlemen planters.⁴⁸ The antebellum planter class expanded both numerically and geo-

⁴⁶ Dwight Lowell Dumond, *The Secession Movement, 1860-1861* (1931; reprint ed., New York, 1973), 98. With respect to Georgia, Michael P. Johnson has mistakenly assumed that, in the election of delegates to the state secession convention on January 2, 1861, votes in favor of delegates committed to cooperative action among the slave states vis-à-vis the Republican North, as opposed to votes for delegates committed to separate-state secession, reflected in many instances a lack of commitment to the preservation of slavery; Johnson, *Toward a Patriarchal Republic: The Secession of Georgia* (Baton Rouge, 1977), 65-93. Cooperationists throughout the South included in their number wealthy slaveholders as well as nonslaveholders.

⁴⁷ The total land area of East Elbia was 90,283 square miles in the 1850s; that of the state of Mississippi, 47,240, and that of Louisiana, 45,177 (for a total of 92,417 square miles). East Elbia's population in 1849 was just over twelve million against twelve million three hundred thousand (white and black) for all fifteen slave states in 1860.

⁴⁸ Even though the ratio of slaveholders to the South's total free population declined slightly between 1790 and 1850 and then dropped substantially during the 1850s, the absolute number of slaveholders continued to climb until the Civil War—from 347,525 in 1850 to 384,884 in 1860, an increase of more than 10 percent. Moreover, the number of "planters" owning twenty to fifty slaves increased from 29,733 in 1850 to 35,623 in 1860, an increase of almost 20 percent. In 1860, 31 percent of white families in the eleven states that formed the Confederacy owned slaves. Otto Olson has explained that, "while 31 per cent may not appear large as a voting or even [an] isolated ownership statistic, it is enormous if . . . slavery is viewed as the economic founda-

graphically, while in *Vormärz* East Elbia the fixed number of increasingly expensive *Rittergüter* could only change hands among the wealthy upper crust.⁴⁹ Consequently, the number of Prussians who could hope to become part of landed Junkerdom was proportionately far smaller than the number of Southerners who could aspire to the status of gentlemen planters. If only for this reason, the planters enjoyed a much higher degree of popular legitimacy than did the Junkers.⁵⁰ Accordingly, the planters of older, more settled states had at least a threefold interest in securing new territory for the expansion of the plantation system: (1) to enlarge the South's representation in Congress by creating new slave states; (2) to guarantee a steady market for their surplus slaves, which would maintain and even increase the market value of the slaves they retained and, by exporting excess Negroes, reduce the possibility of a successful slave rebellion; and (3) to assure the continued availability of cheap, fresh land for generations of aspiring planters, whose aspirations were instrumental in legitimizing plantation society.⁵¹

Another crucial difference between the historical environments of the two landed elites—and perhaps the most important—was racial. The South was a racially bifurcated society in which the enslavement of Negroes and anti-Negro prejudice had fed on each other since the seventeenth century. But the Junkers never enjoyed the advantage of a labor force isolated by race from the population at large; they therefore had much more to fear than did the planters from an indigenous popular movement for democratic reforms. In the aftermath of 1848–49, Count Friedrich zu Dohna-Lauck of East Prussia wrote of “the lack of political judgment and the gullibility of the lower classes” and of the consequent “necessity for the complete exclusion of the working class and those not economically self-sustaining from any participation in the election of representatives of the country.” Dohna-Lauck saw the plutocratic three-class suffrage system de-

tion of an entire social system and the distribution of slaves is compared to analogous factors in a free society.” Olson pointed out that in 1949 only 2 percent of American families owned stock worth \$5,000 or more, an investment comparable to the ownership of one slave in 1860. Olson, “Historians and the Extent of Slave Ownership in the Southern United States,” *Civil War History*, 18 (1972): 111–12.

⁴⁹ Editor Carl Sprengel reported that in 1840 *Rittergüter* in eastern Pomerania, which had sold in the late 1820s for 50,000 *Taler*, were selling for 100,000 to 150,000 *Taler*; Sprengel, “Landwirtschaftlicher Bericht aus Hinterpommern. Anfang April 1840,” *Allgemeine Landwirtschaftliche Monatschrift*, 1 (1840): 144. Estate owner Lüdersdorf auf Christophlack reported that an estate in a neighboring East Prussian county (*Kreis*) had sold in 1834 for 11,000 *Taler*, in 1841 for 24,000 *Taler*, and in 1849 for 44,000 *Taler*; “Die landwirtschaftlichen Verhältnisse des Labiau Kreises,” 116. In the mid-1850s, one Prussian *Taler* was the equivalent of approximately \$0.70 in U.S. currency; I have computed this equivalence from figures in Georg Friedrich Kolb, *Handbuch der vergleichenden Statistik der Völkerzustands- und Staatenkunde* (Zurich, 1857), 127, 314.

⁵⁰ Alexis de Tocqueville made an incisive observation about the English aristocracy, which is also applicable to the planter elite. The English aristocracy, he wrote in 1833, was “not in the least founded on birth, but on the wealth that everyone can acquire”; and, “as everyone had the hope of being among the privileged, the privileges made the aristocracy not more hated, but more valued”; Tocqueville, “Journey to England,” in Tocqueville, *Journeys to England and Ireland*, ed. J. P. Mayer (New Haven, 1958), 59. In the words of Robert S. Starobin, “Between poor whites and slaveless yeomen on the one hand and planters on the other there was often a degree of hostility, but class conflict seldom became serious so long as economic opportunities for ambitious whites seemed open”; Starobin, *Industrial Slavery in the Old South* (New York, 1970), 6.

⁵¹ On the interests of Southern yeomen farmers and small slaveholders in securing new land for the expansion of slavery, see Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978), 33; and William R. Brock, *Parties and Political Conscience: American Dilemmas, 1840–1850* (Millwood, N.Y., 1979), 236–37, 274.

creed by the Manteuffel ministry in May 1849 as only the first step in the development of a corporate scheme of representation under which large landowners would enjoy their traditional pre-eminence.⁵² In the South black slavery practically eliminated for the planters the troublesome problem of a free agricultural working class whose poverty and exclusion from political participation inspired social and political agitation. Charleston's *Southern Quarterly Review* explained in 1853 that "the mass of mankind constitute the labouring class, and are, perforce, required to live by the sweat of the brow." Given this condition, the South was fortunate to have a working class composed of those "who are not pampered with false notions of their own claims to liberty and unnatural elevation."⁵³

Planters could preach and even practice political democracy among whites because their labor force was excluded by race from the political process and because nonslaveholding whites could and did share in the planters' racial contempt for the deprived and depraved Negro.⁵⁴ While the Junkers believed that a corporate structuring of political life was necessary to preserve their status as a landed elite, the planters could participate and even predominate in what has been called a *Herrenvolk* democracy—that is, a democracy for the master race.⁵⁵ Thus, Alabama Governor and Black Belt planter John A. Winston could declare in 1855, during his second inaugural address, that "the existence of a race among us—inferior by nature to ourselves, in a state of servitude—necessarily adds to the tone of manliness and character of the superior race."⁵⁶

IN CONSIDERING THE COMPARABILITY OF PLANTERS AND JUNKERS as "conservative" landed elites, some clarification of the historical concept represented by the slippery words "conservative" and "conservatism" is in order. Like all historical concepts, this one should be understood as an analytical rather than a descriptive tool, an ideal-typical construct that can help make sense of complex histori-

⁵² Dohna-Lauck, *Ueber die nothwendige Abänderung der beiden Wahlgesetze für die erste und die zweite Kammer: Eine Petition an die hohen Kammern* [July 30, 1849] (Königsberg, 1849), 5–6, 15, 18–33.

⁵³ [James Chestnut, Jr.] "The Destinies of the South," *Southern Quarterly Review*, 23 (1853): 196–97. (William Gilmore Simms was editor of the *SQR* from 1849 to 1854.) Several years later this same periodical published a review of George Fitzhugh's *Sociology for the South* (1854) and therein expressed the feelings of the vast majority of proslavery conservatives by refusing "to go so far as Mr. Fitzhugh and maintain that nothing but slavery can be right"; George Frederick Holmes, "Slavery and Freedom," *Southern Quarterly Review*, 29 (1856): 71. Holmes was a migrant academic, who gave a similar response to Fitzhugh, also emphasizing that Southern slavery was founded on the racial inferiority of blacks rather than on the merits of slavery *per se*, in another periodical; see Holmes, "Failure of Free Societies," *Southern Literary Messenger*, 21 (1855): 132. According to Eugene Genovese, however, "the notion that slavery was a proper system for all labor, not merely for black labor, . . . grew steadily as part of the growing self-awareness of the planter class," and Fitzhugh provided "a more rigorous and mature presentation of a line of thought which had been gaining steadily for years"; *The World the Slaveholders Made*, 130.

⁵⁴ See the suggestive observations in Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 380–86.

⁵⁵ Pierre L. van den Berghe, *Race and Racism: A Comparative Perspective* (2d ed., New York, 1978), 18, 29; George M. Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817–1914* (New York, 1971), 61–68; and Kenneth P. Vickery, " 'Herrenvolk' Democracy and Egalitarianism in South Africa and the U.S. South," *Comparative Studies in Society and History*, 16 (1974): 309–28.

⁵⁶ Winston's address was published in the *Richmond Whig*, January 4, 1856, with an editorial note commending the governor's "just views" to the paper's readers.

cal developments. As such, it transcends any specific historical context, but the use of such a construct carries the presumption that every manifestation of conservatism is the product of a particular historical milieu.⁵⁷ Its transcendence is based on the premise that being "conservative" entails defending the status quo before a "progressive" attack demanding change. Occasionally, individuals may exhibit a conservative disposition to distrust and may resist change for reasons whose explanation belongs more to the realm of personal psychology than to the sphere of historical analysis. But a historically significant manifestation of conservatism—a conservative movement—emerges only when members of a substantial social group or community are confronted by a serious "progressive" challenge to the values and institutions they have previously taken for granted. "Reactionary" ideas and sentiments can often serve conservative purposes, in that the reactionary's romantic glorification of a bygone era can have the effect of discrediting progressive change in the present.

An elitist variety of conservatism emerges when a highly influential and prestigious minority group faces a potent reformist threat to those established, "traditional" institutions and values that are essential to its favored position in society. An elitist conservative movement, like the progressive challenge that provokes it, is both a political and an intellectual phenomenon and gives rise to complex theoretical justifications of the status quo—that is, a multifaceted conservative ideology. Conservative ideologues, of course, constitute only a small proportion of the elite's members; indeed, some of these ideologues are well-educated "outsiders" who fulfill an intricate blend of social, material, and intellectual ambitions by identifying their lives and ideas with the elite's influence and prestige.

Proslavery conservatism in the South and Old Prussian conservatism in East Elbia developed in response to substantive reformist attacks on both the fundamental interests and the self-esteem of the planters and Junkers. In each case the progressive ideology was a mixture of democratic, free-labor, and humanitarian impulses, which shared an intolerance of planter "aristocrats" and Negro servitude in the South or of "feudal" Junkers and oppressed *Rittergut* labor in East Elbia. Although the conservative ideas advanced by proslavery and Old Prussian ideologues did serve as occasional weapons in political struggles to protect the concrete interests of the two landed elites, these ideas also helped satisfy the personal desires of many planters and Junkers for public vindication before the frequent invectives of progressive critics. A widespread proslavery movement in Southern thought and politics developed between the Missouri crisis of 1819–21 and the controversies over abolitionist mailings and petitions of 1835–37. During these years the planters' apprehensions that Congress's Northern majority would interfere with the expansion of slavery were augmented by their more in-

⁵⁷ My approach to the concept of conservatism has been influenced by Karl Mannheim, "Conservative Thought" (1927), trans. Paul Kecskemeti and Kurt Wolff in Mannheim, *From Karl Mannheim*, ed. Kurt Wolff (New York, 1971), 132–222; Sigmund Neumann, *Die Stufen des preussischen Konservatismus: Ein Beitrag zum Staats- und Gesellschaftsbild Deutschlands im 19. Jahrhundert*, Historische Studien, no. 190 (Berlin, 1930), esp. 7–12; Samuel P. Huntington, "Conservatism as an Ideology," *American Political Science Review*, 51 (1957): 454–73; and Hans-Gerd Schumann, "The Problem of Conservatism: Some Notes on Methodology," *Journal of Contemporary History*, 13 (1978): 803–17.

tense fears that abolitionist agitation would set free the demon of slave rebellion.⁵⁸ Old Prussian conservatism emerged in response to the "revolution from above" that the Junkers confronted during the Stein-Hardenberg Reform Era of 1807–22.⁵⁹ At this time the bureaucracy not only ended serfdom and the nobility's de jure monopoly over the ownership of *Rittergüter* but even threatened, albeit unsuccessfully, to undermine the Junkers' *Rittergut*-based combination of large estates with autocratic rights of local government. Since *Vormärz* Junkers reacted to the actual abolition of serfdom, while antebellum planters confronted only a threat to slavery, Old Prussian ideology included a pronounced reactionary strain that was absent from proslavery thought. Proslavery ideology, moreover, was much less overtly elitist, and much more regional, than was its Old Prussian counterpart, since planters and slavery enjoyed a higher level of popular legitimacy than did Junkers and privileged *Rittergüter*. It was no accident, however, that both the Southern proslavery and the Old Prussian ideologies first unfolded in older states or provinces with relatively entrenched planter or Junker elites—South Carolina and Virginia, Brandenburg and Pomerania.

The comparability of the conservatism of antebellum planters and *Vormärz* Junkers does not rest solely on the progressive challenge to the status quo that each landed elite perceived. It also rests on the analogous economic, social, and political institutions—the "traditional" institutions—that, in the final analysis, the two landed elites defended: the plantation with its enslaved work force and the *Rittergut* with its dependent laborers. The continuing economic vitality of these two institutions and their owners served to prolong the exclusion of large segments of the American and Prussian populations from civil and political equality; and the proprietor of a plantation or *Rittergut* was entitled to wield a species of private authority that is incompatible with the democratic nation-state.⁶⁰ If the United States and the Kingdom of Prussia during the first half of the nineteenth century were public law states, then the plantation and *Rittergut* were miniature private law states within which the planter and Junker exercised

⁵⁸ By the mid-1830s some Southerners—South Carolinians in particular—had developed the argument that slavery was a "positive good" rather than a "necessary evil"; William Sumner Jenkins, *Pro-Slavery Thought in the Old South* (1935; reprint ed., Gloucester, Mass., 1960), 65–81; and William W. Freehling, *Prelude to Civil War: The Nullification Controversy in South Carolina, 1816–1836* (New York, 1965), 76–82, 327–33. Both arguments, "positive good" and "necessary evil," had their adherents among Southern planters during the three decades before the Civil War, although the former certainly dominated public discussion of the slavery issue below the Mason-Dixon Line during the 1850s. As William Cooper, Jr., has observed, it is impossible to say how many Southerners "accepted or believed" the argument that slavery was a "positive good" instead of the argument from necessity. "In practical terms," however, "it mattered little whether or not a southerner accepted or rejected the argument that slavery was a positive good," since those who favored the argument from necessity also insisted that the South must deal with its peculiar institution free from outside interference; *The South and the Politics of Slavery*, 60–63.

⁵⁹ The term "Old Prussian," a translation of the adjective *altpreussisch*, is used in Rosenberg, *Bureaucracy, Aristocracy, and Autocracy*, 221–28. During and after the period of the Stein-Hardenberg reforms, *altpreussisch* became a historical synonym for the adjectives *hochkonservativ* ("high conservative") and *ständisch-konservativ* ("corporate-conservative"). Ernst Huber has used these two terms exclusively in discussing conservatives in the 1840s, those whom I call Old Prussian; Huber, *Deutsche Verfassungsgeschichte seit 1789*, vol. 2: *Der Kampf um Einheit und Freiheit, 1830 bis 1850* (Stuttgart, 1960), 331–39.

⁶⁰ On the incompatibility of the planter's or Junker's private autocratic authority with the democratic nation-state, see William H. McNeill, *The Rise of the West* (New York, 1965), 815–16; and Reinhard Bendix, *Nation-Building and Citizenship: Studies in Our Changing Social Order* (rev. ed., Berkeley and Los Angeles, 1977), 122, 128.

almost sovereign authority. From this perspective, planter apologists defending slavery were just as conservative, in a qualitative sense, as were Junker apologists defending the authority of the *Rittergut* owner over his menials. Plantations, planters, and proslavery conservatism in the South and *Rittergüter*, Junkers, and Old Prussian conservatism in East Elbia constituted the major breakwaters in the United States and Prussia during the nineteenth century against what can be called the liberal main currents of Western thought and politics since the American and French Revolutions—that is, against the increasingly popular arguments for and the gradual and halting movement toward civil and political equality, which in the twentieth century has become a campaign for social and economic equality as well.⁶¹

Proslavery and Old Prussian ideologues made extensive use of conservative principles associated with Edmund Burke and his *Reflections on the Revolution in France* (1790). “Burkean principles” were exceedingly well suited to an ideological defense of both the plantation with its slaves and the Junker estate with its dependent cottagers and servants. Although political scientist Louis Hartz has argued that the Old South’s invocation of Burke was fraudulent, because antebellum conservatives defended not feudalism but slavery,⁶² the British society that Edmund Burke defended was far from feudal.⁶³ On the contrary, with the possible exception of the Netherlands, England enjoyed the most “liberal” and “capitalistic” civilization in eighteenth-century Europe.

Burkean conservatism can be reduced to two fundamental principles. The first is the necessity for historical continuity, which entails respect for established

⁶¹ My use of the terms civil, political, and social equality follows that of T. H. Marshall, “Citizenship and Social Class” (1949), in Marshall, *Class, Citizenship, and Social Development: Essays by T. H. Marshall* (Garden City, N.Y., 1965), 78. I suppose an ideal-typical nineteenth-century conservative in the West would have reacted against the manifold effects of what Eric Hobsbawm has identified as the “dual revolution” of the late eighteenth century—that is, “the French Revolution of 1789 and the contemporaneous (British) Industrial Revolution”; Hobsbawm, *The Age of Revolution: Europe, 1789–1848* (London, 1962), xv. In their historical interaction, the two facets of this dual revolution have been the primary agents of what has been called the process of modernization; see Reinhard Bendix, “Tradition and Modernity Reconsidered,” in his *Embattled Reason: Essays on Social Knowledge* (New York, 1970), 294–96. Bendix has written more recently that the “term modernization is applied best where nonindustrial ways of life and hierarchical social orders are threatened by industrial ways and egalitarian social norms”; Bendix, *Kings or People: Power and the Mandate to Rule* (Berkeley and Los Angeles, 1978), 12. It can be argued that antebellum planters and Vormärz Junkers were “antimodern” according to Bendix’s ideal-typical formulation; but these two landed elites felt themselves to be—and in fact were—far less threatened by industrialization than by the democratic and egalitarian impulses that were a legacy of the American and French Revolutions. The cooperation between Junkers and industrialists that evolved in Prussia after 1848 shows that conflict between agrarian and industrial elites in the nineteenth century was neither intrinsic nor irrepressible. The major obstacle to the development of such peaceful cooperation between antebellum Southern planters and Northern industrial interests was the accelerating controversy over slavery’s spread into the western territories. As Barrington Moore has pointed out, plantation slavery was not “an economic fetter upon industrial capitalism,” but rather “an obstacle to a political and social democracy”; *Social Origins of Dictatorship and Democracy*, 112. Unfortunately, however, Moore has ignored the important racist components of the North’s opposition to the expansion of slavery.

⁶² Hartz, *The Liberal Tradition in America*, 146–47.

⁶³ According to Harold Perkin, “Feudalism as a social system in England disappeared with the fourteenth- and fifteenth-century commutation of labor services”; Perkin, *The Origins of Modern English Society, 1780–1880* (Toronto, 1969), 25. R. S. Neale has lent support to Perkin’s statement in observing that by the early sixteenth century English agriculture “was largely a specialised and market-oriented agriculture, literally dominated in parts of the west country by large-scale capitalist farming.” The English landowner, Neale has suggested, “experienced a substantial dose of bourgeoisie well before the bourgeoisie existed.” Neale, “The Bourgeoisie, Historically, Has Played a Most Revolutionary Part,” in Eugene Kamenka and R. S. Neale, eds., *Feudalism, Capitalism, and Beyond* (London, 1975), 92, 94.

property rights and regard for tradition and experience over abstract reason and theory as guides to social and political policy; this regard for tradition and experience is imperative because human nature is too sinful and human reason too flawed to be relied upon as the fonts of wisdom. The second principle is the belief that the hierarchical structure and inegalitarian values of established society are in accord with divine law and that, consequently, any natural law doctrine of inherent human rights is false.⁶⁴ These Burkean principles were no more fraudulent when Thomas R. Dew or William Harper invoked them to defend plantation slavery than when Adam Müller or Carl Ernst Jarcke invoked them to defend the privileges of *Rittergut* owners. Although an explanation of the ways in which proslavery and Old Prussian ideologues elaborated upon Burkean thought is not feasible here, an examination of their use of Burkean principles with respect to inherent human rights is instructive. Scholars on both sides of the Atlantic have suggested that the basic characteristic of all nineteenth-century conservative thought in the West was its opposition to the Enlightenment doctrine of inherent and inalienable human rights.⁶⁵

Antebellum planters, apart from a small number of consistently anti-democratic ideologues like Abel P. Upshur of Virginia, talked publicly in biracial terms about the virtues of popular sovereignty and democratic republicanism for whites and the benefits of patriarchal servitude for blacks. Because proslavery ideologues by and large denied the validity of natural rights political theory only with respect to the Negro race, the proslavery argument against natural rights was burdened with ideological convolutions that had no parallel in Old Prussian thought. For *Vormärz* Junkers sovereignty resided in their Hohenzollern monarch, even though he was expected to rule in many matters with the advice and cooperation of the county and provincial assemblies dominated by *Rittergut* owners. Without a viable democratic or parliamentary tradition in Prussia prior to 1848, Old Prussian conservatives associated natural law and social contract theories—and the constitutional, representative forms of government that these ideas had spawned—with the subversive foreign influences of Jean Jacques Rousseau and the French Revolutions of 1789 and 1830.⁶⁶ Proslavery conservatives were faced with the more ticklish task of discrediting, or at least qualifying, the natural rights principles embodied in their own Declaration of Independence from England, written by a Virginia planter who decried the injustice of holding men as property and who hoped that both slavery and Negroes might someday be expunged from America. As Edmund Ruffin of Virginia noted in October 1858, Jefferson “has done great harm, by the countenance which his opinions on this subject, & even the words of the Declaration

⁶⁴ This summary is based on my own reading of Burke's *Reflections on the Revolution in France*. Also see Herbert Dinkel, “Die konservativ Bewegung in Deutschland,” in Friedrich Schultes, ed., *Geschichte* (Frankfurt a/M., 1973), 125.

⁶⁵ Mannheim, “Conservative Thought,” 174; and Carl L. Becker, *The Declaration of Independence: A Study in the History of Political Ideas* (New York, 1942), 255–58.

⁶⁶ See, for example, Carl Ernst Jarcke, “Revolution und Absolutismus,” in his *Vermischte Schriften*, 3 vols. (Munich, 1839), 1: 174–75. This essay was originally published in 1833 in the *Berliner Politisches Wochenblatt*, founded by Old Prussian conservatives in 1831 in response to the Revolution of 1830 in Paris. Jarcke was the periodical's first editor, 1831–32.

of Independence, have afforded to the anti-slavery fanatics of the present times." Some proslavery ideologues insisted that the slaveholding author of the Declaration of Independence had simply neglected to make the obvious distinction between the unbounded rights and freedoms possible in a state of nature and the social restraints and obligations incumbent upon any individual living in an organized community. To Jefferson, wrote William Gilmore Simms of South Carolina, "the distinction was clear between the conventional or political and the natural rights of man."⁶⁷ Since Jefferson's will manumitted only five of his nearly two hundred and seventy slaves,⁶⁸ the argument was eminently plausible. This tactic was, moreover, logically compatible with the conviction that the Negro was inherently, racially inferior to his Caucasian master, a belief that Jefferson seems to have shared. Since blacks were inferior, they could never intermingle with whites on the basis of civil, political, or social equality without causing great harm to both races. Consequently, severe restraints and obligations—slavery—had to be imposed on blacks so that they could live in white society.

Another proslavery tack, a different course to the same end, simply reconstituted the concept of natural law inherited from the Enlightenment, making white enslavement of blacks compatible with the "law of Nature." University of Virginia law professor James Philemon Holcombe maintained, "African Slavery in the United States is consistent with Natural Law, because if all the bonds of public authority were suddenly dissolved, and the community called upon to reconstruct its social and political system, the relations of the two races remaining in other respects unaltered, it would be our right and duty to reduce the negro to subjection."⁶⁹ This reformulation of "natural law," justifying the reduction of a portion of the community to "subjection," found striking correspondence in the thought of Carl Ludwig von Haller, a Swiss-German patrician whose political philosophy exercised a pervasive influence on Old Prussian ideologues. Contrary to the suppositions of social contract thinkers, wrote Haller in his multi-volume treatise on the "restoration" of political science begun in 1816, men have never left the state of nature, which is "the eternal, unchanging order of God" by which "the more powerful person rules, must rule, and will always rule." All social relations, said Haller, are reciprocal relationships of authority and dependence: to the strong and independent belong "authority and freedom," to the weak and needy "a greater or lesser degree of dependence or servitude." Haller's appeal to Prussian Junkers was further enhanced by his notion that every landed estate constituted a small "patrimonial state," whose proprietor wielded a degree of monarchical and protective authority over his loyal dependents comparable to that of a prince over his subjects.⁷⁰ But Haller and his

⁶⁷ Ruffin, *The Diary of Edmund Ruffin*, ed. William Kauffman Scarborough, 1 (Baton Rouge, 1972): 238; and [Simms (?)] "Thoughts on Slavery," *Southern Literary Messenger*, 4 (1838): 742.

⁶⁸ William Cohen, "Thomas Jefferson and the Problem of Slavery," *Journal of American History*, 56 (1969): 519.

⁶⁹ Holcombe, *An Address Delivered Before the Seventh Annual Meeting of the Virginia Agricultural Society, November 4th, 1858* (Richmond, Va., 1858), 4. For a broad analysis of the rejection of natural rights theory in proslavery thought, see Jenkins, *Pro-Slavery Thought in the Old South*, 125-40.

⁷⁰ Haller, *Restauration der Staats-Wissenschaft oder Theorie des natürlich-geselligen Zustands*, 6 vols. (2d ed., Winterthur, 1820-25), 1: 375, 340-41, 351, 6: 561-62, 2: 13-17, 57-60. Carl Ernst Jarcke's essay on the state of nature,

Old Prussian disciples, unlike proslavery conservatives in the South, had no cause to translate the distinction between the strong and the weak, the estate owner and his laborers, into a racial dichotomy.

THE HISTORICAL DEVELOPMENT AND CHARACTER of proslavery conservatism in the South and its Old Prussian counterpart in East Elbia cannot be properly understood apart from a fluctuating tension within planter and Junker ranks between what can be termed pragmatic (that is, interest-oriented and realistic) and idealistic (that is, metaphysical and romantic) defenses of the status quo. To some extent, of course, this tension reflected an apparently universal characteristic of human nature, in that the same individual is frequently torn between principled and opportunistic motives. At the same time, human nature seems to insist in the long run upon an inner reconciliation between principle and practice, in the interests of a peaceful conscience. For many planters and Junkers this reconciliation was achieved through religion. The idea that the hierarchical and authoritarian relations on the plantation or *Rittergut* were a divinely sanctioned arrangement, ordained by God to regulate the affairs of sinful men, was fundamental to the conservative arguments advanced by religious apologists for Southern and East Elbian society.⁷¹ Such apologists saw relations on the plantation or *Rittergut* as having been instituted by God to assure harmonious, mutually beneficial interaction between men endowed with unequal mental and physical capacities—between the weak and the strong. Again, there was no reason in East Elbia to define the weak and the strong in racial terms, at least prior to the Junkers' growing reliance on seasonal labor from Poland during the last quarter of the nineteenth century.

The argument for divine approval of relations on the plantation or *Rittergut* struck a responsive chord in many a planter and Junker heart not merely because the gentleman farmer thought it could be an effective agent of social control when preached to his laborers but also because it fit logically and comfortably into the regional styles of pietistic Protestantism that won so many converts among the two landed elites during the first third of the nineteenth century. Both Southern evangelical Protestantism and Lutheran Neo-Pietism stressed the doctrine of original sin, thereby accentuating the inherent imperfection and inadequacy of the human condition, whether individual or collective. In so doing, these faiths denigrated the optimistic notion that man could achieve self-fulfillment through the rational manipulation of his social and physical environment. While pietistic Protestantism in the South and East Elbia counseled benevolent, humane treatment of slaves and estate laborers, its theological focus was on

which was originally published in the *Berliner Politisches Wochenblatt* in 1836, is a clear statement of Haller's ideas; see Jarcke, "Über die Entstehung des Staats durch die Natur," in his *Vermischte Schriften*, 3: 32-64.

⁷¹ For representative "conservative" sermons, see Benjamin M. Palmer, "Thanksgiving Sermon, Delivered in the First Presbyterian Church, New Orleans, on Thursday, November 29, 1860," in B. M. Palmer and W. T. Leacock, *The Rights of the South Defended in the Pulpits* (Mobile, 1860), 1-11; and C. W. Hoffmann, *Predigt über Jerm. 18. 7-14. am 18. März 1849 in der evangelischen Kirche in Gross-Strehlitz gehalten und dem vaterländischen Vereine hiesigen Ortes auf Verlangen zum Druck überlassen von Pastor C. W. Hoffmann* (Gross-Strehlitz, 1849).

transforming the individual's personal relation to God in heaven, not on restructuring society for the promotion of the Kingdom of God on earth.⁷²

The tension in planter and Junker ranks between pragmatic and idealistic conservatism was not completely resolved by religion, however, because this tension was in part a manifestation of the divergence of the visionary designs of most conservative intellectuals from the more mundane concerns of most planters and Junkers.⁷³ In fact, relatively few of these agrarian entrepreneurs were much interested in the formulation of systematic philosophical defenses of their own positions. Their predominantly rural or military experiences did not dispose or equip them to evaluate their interests and values in theoretical categories; and, as owners of large estates with sizable work forces, they did not find ideological subtlety to be a necessary virtue or systematic ethics a pressing concern. The small but vocal contingent of idealistic apologists comprised men of ideas whose temperament or education inclined them to attach great significance to a degree of moral rigor and philosophical consistency that (as their own lives so often testified) is far more easily attained on the printed page than in personal or public affairs. To these intellectuals the success of the plantation or *Rittergut* as an agricultural business was less important than its embodiment of the ideal of a patriarchal and Christian community where social relations manifested less exploitation and more humanity than in the depersonalized world of urban commerce and industry. Their perceptions of the world were molded more by abstract ethical principles than by the considerations of economic and political feasibility that impelled most planters and Junkers.

Relations between idealistic intellectuals and the pragmatic majority of planters and Junkers were, as might be expected, ambiguous. On the one hand, the intellectuals tended to "romanticize" established institutions: their musings elevated life on the plantation and *Rittergut* to a noncommercial, paternalistic, and even religious level of meaning and quality. Since most people prefer to see their own behavior and motives cast in the best possible light, quite naturally pragmatic, nonspeculative planters and Junkers were at times disposed to esteem the writings and ideas of ideologues like William Gilmore Simms or Hermann Wagener, Nathaniel Beverly Tucker or Ludwig von Gerlach. On the

⁷² On pietistic Protestantism in the South, see John R. Boles, *The Great Revival, 1787-1805: The Origins of the Southern Evangelical Mind* (Lexington, Ky., 1972), esp. 140-42; and Donald G. Mathews, *Religion in the Old South* (Chicago, 1977), esp. 62-80. On Lutheran Neo-Pietism in East Elbia, see Neumann, *Die Stufen des preussischen Konservatismus*, 87-88, 109-10; and Bigler, *The Politics of German Protestantism*, 46-50, 125-55.

⁷³ For valuable insights into the relations of idealistic proslavery ideologues and pragmatic planters, see John R. Welsh, "William Gilmore Simms, Critic of the Old South," *Journal of Southern History*, 26 (1960): 201-14; David Donald, "The Proslavery Argument Reconsidered," *ibid.*, 38 (1971): esp. 12-17; Drew Gilpin Faust, *A Sacred Circle: The Dilemma of the Intellectual in the Old South, 1840-1860* (Baltimore, 1977), esp. 59, 107-31, 147-48; and Robert J. Brugger, *Beverly Tucker: Heart over Head in the Old South* (Baltimore, 1978), esp. 107-10, 198-209. For comparably valuable insights into the relations between idealistic Old Prussian ideologues and pragmatic Junkers, see Erich Jordan, *Die Entstehung der konservativen Partei und die preussischen Agrarverhältnisse von 1848* (Munich, 1914), esp. 254-74; Mannheim, "Conservative Thought," 183-86; Neumann, *Die Stufen des preussischen Konservatismus*, esp. 54, 66, 111; and Hans-Joachim Schoeps, *Das andere Preussen: Konservative Gestalten und Probleme im Zeitalter Friedrich Wilhelms IV* (3d ed., Berlin, 1964), esp. 54-62. There were, of course, idealistic intellectuals in both the South and East Elbia who were also efficient, even innovative, owners of plantations and *Rittergüter*: Edmund Ruffin and James Henry Hammond in the South and Ludwig von der Marwitz and Ernst von Senfft-Pilsach in Prussia, to name but a few.

other hand, the intellectuals frequently reproached more mundane planters and Junkers for their failure to live up to the ideal standards of behavior set for them. The idealists, in defending plantation slavery and the Junkers' *Rittergut*-based privileges as an abstract good, were also waging an ideological crusade to purge the economic and political life of the South and East Elbia of selfish expediency, moral corruption, and intellectual vapidness.

But the distinction between idealistic intellectuals and pragmatic gentlemen farmers was more blurred in the South than in East Elbia. The planter class included a higher percentage of educated professionals—and of lawyers in particular—than did landed Junkerdom. The planters' long participation in competitive electoral politics, unlike the Junkers' experience with monarchical, military, and bureaucratic absolutism, meant that the planters were more articulate public spokesmen for their own values and interests. At the same time, antebellum party politics was less conducive to maintaining ideological purity and consistency than was the nonparliamentary, authoritarian political environment of *Vormärz* Prussia.⁷⁴ Most important, in defending Negro slavery before the Southern electorate, the planters could assume a simpler and more popular conservative stance than could Junkers in defending their more complex and less popular network of aristocratic privileges. For these and other reasons, the pragmatic-idealistic tension had a less dramatic and divisive impact on the history of the Old South than on the history of *Vormärz* Prussia, as the planters' behavior during the secession crisis of 1860–61 and the Junkers' behavior during the revolutionary crises of 1848–49 demonstrate.

The tension within Junker ranks between pragmatic and idealistic modes of conservatism first became apparent during the Stein-Hardenberg revolution from above. In the province of Brandenburg Karl Reichsgraf Finck von Finckenstein and Ludwig von der Marwitz (aided by the intellectual talents of Adam Müller) were opposed in principle to the reforms, while most of their peers pursued an adaptable conservative policy shaped primarily by considerations of economic self-interest.⁷⁵ Although the pragmatic-idealistic tension subsided with the attenuation of the reform movement during and after the Wars of Liberation against Napoleon, and remained relatively subdued for most of *Vormärz*, it exploded into open conflict during the revolutions of 1848–49 "from below." During the spring and summer of 1848, as the confused and indecisive Frederick William IV attempted to conciliate the forces of revolution, the Junkers were temporarily shorn of absolutist protection; at the same time they confronted radical demands from the first democratically elected state assembly in Prussia's history.⁷⁶ The idealistic impulse in Junker conservatism was represented by a

⁷⁴ For suggestions to this effect, see David M. Potter, *The Impending Crisis, 1848–1861*, ed. Don E. Fehrenbacher (New York, 1976), 226; and Mannheim, "Conservative Thought," 138–39.

⁷⁵ See, in particular, Gerhard Ramlow, *Ludwig von der Marwitz und die Anfänge konservativer Politik und Staatsanschauung in Preussen*, *Historische Studien*, no. 190 (Berlin, 1930), 78–80.

⁷⁶ The inner conflict between pragmatic and idealistic conservatism abated somewhat after Frederick William IV finally stiffened in November 1848, relying primarily on the strength and loyalty of the Prussian army to inaugurate a decade of political reaction. The tension remained in the background until Bismarck's *realpolitisch* and democratic methods of unifying Germany between 1866 and 1871—for example, his war against

pietistic-legitimist circle centered in the "Christian-Germanic" court clique (*Kamarilla*) close to the king; its principal spokesman was the jurist Ernst Ludwig von Gerlach. Using the new conservative newspaper *Kreuzzeitung* as a forum, Gerlach and his associates insisted that landed Junkerdom must retain a legally privileged position in state and society as the divinely ordained intermediary power between the monarch and his people. This point of view did not correspond to the outlook of the pragmatic Junkers who dominated the "General Assembly for the Protection of the Interests of Landed Property and for the Promotion of the Welfare of All Classes of People," or "Junker Parliament," which met in Berlin in August to protest the "expropriatory" agrarian reforms (including an end to the tax-exempt status of *Rittergüter* in several provinces) proposed by the Camphausen-Hansemann ministry. Gerlach attended the meetings, and he argued eloquently that *Rittergut* owners must not give up their patrimonial rights of local government, because these privileges were obligations imposed on the landed aristocracy by God. But the great majority of the two to three hundred Junkers who sat in the Junker Parliament were ready to sacrifice aristocratic privileges like patrimonial justice and police power and were willing to commute the remaining "feudal" services of lesser peasants into long-term monetary payments, if such concessions would serve to defuse popular unrest and radicalism. These men, concerned primarily about the security of their property rights, were prepared to accept the political revolution if a social revolution could be averted—averted in part by appealing to the fears of other property owners (such as well-to-do peasants and the urban middle classes) about the possibility of socialist expropriation. The septuagenarian pragmatist Ernst von Bülow-Cummerow concluded his opening address to the Junker Parliament by pointing out, "Material interests have an importance that outweighs all others; insofar as we follow them, we will always have firm ground under our feet. Let us call forth all our powers to further them, and, if we succeed, we can count on the concurrence of the great mass of people and of all propertied classes."⁷⁷

Austria and his sponsoring of a democratic suffrage for elections to the Reichstag of the North German Confederation in order to win popular support throughout Germany for a Prussian solution to the German problem—outraged the legitimist and corporatist principles of a few die-hard Old Prussian idealists like Ludwig von Gerlach and Hans Hugo von Kleist-Retzow. See Gerhard Ritter, *Die preussischen Konservativen und Bismarcks deutsche Politik, 1858–1876* (Heidelberg, 1913), 184–89, 220–21, 246–50; and Otto Pflanze, *Bismarck and the Development of Germany: The Period of Unification, 1815–1871* (Princeton, 1963), 331–34. But in response to Bismarck's proposed reforms of county government—the *Kreisordnung* of 1872, which threatened to undermine the *Rittergut* owner's traditional control over local government through a "mixture of self-government from below and bureaucratic control from above"—idealistic conviction and pragmatic self-interest coincided among Junkers more closely than they had since the Wars of Liberation against Napoleon; Robert M. Berdahl, "Conservative Politics and Aristocratic Landholders in Bismarckian Germany," *Journal of Modern History*, 44 (1972): 7–14.

⁷⁷ Both Gerlach's and Bülow-Cummerow's speeches were originally published in the *Kreuzzeitung* (*Neue Preussische Zeitung*), no. 44 (August 20, 1848), and no. 45 (August 22, 1848), *Beilagen*. Gerlach's speech is reprinted in Gerlach, *Aufzeichnungen aus seinem Leben und Wirken, 1795–1877*, ed. Jakob von Gerlach, 1 (Schwerin, 1903): 540–41, and in Hellmut Diwald, ed., *Von der Revolution zum Norddeutschen Bund: Aus dem Nachlass von Ernst Ludwig von Gerlach*, 1 (Göttingen, 1970): 53–54. Bülow-Cummerow's speech is reprinted in Hans Fenske, ed., *Vormärz und Revolution, 1840–1849* (Darmstadt, 1976), 332–40. For an excellent account of the Junker Parliament, see Klaus Klatte, "Die Anfänge des Agrarkapitalismus und der preussische Konservatismus" (D.Phil. dissertation, Hamburg University, 1974), 247–72.

The intensity of Southern proslavery conservatism generally reflected the intensity of the North-South sectional conflict that spawned it;⁷⁸ and, aside from a brief lull immediately after the Compromise of 1850, the intensity of both increased from the annexation of Texas in 1845 to the Civil War. But the idealistic-pragmatic tension in the proslavery movement never developed into a major rift. Idealists and pragmatists never disagreed over the fundamental need to maintain the institution of slavery as a rigid system of labor and social control, while in East Elbia principled and opportunistic conservatives parted company in 1848 over the question of preserving the formal legal authority of a Junker over his work force.

What conditions and circumstances present in the South and absent from East Elbia prevented the pragmatic majority of planters from considering a substantive change in their peculiar institution prior to the Civil War? One such condition was demographic. In eastern Prussia the rural population had increased since the mid-eighteenth century, particularly after 1815. By the 1830s and 1840s the region had a labor surplus. Although later drained away both by migration to the industrial cities west of the Elbe and by emigration overseas, that surplus temporarily gave *Vormärz* Junkers an abundance of cheap labor.⁷⁹ In the more sparsely settled South the demand for plantation labor, especially on the southwestern frontier, almost always exceeded the supply; and, without the control over their work force provided by chattel slavery, planters could not be sure of a sufficient supply of suitable gang labor. Black slaves also represented, in addition to a work force, a huge capital investment that the Junkers never had to make: slaveholders who merely kept their chattel alive during the 1850s made a respectable profit from capital gains alone. Planters were understandably concerned, therefore, about the threat to confidence in the future of slavery posed by the prospect of interference from a federal government dominated by the antislavery North.⁸⁰ Perhaps most important, antebellum planters refused to consider seriously even a gradual modification of slavery because of their deep-seated racial fears—manifested with painful clarity in their diatribes against abolitionism—about the general societal consequences of losing their virtually absolute legal control over several million Negro slaves. Howell Cobb, a prominent cotton planter from Houston County, Georgia, virulently attacked the alleged African descendants of sinful Ham in *A Scriptural Examination of the Institution of Slavery* (1856). After acknowledging that some “hard masters” committed illegal “excesses” in punishing their slaves, Cobb maintained, “We have to control a race of human beings who are under the influence of the most de-

⁷⁸ A notable exception to this rule was Thomas R. Dew's “Review of the Debate in the Virginia Legislature of 1831–32,” *Political Register*, 2 (1833): 769–831, reprinted in *The Pro-Slavery Argument, as Maintained by the Most Distinguished Writers of the Southern States* (Charleston, S.C., 1852). Dew, a professor at, and later president of, William and Mary College, wrote this treatise in 1832 in response to proposals introduced in the Virginia legislature of 1831–32 for the gradual emancipation and removal of Virginia's slaves; these proposals were, in turn, a response to the Nat Turner slave revolt in Southampton County during August 1831, which many Southerners attempted to blame on abolitionist agitation in the North.

⁷⁹ Schissler, *Preussische Agrarverhältnisse im Wandel*, 162–63, 198.

⁸⁰ Wright, *The Political Economy of the Cotton South*, 141–50. Wright has overstated his case, however, by explaining secession solely in economic terms and, thereby, implicitly downgrading the racial issues that concerned slaveholders and nonslaveholders alike.

praved and vicious propensities that ever marked the character of the debased individuals of which race seem to be incapable of redemption, either by kindness or severity."⁸¹

In short, pragmatic planters and idealistic intellectuals were united by their apprehensions for the security of slave property and by their profound racial fears about the consequences of emancipation—fears that magnified their terror at the thought of a retributive slave rebellion. These concerns, moreover, were those most likely to unite the white South in support of secession. In 1860–61, when planter and proslavery interests in eleven states concluded that the election and conduct of a "Black Republican" president posed a real and present danger to the stability, even the survival, of slavery, pragmatic and idealistic conservatives agreed that secession was the necessary, if regrettable, step to be taken—that a political revolution was necessary to avert a social one. William Henry Holcombe, a well-educated and reflective slaveholding physician in Louisiana, maintained that Lincoln's victory offered Southerners "the alternative" of secession and "a separate nationality" or "the Africanization of the South" within the federal Union.⁸² A more materialistic perspective characterized the official "Declaration" of the reasons for secession published by the Mississippi state convention: "We must either submit to degradation, and to the loss of property worth four billions of money, or we must secede from the Union framed by our fathers, to secure this as well as every other species of property."⁸³

WHAT DOES A COMPARISON OF ANTEBELLUM PLANTERS and *Vormärz* Junkers reveal about the "conservative" qualities of the planter class and proslavery thought? If, following Burke, respect for established property rights and regard for existing civil, political, and social inequalities are posited as archetypal conservative principles, then planters and proslavery ideology were perhaps even more conservative than were Junkers and Old Prussian ideology. But this overly simplistic perspective has two major shortcomings. First, it risks falling into the trap of an inflexible, homogenizing mode of social class analysis that is insensitive to the tension within both planter and Junker ranks between idealistic and pragmatic manifestations of conservatism. Even though, for a variety of historical

⁸¹ Cobb, *A Scriptural Examination of the Institution of Slavery in the United States, with Its Objects and Purposes* ([Perry] Ga., 1856), 121. This Howell Cobb, who became president of the Cotton Planters' Convention in Georgia in 1859–60, should not be confused with the famous Clarke County politician of the same name.

⁸² Holcombe, *The Alternative: A Separate Nationality or the Africanization of the South?* (n.p., 1860), reprinted in the *Southern Literary Messenger*, 32 (1861): 81–88. William Henry Holcombe—brother of James P., cited in note 71, above—was born in Lynchburg, Va., and graduated from the medical department of the University of Pennsylvania in 1847. From 1855 to 1862 he resided in Tensas Parish, La., but had close ties to Natchez, Miss., society. His manuscript diary in the Southern Historical Collection, University of North Carolina, Chapel Hill, and his several poems published in the *Southern Literary Messenger* in 1860 and 1861 testify to his reflective and idealistic bent.

⁸³ *An Address Setting Forth the Declaration of the Immediate Causes Which Induce and Justify the Secession of the State of Mississippi from the Federal Union and the Ordinance of Secession* (Jackson, Miss., 1861), 5. At least eighty-five of the one hundred members of the Mississippi secession convention were slaveholders; forty-four owned twenty or more slaves, and sixty-one owned ten or more slaves; Ralph A. Wooster, *The Secession Conventions of the South* (Princeton, 1962), 32.

reasons, the pragmatic-idealistic tension had less impact on antebellum planters than on contemporary Junkers, the historian of the Old South should nevertheless be cognizant of a divergence between the transcendent, romantic values and ideas that inspired a good many proslavery ideologues and the more mundane concerns and practical attitudes that characterized the majority of planters.

Second, reliance on Burkean principles as the touchstone of nineteenth-century conservatism in the West serves to reinforce the one-sided emphasis in both American and German historiography on the structure and content of proslavery or Old Prussian ideology as the measure of authentic conservatism among planters and Junkers. Historically, conservatism has been a political as well as an intellectual phenomenon; and to study ideological conservatism apart from its complex interaction with political conservatism is to present a partial, even distorted, view of the past. Political scientist Hans-Gerd Schumann has persuasively argued that "the real criterion of conservatism" is "the conscious political action taken by socially privileged classes, strata, or groups to safeguard the institutions in which their social position is embedded against attempts to alter the norms prevailing in the political domain, or polis."⁸⁴ Accordingly, the true measure of planter or Junker conservatism is to be found in the assiduousness with which each landed elite fought in the political arena to protect its plantation- or *Rittergut*-centered world. Perhaps Southern planters should again be adjudged more conservative than *Vormärz* Junkers, because they had a greater compulsion in 1860-61 to preserve the traditional character of the plantation as a hierarchical and authoritarian private law state than did most Junkers in 1848-49 with regard to their *Rittergüter*. The Junkers could afford to sacrifice some of the traditional character of the *Rittergut* as a private law state, since, unlike planters, they were privileged members of a larger hierarchical and authoritarian political order. Political flexibility, even more than the weakness and divisiveness of their foes, gave the Junkers a durability that history denied to the planters, whose inflexibility grew as their political influence over the federal government waned, until they finally determined to wager their survival on secession and civil war. Not only did the planters face stronger and more intractable opposition than did the Junkers, but the financial and racial characteristics of Southern slavery also engendered in planters far greater fears about the economic and social consequences of losing personal control over their labor force.

⁸⁴ Schumann, "The Problem of Conservatism: Some Notes on Methodology," 807.

In Defense of Servitude: American Proslavery and Russian Proserfdom Arguments, 1760–1860

PETER KOLCHIN

CONSERVATIVE IDEOLOGIES INSPIRE little interest today. A generation believing in the axiomatic virtues of freedom and equality—however they may be defined—finds little to celebrate in men who defended social systems predicated on inequality and the restriction of the freedom of a substantial proportion of the population. It is not surprising, therefore, that modern historians, while dissecting the lives, thoughts, and motives of abolitionists, have all but ignored the defenders of human bondage. Even historians of such bondage—whether in the United States South,¹ Russia,² or Latin America—have paid relatively little attention to the proponents of unfree labor and have commonly assumed in passing that proslavery and proserfdom arguments consisted largely of self-serving rhetoric unworthy of being taken seriously.

This article, based on research I have been conducting for a general comparison of American slavery and Russian serfdom, is a revised version of a paper presented at the annual meeting of the American Historical Association in December 1978. In making revisions I have benefited from helpful criticisms by Daniel Field, George M. Fredrickson, and two anonymous referees for the *American Historical Review*.

¹ The only comprehensive survey of American proslavery thought remains William Sumner Jenkins's *Proslavery Thought in the Old South* (Chapel Hill, N.C., 1935). For important recent exceptions to the general neglect of the subject, see Eugene D. Genovese, *The World the Slaveholders Made: Two Essays in Interpretation* (New York, 1969); George M. Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817–1914* (New York, 1971), chaps. 2, 3; Larry Robert Morrison, "The Proslavery Argument in the Early Republic, 1790–1830" (Ph.D. dissertation, University of Virginia, 1975); and Larry Edward Tise, "Proslavery Ideology: A Social and Intellectual History of the Defense of Slavery in America, 1790–1840" (Ph.D. dissertation, University of North Carolina, 1975). The following works deal with particular aspects of the defense of slavery: David Donald, "The Proslavery Argument Reconsidered," *Journal of Southern History* [hereafter, *JSH*], 37 (1971): 3–18; Drew Gilpin Faust, *A Sacred Circle: The Dilemma of the Intellectual in the Old South, 1840–1860* (Baltimore, 1977), 113–31; Ralph E. Morrow, "The Proslavery Argument Revisited," *Mississippi Valley Historical Review*, 48 (1961): 79–94; H. Shelton Smith, *In His Image, But . . . : Racism in Southern Religion, 1780–1910* (Durham, N.C., 1972), 129–64; William Stanton, *The Leopard's Spots: Scientific Attitudes toward Race in America, 1815–59* (Chicago, 1960); and Ronald T. Takaki, *A Pro-Slavery Crusade: The Agitation to Reopen the African Slave Trade* (New York, 1971).

² Russian historians have paid even less attention to the defense of serfdom than American historians have devoted to the defense of slavery. Most Russian writing on the subject appears in passing, in works primarily devoted to the opposition to serfdom. See V. I. Semevskii, *Krest'ianskii vopros v Rossii v XVIII i pervoi polovine XIX veka*, 2 vols. (St. Petersburg, 1888); M. T. Beliaevskii, *Krest'ianskii vopros v Rossii nakanune vosstanii E. I. Pugacheva (formirovanie antikrepostnicheskoi mysli)* (Moscow, 1965); and V. V. Mavrodin, *Klassovaia bor'ba i obshchestvenno-politicheskaia mys' v Rossii v XVIII v. (1725–1773 gg.)* (Leningrad, 1964). For an exception that focuses on the life of an important ideologue, see I. A. Fedosov, *Iz istorii russkoi obshchestvennoi mysli XVIII stoletia: M. M. Shcherbatov* (Moscow, 1967).

This assumption, and the ensuing neglect, are unfortunate for several reasons. First, they are based on the ahistorical judgment that people living in the past must share our values, that no one could *really* oppose such obviously supportable concepts as universal freedom and equality. Second, the defense of bondage is intrinsically interesting, especially in the United States, where proslavery spokesmen expounded the most imaginative—one scholar has suggested the only—conservative philosophy in the nation's history.³ But, most important, an examination of the arguments used to defend unfree labor can provide information about the masters and even about the labor systems they directed. Only by taking seriously their ideology can we take seriously their history.

This article examines two such conservative ideologies. The first half of the essay suggests a basic similarity of arguments used to defend unfree labor in Russia and the United States, and the second points to an important difference in the development of these arguments over time. Both the similarity and the difference have valuable implications.

IN 1859 BRITISH TRAVELER Charles Henry Pearson noted a widespread belief among Russian noblemen that the peasants "are not fit for freedom." Such sentiment puzzled him since, as he put it, "there is no difference of race between the governors and the governed."⁴ Pearson had made an observation that implicitly raised an important issue. Russian serfdom, unlike American slavery, was a system of bondage that was not based on any racial distinction between master and laborer. Surely, one might postulate, this difference would be reflected in the arguments used to defend the two institutions.

Certainly, racial arguments in defense of slavery were pervasive in the South. They were common, of course, among "scientific" racists and ethnologists such as Dr. Samuel A. Cartwright, who concluded that blacks, with their smaller brains, sloping foreheads, and deficient respiratory systems, were physiologically so different from whites that they were fit only for slavery.⁵ But racial arguments were also common among Southerners who defended slavery on other grounds. They were present early, as in eighteenth-century planter Landon Carter's casual remark in his diary that blacks "are devils and to make them otherwise than slaves will be to set devils free," and late, as in the insistence of James H. Hammond of South Carolina that emancipation was impossible because "the doom of Ham has been branded on the form and features of his African descendants."⁶ Throughout the South, whites insisted that blacks were different, inferior, and suited for slavery.⁷

³ See Louis Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought since the Revolution* (New York, 1955), pt. 4, esp. 174, 176.

⁴ Pearson, *Russia, by a Recent Traveller: A Series of Letters* (1859; reprint ed., London, 1970), 22–23.

⁵ See Cartwright, "Slavery in the Light of Ethnology," in E. N. Elliott, ed., *Cotton Is King, and Pro-Slavery Arguments* (1860; reprint ed., New York, 1970), 689–727. On scientific racism, see Stanton, *The Leopard's Spots*; and Fredrickson, *The Black Image in the White Mind*, chaps. 2, 3.

⁶ Carter, *The Diary of Colonel Landon Carter of Sabine Hall, 1752–1778*, ed. Jack P. Greene, 2 vols. (Charlottesville, Va., 1965), 2: 1148–49; and Hammond, "Speech on the Justice of Receiving Petitions for the Abolition of Slavery in the District of Columbia," in Hammond, *Selections from the Letters and Speeches of the Hon. James H. Hammond, of South Carolina* (New York, 1866), 37–38.

⁷ On Southern racial consciousness, especially see Winthrop D. Jordan, *White over Black: American Attitudes*

What is striking, however, is that similar arguments were used to defend serfdom, even though master and serf were usually of the same national and racial origin.⁸ By the eighteenth century, Russian noblemen had come to regard themselves as inherently different from their peasants and the serfs as inherently incapable of freedom.⁹ This sense of distinction between lord and peasant deepened over the course of the eighteenth century; Peter I contributed to it by forcing noblemen to shave their beards and adopt Western European manners, but equally important was the natural cultural division between two very different social classes. In the hundred years before emancipation, nobleman and peasant inhabited such different worlds that the distinction between the two seemed as inherent—as “racial”—as the distinction between white and black.¹⁰

It is noteworthy, then, but perhaps not entirely surprising, that Russian noblemen invented many of the same kinds of racial arguments to defend serfdom that American slaveowners used to justify their peculiar institution. The arguments were not as elaborately worked out: one does not find tomes exploring the physiological differences between peasant and nobleman (although in the eighteenth century some noble spokesmen claimed that, whereas they had white bones, peasants had black bones¹¹). The basic assumptions, however, were similar; they were “racial” in that they were predicated on belief in inherent and immutable differences rather than in distinctions based on particular social or environmental conditions. Peasants were just as intrinsically lazy, childlike, and requiring of direction as were blacks. Thus, Prince M. M. Shcherbatov, the most persistent exponent of the nobility’s interests in the late eighteenth century, asserted that without strict supervision peasants would not perform agricultural labor because of their “voluptuousness” and “laziness.”¹² In 1802 author Nicholas M. Karamzin rebutted the environmentalist argument, sometimes advanced by foreigners, that Russian peasants were lazy *because* of serfdom; on the contrary, he insisted, “they are lazy from nature, from habit, from ignorance of the advantages of diligence.”¹³ Just as defenders of American slavery saw free

toward the Negro, 1550–1812 (1968; reprint: ed., Baltimore, 1969); and Fredrickson, *The Black Image in the White Mind*, chaps. 2, 3.

⁸ The lack of racial or national distinction between lord and peasant makes the comparison of Russian serfdom with American slavery especially fruitful and flies in the face of assumptions by some scholars that enslavement of one’s own people is virtually impossible. “What sets the slave apart from all other forms of involuntary labor is that, in the strictest sense, he is an outsider,” Moses I. Finley has written. “He is brought into a new society violently and traumatically; he is cut off from all traditional human ties of kin and nation and even his own religion.” Finley, “The Idea of Slavery: Critique of David Brion Davis’ *The Problem of Slavery in Western Culture*,” in Laura Foner and Eugene D. Genovese, eds., *Slavery in the New World: A Reader in Comparative History* (Englewood Cliffs, N.J., 1969), 260. This was clearly not the case, however, with Russian serfs, who, as numerous historians have pointed out, were essentially slaves. See, for example, Jerome Blum, *Lord and Peasant in Russia: From the Ninth to the Nineteenth Century* (Princeton, 1961), 468–69.

⁹ Some foreign travelers shared this view. “There is something of the Negro in the nature of a Russian,” wrote Frenchman Germain de Lagny; see his *The Knout and the Russians: or, The Muscovite Empire, the Czar, and His People*, trans. John Bridgeman (London, 1854), 154.

¹⁰ A. Romanovich-Slavatinskii, *Dvorianstvo v Rossii ot nachala XVIII veka do otmeny krestnogo prava* (St. Petersburg, 1870), 58–87; and P. K. Alefirenko, “Russkaia obshchestvennaia mysl’ pervoi poloviny XVIII stoletia o sel’skom khoziaistve,” in *Materialy po istorii zemledel’iia SSSR*, 1 (Moscow, 1952): 545–47.

¹¹ Romanovich-Slavatinskii, *Dvorianstvo v Rossii*, 71.

¹² Shcherbatov, “Razsuzhdenie o nyneshnem v 1778 godu pochti povsemestnom golode v Rossii, o sposobakh onomu pomoch’ i vpred predupredit’ podobnoe zhe neshchastie,” in his *Sochineniia kniazia M. M. Shcherbatova*, ed. I. P. Khrushchov, 1 (St. Petersburg, 1896): 631–32.

¹³ Karamzin, “Pis’mo sel’skago zhitelia,” in his *Sochineniia Karamzina*, ed. Aleksandr Smirdin, 3 (St. Peters-

blacks as an anomaly and insisted that all Negroes were better off as slaves,¹⁴ so too advocates of Russian serfdom pointed to their "free" (or state) peasants and maintained that they would be happier as serfs, under the protection of *pomeshchiki* (noble landowners). During his short reign (1796–1801), Emperor Paul was able to act on his belief in the desirability of distributing "all state peasants to *pomeshchiki*" by awarding generous grants of state lands and peasants to deserving noblemen; and he rejoiced in placing peasants under the supervision of "police masters," who would both care for them and guard the peace and security of the state.¹⁵

If racial arguments would seem to be particularly suited to the defense of black slavery, then surely "class" arguments would be the pre-eminent Russian defense of serfdom, for few Western societies in the late eighteenth and early nineteenth centuries had so many hereditary, legal class distinctions as did imperial Russia. And class arguments did abound. Russian noble spokesmen eagerly composed paeans to the virtue, honor, and service of the nobility, which they contrasted with the money-grubbing ways of the merchants.¹⁶ If merchants deserved scorn, peasants required care and supervision. "The principal right of a Russian nobleman is to be a *pomeshchik*," wrote Karamzin in an essay extolling his own patriarchal regard for his "people"; "his principal duty is to be a good *pomeshchik*."¹⁷ Serfdom enabled noblemen to care for and protect their well-meaning but ignorant charges. Playwright and poet Ivan Boltin emphasized the

burg, 1848): 573. For the prevalence of such views among seigneurs of other European countries, see Jerome Blum, *The End of the Old Order in Rural Europe* (Princeton, 1978), 45–46.

¹⁴ See, for example, the comments of Thornton Stringfellow: "Slavery is becoming, to this people [Negroes], so manifestly a blessing in our country that fugitives from labor are constantly returning to their masters again, after tasting the blessings, or rather the awful curse to them, of freedom in nonslaveholding States; and while I write, those who are lawfully free in this State, are praying our Legislature for a law that will allow them to become slaves." Stringfellow, "Statistical View of Slavery," in Elliott, *Cotton Is King*, 540. On Southern white distaste for free Negroes, see Ira Berlin, *Slave without Masters: The Free Negro in the Antebellum South* (New York, 1974), 316–80.

¹⁵ Paul I, as quoted in N. M. Druzhinin, *Gosudarstvennye krest'iane i reforma P. D. Kiseleva*, 1 (Moscow, 1946): 147. For other expressions of this viewpoint, see Shcherbatov, "Razsuzhdenie o nyneshnem v 1778 godu pochti povsemestnom golode v Rossii," 645–47; Karamzin, "Pis'mo sel'skago zhitelia," 574; A. I. Komissarenko, "Proekt vvedeniia lichnoi krepostnoi zavisimosti ekonomicheskikh krest'ian v Rossii v pervye gody posle sekularizatsii tserkovnykh imushchestv (60-e gody XVIII veka)," *Ezhegodnik po agrarnoi istorii Vostochnoi Evropy* 1970 g. (1977): 95–103; and "Obozrenie raspolozheniia umov i razlichnykh chastei gosudarstvennogo upravleniia v 1835 godu," in E. A. Morokhovets, ed., *Krest'iansko-dvizhenie 1827–1869 godov*, 1 (Moscow, 1931): 18. On attitudes toward state peasants and on their inadequate supervision, see Druzhinin, *Gosudarstvennye krest'iane i reforma P. D. Kiseleva*, 125–47.

¹⁶ The foremost noble opponent of merchant pretensions was Prince Shcherbatov, who during the 1760s, 1770s, and 1780s spent much of his public efforts clarifying the distinction between nobleman and merchant. See, for example, his "Razmyslenie o dvorianstve," in his *Sochineniia*, 219–68, and "Razsmotrenie o voprose—mogut li dvoriane zapisivat'sia v kuptsy," in his *Nizdannye sochineniia* (Moscow, 1935), 139–58. On Shcherbatov, see Fedosov, *Iz istorii russkoi obshchestvennoi mysli*; Marc Raeff, "State and Nobility in the Ideology of M. M. Shcherbatov," *American Slavic and East European Review*, 29 (1960): 363–79; and Joan M. Affrica, "The Political and Social Thought of Prince M. M. Shcherbatov, 1733–1790" (Ph.D. dissertation, Harvard University, 1966). On noble efforts to resist merchant encroachments during the last third of the eighteenth century, see Wilson Robert Augustine, "The Economic Attitudes and Opinions Expressed by the Russian Nobility in the Great Commission of 1767" (Ph.D. dissertation, Columbia University, 1969), 81–89; Beliauskii, *Krest'ianskii vopros v Rossii nakanune vosstania E. I. Pugacheva*, 87–88, 195–96, 239–43; Paul Duker, *Catherine the Great and the Russian Nobility: A Study Based on the Materials of the Legislative Commission of 1767* (Cambridge, 1967), 113–15, 129–30; and Robert E. Jones, *The Emancipation of the Russian Nobility, 1762–1785* (Princeton, 1973), 64–67, 149–51.

¹⁷ Karamzin, "Pis'mo sel'skago zhitelia," 579.

"affection of the slaves for their masters" and noted that these masters "maintain their slaves as the duty of humanity demands"; he contrasted the happiness of the Russian peasant with the misery of the poor in Western Europe.¹⁸ Count D. P. Buturlin, writing in French to his uncle in 1803, succinctly expressed the paternalistic assumptions inherent in the defense of serfdom. "There is something paternal and gentle in the reciprocal relationship between the master and his born servant, whereas this same relationship strikes me as purely mercenary when between the hired servant and his master." In the latter case, he explained, "it is a free market, an exchange of his service for my money, and from that point it seems to me that I am finished with everything when I have paid him."¹⁹ Often Russians contrasted the order, patriarchal relations, and security enjoyed by their peasants with the poverty, exploitation, and insecurity of free Western Europeans.²⁰

Such arguments also abounded, however, in the antebellum South. Indeed, a perusal of proslavery writings provides ample evidence that a paternalistic insistence on the humanity and harmony of slavery was as pervasive as the racial argument in its defense, especially during the last two decades before the Civil War. Most historians are aware of the writings of George Fitzhugh,²¹ but perhaps the attention his works have received has obscured similar justifications developed by a host of other Southerners, such as James H. Hammond, William Harper, William Gilmore Simms, Henry Hughes, Thomas R. Dew, and Thomas R. R. Cobb. The humanity of slavery, the reciprocal relationship between master and slave, and the brutality of the free-labor system were at the core of their defense of the peculiar institution. As Baptist minister Thornton Stringfellow put it in an otherwise largely religious justification of the treatment of slaves, "Their condition, as a class, is now better than that of any other equal number of laborers on earth, and is daily improving."²²

¹⁸ Ivan Boltin, *Primechaniia na Istoriu drevniia i nyneshniia Rossii g. Leklerka*, 2 (n.p., 1788): 243-44. For similar arguments, see M. M. Shcherbatov, "Na chitannii oktiabria 12 chisla gospodina deputata goroda Serpeiska, Rodiona Glinkova," in his *Sochineniia*, 128-29, and "Primechanie na 13-iu stat'iu II glavy proekta pravam blagorodnykh," *ibid.*, 195-96; E. R. Dashkova, "Memoires de la Princesse Dashkawa," in *Arkhiv kniazia Vorontsova*, 40 vols. (Moscow, 1870-97), 21: 137; Karamzin, "Pis'mo sel'skago zhitelia," 579-80; and Richard Pipes, ed., *Karamzin's Memoir on Ancient and Modern Russia: A Translation and Analysis* (Cambridge, Mass., 1969), 165.

¹⁹ D. P. Buturlin to his uncle, S. R. Vorontsov, St. Petersburg, July 30, 1803, in *Arkhiv kniazia Vorontsova*, 32: 366.

²⁰ See, for example, F. V. Rostopchin, "Vozrazhenie Grafa Rastopchina, na knigu, sochinennuiu Grafom Stroinovskim, o usloviakh s krest'ianami" (1811), *Chteniiia v Imperatorskom obshchestve istorii i drevnostei rossiiskikh pri Moskovskom universitete*, 30, no. 3, pt. 5 (1859): 40; and A. S. Shishkov, *Zapiski, mneniia, i perepiski Admirala A. S. Shishkova*, 2 (Berlin, 1870): 128-29. On paternalistic defenses of serfdom under Nicholas I, see S. P. Mel'gunov, "Epokha 'ofitsial'noi narodnosti' i krepostnoe pravo," in A. K. Dzhivelegov et al., eds., *Velikaia reforma: Russkoe obshchestvo i krest'ianskii vopros v proshlom i nastoiashchem. Iubileinoe izdanie*, 2 (Moscow, 1911): 8-14.

²¹ George Fitzhugh, *Sociology for the South: or, The Failure of Free Society* (Richmond, Va., 1854), and *Cannibals All! or, Slaves without Masters* (1857), ed. C. Vann Woodward (Cambridge, Mass., 1960). On Fitzhugh, see Harvey Wish, *George Fitzhugh: Propagandist of the Old South* (Baton Rouge, 1943); C. Vann Woodward, "George Fitzhugh, *Sui Generis*," Introduction to Fitzhugh, *Cannibals All!*, vii-xxxix; and Genovese, *The World the Slaveholders Made*, pt. 2.

²² Stringfellow, "The Bible Argument: or, Slavery in the Light of Divine Revelation," in Elliott, *Cotton Is King*, 491. Many of the best paternalistic defenses of slavery were reprinted in the 1850s in two large collections: *The Pro-Slavery Argument, as Maintained by the Most Distinguished Writers of the Southern States* (Charleston, S.C., 1852), and E. N. Elliott's much expanded volume, *Cotton Is King*. Leading Southern periodicals also frequently published paternalistic defenses of slavery. See, for example, W., "Slavery in the Southern States,"

Antebellum Southerners recognized that many of their best arguments in favor of slavery were nonracial, and the less timid among them fully acknowledged this fact. They noted that throughout history slavery had been a prerequisite for civilization and pointed to the slave societies of antiquity and the unfree labor systems of medieval Europe as precedents for Southern slavery.²³ These Southerners were not stupid men, and they realized that most of the slave systems they cited were not based on racial distinctions. The logic of their position, therefore, increasingly led them to broaden the defense of slavery to that of a superior social system, regardless of race. They extolled slavery for serving the best interests of all elements in society, without pitting class against class, while fostering all of the tried and true social virtues.²⁴ They did not deny that blacks were ideally suited for slavery, but they treated this predisposition as a fortunate accident rather than as the essential reason for slavery: had Africans not existed, other slaves would have been required in their place. As Hammond put it in his famous mud-sill speech,

In all social systems there must be a class to do the menial duties, to perform the drudgery of life. That is, a class requiring but a low order of intellect and but little skill. Its requisites are vigor, docility, and fidelity. Such a class you must have, or you would not have that other class which leads progress, civilization, and refinement. It constitutes the very mud-sill of society and of political government; and you might just as well attempt to build a house in the air, as to build either the one or the other, except on this mud-sill.²⁵

The South was fortunate in having found blacks, but Northern society also rested upon a mud-sill of workers who were "essentially slaves."

During the 1840s and 1850s the boldest and most consistent of the proslavery advocates increasingly downplayed race as a justification for slavery and made explicit their belief that slavery was part of a superior social system, regardless of any racial differences within society. This point of view never dominated the an-

Southern Literary Messenger, 9 (1843): 736-44; and Solon Robinson, "Negro Slavery at the South," *DeBow's Review*, 7 (1849): 206-25, 379-89.

²³ See, for example, Chancellor Harper, "Slavery in the Light of Social Ethics," in Elliot, *Cotton Is King*, 549-52, 574-75, 604-06; J. H. Hammond, "Slavery in the Light of Political Science," *ibid.*, 634-37; Thomas R. R. Cobb, *An Inquiry into the Law of Negro Slavery in the United States of America* (Philadelphia, 1858), xxxvi-cxxi; Matthew Estes, *A Defence of Negro Slavery as It Exists in the United States* (Montgomery, Ala., 1846), 13-48; Fitzhugh, *Cannibals All*, *passim*; and J. D. B. DeBow, "The Origin, Progress, and Prospect of Slavery," *DeBow's Review*, 9 (1850): 9-19. Occasionally, proslavery writers even used Russian serfdom as a precedent: Russian serfs, noted Cobb, "are contented with their lot and seek no change. They are indolent, constitutionally. . . . They are mendacious, beyond the negro perhaps, and feel no shame at detection. Like him, too, they have no providence for the future, and no anxiety about it." Cobb, *An Inquiry into the Law of Negro Slavery*, cxviii. On the precedent of classical slavery, see Edwin A. Miles, "The Old South and the Classical World," *North Carolina Historical Review*, 48 (1971): 258-75.

²⁴ See, for example, Hammond, "Slavery in the Light of Political Science," 643-46, and "Speech on the Admission of Kansas . . . , March 4, 1858," in Hammond, *Selections from the Letters and Speeches of the Hon. James H. Hammond*, 317-19; Thomas R. Dew, "Professor Dew on Slavery," in *The Pro-Slavery Argument*, 325-26, 457-61; Cobb, *An Inquiry into the Law of Negro Slavery*, xxxvi, ccxvii-ccxviii; D. R. Hundley, *Social Relations in Our Southern States* (New York, 1860), 64-70; [Edwin C. Holland] *A Refutation of the Calumnies against the Southern and Western States* . . . (1822; reprint ed., New York, 1969), 45-64; Henry Hughes, *Treatise on Sociology, Theoretical and Practical* (1854; reprint ed., New York, 1968), *passim*; Stringfellow, "Statistical View of Slavery," 533-34, 539; and Fitzhugh, *Cannibals All*, *passim*. On Fitzhugh's development of this argument, see Genovese, *The World the Slaveholders Made*, 165-234.

²⁵ Hammond, "Speech on the Admission of Kansas," 318-19.

tebellum South's defense of slavery, because planters needed the political support of nonslaveholding whites, but it was not limited to Fitzhugh or to a handful of atypical eccentrics.²⁶ "Pity it is," wrote author William Gilmore Simms, "that the louzy and lounging lazzaroni of Italy cannot be made to labor in the fields under the whip of a severe task-master!"²⁷ Henry Hughes, in his highly abstract defense of "warranteism," as he renamed slavery, noted that the "ethnical qualification" was "accidental." "Warranteism without the ethnical qualification," he concluded, "is that to which every society of one race must progress."²⁸ In his diary, fire-eater Edmund Ruffin praised George Fitzhugh's analysis of the "slavery of labor to capital" and ridiculed the notion that "the Africans generally, or the negroes particularly, are descended from Ham."²⁹ Fitzhugh's lead article in the October 1857 issue of the influential *DeBow's Review* argued forcefully that even among whites free labor was inferior to slave labor and chided Southerners for failing to carry proslavery arguments to their logical conclusion. "Domestic slavery must be vindicated in the abstract, and in the general," he asserted, "as a normal, natural, and, *in general*, necessitous element of civilized society, without regard to race or color."³⁰

Russians, then, developed an essentially racial argument in defense of serfdom, even though no racial distinction divided lord and peasant; at the same time, Americans elaborated an ideology that stressed the virtues of aristocracy and *noblesse oblige* in a society with democratic pretensions. Clearly, the need to defend unfree labor had a logic of its own that propelled Russians and Americans to arrive independently at almost all of the same major arguments and conclusions. The religious justification for bondage, which flourished in the United States but was perfunctory in Russia, was a major exception. *Pomeshchiki* told their serfs that their status was ordained by God and regularly relied on priests to instill obedience in their peasant parishioners, and defenders of serfdom often made passing references to a God-given order. But religious arguments were rarely central to formal writings advocating serfdom.³¹

In addition to racial and paternalistic arguments, two practical points received widespread circulation in both countries. The first was the economic ne-

²⁶ Drew Faust has seen South Carolina's leading proslavery spokesmen as alienated intellectuals who through "their essays on slavery won recognition other intellectual efforts failed to secure"; *A Sacred Circle*, 116, 113-31.

²⁷ Simms, "The Morals of Slavery," in *The Pro-Slavery Argument*, 265.

²⁸ Hughes, *Treatise on Sociology*, 207.

²⁹ Ruffin, *The Diary of Edmund Ruffin*, ed. William Kauffman Scarborough, 1 (Baton Rouge, 1972): 240, 308.

³⁰ Fitzhugh, "Southern Thought," *DeBow's Review*, 23 (1857): 347.

³¹ For a description of Russian religious justifications, see Mel'gunov, "Epokha 'ofitsial'noi narodnosti," 12-14; and, for examples of the far more numerous and detailed American religious arguments on behalf of slavery, see Richard Furman, *Rev. Dr. Richard Furman's Exposition of the Views of the Baptists, Relative to the Coloured Population of the United States* (Charleston, S.C., 1822), 7-12, 16-17; Charles Hodge, "The Bible Argument on Slavery," in Elliott, *Cotton Is King*, 841-77; Stringfellow, "The Bible Argument"; and Albert Taylor Bledsoe, "Liberty and Slavery: or, Slavery in the Light of Moral and Practical Philosophy," in Elliott, *Cotton Is King*, esp. 337-80. Most of the other defenses of slavery, which I have cited above, mention, at least in passing, the religious justification. One reason for the prevalence of religious justifications of American slavery is doubtless the prominent role Southern clergymen played in defending the peculiar institution; see Smith, *In His Image*, 129-64; and Morrison, "The Proslavery Argument," 30-59. Whereas Protestant ministers in the United States were an independent-minded group who often spoke out on social issues, the Russian parish clergy was a closed and increasingly isolated caste, "a weak, tangential group lacking in influence and power." See Gregory L. Freeze, *The Russian Levites: Parish Clergy in the Eighteenth Century* (Cambridge, Mass., 1977), 222, 179-217.

cessity of forced labor. Americans and Russians recognized that their systems of bondage arose to meet a general labor shortage under conditions of relative population scarcity; both held out the prospect of economic disaster should unfree labor be abolished.³² Even more widespread were dire predictions of social collapse—refusal to work, unrest, and rebellion—should slaves and serfs be emancipated. If Russian polemicists could point to the degenerate condition of state peasants, who were without proper seigneurial supervision, their American counterparts had a far more compelling argument in the example of emancipation in the West Indies. The British colonies of Barbados and Jamaica showed how blacks, freed from the protective care of slavery, would revert to their primitive African ways, while Haiti held out the ultimate horror of revolution. A similar nightmare stalked the Russian gentry: the bloody Pugachev rebellion of 1773–74, which for generations served as a warning to *pomeshchiki* of just how precarious their position was.³³

Russians and Americans even played the same word games, insisting that their particular form of bondage was the mildest, or even that it was not really bondage at all. In the eighteenth and early nineteenth centuries, Russians commonly used the word "slavery" (*rabs'tvo*) to describe the status of their serfs, but defenders of serfdom often pointed out that their form of slavery was different from others. "Between freedom and freedom, and between slavery and slavery, there is a difference, and this difference is great and varied," wrote Boltin; "a title means nothing." "There is," he continued, "freedom that is worse than slavery." Naturally, Russian slavery was a variety better than most freedom.³⁴ Precisely the same kinds of assertions were made in the United States: Henry Hughes's refusal to use the word "slavery," for which he substituted "warranteeism," was unusual, but many held with Hammond that slavery was but a name that meant little, for the working classes everywhere were really slaves. "The difference between us," he lectured Northerners, "is that our slaves are hired for life and well compensated. . . . Yours are hired by the day, not cared for, and scantily compensated." Matthew Estes agreed that the word "slavery"

³² See, for example, M. M. Shcherbatov, "Zapiska po krest'ianskomu voprosu," in his *Neizdannye sochineniia*, 8–9; Major General Afanasii Lavrent'evich Komarov, Essay in *Trudy Vol'nago ekonomicheskago obshchestva k pooschreniiu v Rossii zemledeliia i domostroitel'stva*, 65 (1814): 128–61; G. R. Derzhavin, *Sochineniia Derzhavina s ob'iasnitel'nymi primechaniiami Ia. Grola*, 6 (St. Petersburg, 1876): 774–75; Chancellor Harper, "Harper on Slavery," in *The Pro-Slavery Argument*, 85–94; Estes, *A Defence of Negro Slavery*, 155–62; Francis J. Grund, *The Americans in Their Moral, Social, and Political Relations* (1837), ed. Robert F. Berkhofer, Jr., 2 vols. in 1 (New York, 1968), 336–76 *passim*; and J. D. B. DeBow, *The Interest in Slavery of the Southern Non-Slaveholder* (Charleston, S.C., 1860), 4–8. The economic argument was potentially the mildest and most equivocal defense of bondage, since such a rationale was entirely compatible with the idea that unfree labor was wrong in theory but necessary under particular circumstances.

³³ M. M. Shcherbatov, "Zamechaniia Shcherbatova na bol'shoi nakaz Ekateriny," in his *Neizdannye sochineniia*, 55–56, and "Primechanie na 13-iu stat'iu," 196–97; "Mnenie ob osvobozhdenii krest'ian" (1767), in *Russkii arkhiv* (1871), 288–91; Komissarenko, "Proekt vvedeniia lichnoi krepostnoi zavisimosti ekonomicheskikh krest'ian"; Karamzin, "Pis'mo sel'skago zhitelia," 569–74; F. V. Rostopchin, "Zamechanie Grafa F. V. Rostopchina na knigu g-na Stroinovskago" (1811), *Chleniia v Imperatorskom obshchestve istorii i dremostei rossiskikh*, 33, no. 2, pt. 5 (1860): 205, 211–15; Frederika Teute Schmidt and Barbara Ripel Wilhelm, "Early Proslavery Petitions in Virginia," *William and Mary Quarterly*, 3d ser., 30 (1973): 138–40; Dew, "Professor Dew on Slavery," 437–40; Harper, "Slavery in the Light of Social Ethics," 617–23; [Holland] *A Refutation of the Calumnies against the Southern and Western States*, 61–85; and Estes, *A Defence of Negro Slavery*, 232–52.

³⁴ Boltin, *Primechaniia*, 235–36. Also see Shcherbatov, "Zamechaniia Shcherbatova na bol'shoi nakaz Ekateriny," 55; and Shishkov, *Zapiski, mneniia, i perepiski*, 120–24.

gave the South a bad name. "Theoretically, slavery has been abolished in most countries," he admitted; "but, practically, it exists almost every where—but without the responsibilities, interests, humanities, and sympathies of [Southern] slavery."³⁵

DESPITE MANY OBVIOUS INCONSISTENCIES, there is a common thread running through virtually all of the defenses of servitude in Russia and the United States: the assumption that men are naturally unequal. This assumption usually appears only implicitly in the arguments on behalf of forced labor, but at times it is quite explicit. Prince Shcherbatov, for example, boldly refuted the concept of natural equality. "Not one person is completely like another," he wrote; "and, where this similarity is lacking, so too is equality." Half a century later, Admiral Mordvinov made the same point. "Only disorganized, wild society offers equality of rights, conditions, and powers," he explained. "Such is the condition of all Asiatic peoples."³⁶ Nineteenth-century Southerners were sometimes even more explicit in combating head-on the basic precepts of Jeffersonianism. "Man is born to subjection," wrote South Carolina's Chancellor Harper; "it is the very basis of his nature, that the strong should control the weak and ignorant." Hammond was equally direct and forceful in refuting the doctrine of natural, unalienable rights: "I repudiate, as ridiculously absurd, that much lauded but nowhere accredited dogma of Mr. Jefferson, that 'all men are born equal,' " he declared. "It is a wretched and insecure government which is administered by its most ignorant citizens, and those who have the least stake under it."³⁷ Regardless of the variety of ways used to defend inequality (race, class, lineage, wealth, ability, intellect, or moral capacity), the essential premise of all who defended bondage was that all men were *not* created equal.

Because the proponents of unfree labor were forced to defend inequality in an era of natural rights, they were compelled to challenge many of the dominant intellectual currents of their time, to reject the French Enlightenment, Jeffersonianism, and progress. The defense of slavery and serfdom therefore led inevitably to reactionary views on most other social questions. Although some proslavery advocates in the United States South were able to stay in tune with the times by the simple expedient of excluding blacks from the realm of humanity,³⁸ the logic of defending slavery left most polemicists uncomfortable with talk of any kind of reform.

³⁵ Hughes, *Treatise on Sociology*; Hammond, "Speech on the Admission of Kansas," 319; and Estes, *A Defence of Negro Slavery*, 130. Also see Cobb, *An Inquiry into the Law of Negro Slavery*, cxii–cxxxii, cxix–ccxii; and the editor's Introduction to "Northern and Southern Slavery," *Southern Literary Messenger*, 7 (1841): 341. On Southern discomfort with the term "slavery," see Kenneth S. Greenberg's perceptive "Revolutionary Ideology and the Proslavery Argument: The Abolition of Slavery in Antebellum South Carolina," *JSH*, 42 (1976): 365–84.

³⁶ Shcherbatov, "Razmyshlenie o dvorianstve," 222; and Mordvinov, "Mnenie Admirala Mordvinova po rabstvu krest'ian, v 1833 godu," *Chtenia i Imperatorskom obshchestve istorii i drevnosti rossiiskikh*, 30, no. 3, pt. 5 (1859): 56.

³⁷ Harper, "Harper on Slavery," 8; and Hammond, "Slavery in the Light of Political Science," 637–38. Also see W. G. Bean, "Anti-Jeffersonianism in the Antebellum South," *North Carolina Historical Review*, 12 (1935): 103–24.

³⁸ See Fredrickson, *The Black Image in the White Mind*, 61–94.

In 1857, British traveler Barbara Eodichon summed up in her diary a conversation she had with several Southerners while on a Mississippi River steamboat. "There is evidently a feeling," she wrote, "that Abolition and Woman's Rights are supported by the same people and the same arguments, and that both are allied to atheism—and all these slave owners are very religious people."³⁹ Her observation was perceptive. Almost every major antebellum defender of slavery insisted on tying abolitionism to a host of other "isms"—"heresies" that threatened social peace and stability. George Frederick Holmes was typical in his suggestion that in the North, "where Fourierism, and Proudhonism, Free Love, and Total Abstinence, and all the other modern forms of philanthropic innovation have found numerous and enthusiastic votaries, an exaggerated and distorted idea of the nature and functions of liberty has inspired the multitudinous heresy of Abolitionism."⁴⁰ Rejecting the optimistic spirit of Northern reform, Hammond deplored the democratic course of the nineteenth century, with the ascendancy of "the MOB—THE SANS-CULOTTES. Preaching as their watchword that now prostituted sentiment 'that all men are born free and equal,' they have rallied to their standard the ignorant, uneducated, semi-barbarous mass which swarms and starves upon the face of Europe!" Only one bulwark remained against this leveling surge: the slave South. Scoffing at abolitionist charges of a slave-owning aristocracy, Hammond replied, "I accept the terms. *It is a government of the best*, combining all the advantages of the old world."⁴¹

Russian defenders of serfdom denounced the democratic spirit in much the same terms. Like their American counterparts, they saw themselves defending not simply an institution but a conservative regime, one threatened by equality, democracy, and revolution. As early as the 1780s, Count S. R. Vorontsov identified the main enemy as "this spirit of reform and universal equality preached for fifty years by the economists and encyclopedists in France." Half a century later Admiral A. S. Shishkov vigorously protested against the view that the "spirit of the times" demanded reform of serfdom. "By the *spirit of the times*," he asserted, "is often meant a general willfulness and disobedience." Contrasting the social harmony of Russia with the turmoil of Western Europe, he asked rhetorically, "Why changes in laws, changes in customs, changes in manner of thought? And whence these changes? From the schools and philosophizing of those countries where these disorders, these insurrections, this insolence of thought . . . reign supreme." The only cure, he suggested, was strict censorship to guard against the spreading disease of free thought. In a fervent attack on the concept of a natural right to freedom, Count F. V. Rostopchin explained that, although the term "freedom" was appealing, "it is not the natural condition of a person," for all members of society were dependent on one another. "The first consequence of

³⁹ Barbara Leigh Smith Bodichon, *An American Diary, 1857–8*, ed. Joseph W. Reed, Jr. (London, 1972), 61.

⁴⁰ George Frederick Holmes, "Theory of Political Individualism," *DeBow's Review*, 22 (1857): 134. Also see Harper, "Slavery in the Light of Social Ethics," 580–85; Hammond, "Slavery in the Light of Political Science," 117, 149–50; Stringfellow, "Statistical View of Slavery," 524–28, 540–42; Simms, "The Morals of Slavery," 264; Hundley, *Social Relations in Our Southern States*, 16–17; and Fitzhugh, *Cannibals All!*, 6, 9–11, 85–106, 190–98, 213–16.

⁴¹ Hammond, "Speech on the Justice of Receiving Petitions," 43–45.

freedom is willfulness," he noted, "the second is disobedience, and the third is revolt against all authority." In the first half of the nineteenth century Russians and Southerners liked to contrast the harmony of class relations in their societies with the chaos of the free-labor market in Western Europe and the North.⁴² For both, the defense of servitude was an integral part of the general defense of a world threatened by change.

WHILE ESSENTIALLY SIMILAR IN CHARACTER, the arguments for slavery and serfdom differed in their development over time. This difference became apparent, however, only after the beginning of the nineteenth century. Before 1800 American and Russian thought on unfree labor followed remarkably similar paths. In both countries there was little discussion of bondage before the 1760s;⁴³ challenges to slavery and serfdom were few, but so too were articulated defenses of institutions taken largely for granted. In both countries, complacency and silence gave way to concern and debate during the last third of the eighteenth century. Although those who raised the issue of forced labor rarely called for immediate abolition, they did broach the possibility of gradual emancipation in the future and discuss ways of more immediately limiting cruel and arbitrary treatment of the bondsmen.⁴⁴ They also aroused the angry opposition of those who insisted no change was needed.

This parallel course came to an abrupt end in the nineteenth century. Cautious antislavery sentiment, common among spokesmen of the Upper South, gradually evaporated and was replaced, at first, by an awkward, hesitant, even reluctant defense of slavery. As late as 1826 Presbyterian missionary Timothy Flint was surprised to find that planters defended existing conditions primarily

⁴² Vorontsov, "Zapiska Grafa S. R. Vorontsova o dvorianstve," *Arkhiv kniazia Vorontsova*, 16: 299-300; Shishkov, *Zapiski, mneniia, i perepiski*, 121, 129; Shishkov to the Emperor, December 12, 1836, *Chleniia v Imperatorskom obshchestve istorii i drevnostei rossiiskikh*, 71, no. 3, pt. 2 (1868): 121-28; and Rostopchin, "Zamechanie Grafa F. V. Rostopchina na knigu g-na Stroinovskago," 204-05. Also see Count D. P. Buturlin to S. R. Vorontsov, February 22, April 4, and July 30, 1803, in *Arkhiv kniazia Vorontsova*, 22: 334-36, 343-45, 364-66. Although the defense of serfdom was extremely useful to supporters of the status quo, it was not absolutely essential. Under Nicholas I, government spokesmen sometimes contrasted conservative Russia and the revolutionary West—using the official line of "Autocracy, Orthodoxy, and Nationality"—without reference to serfdom; and most Slavophiles, although contemptuous of Western decadence, were at least in theory critical of serfdom. See Edward C. Thaden, *Conservative Nationalism in Nineteenth-Century Russia* (Seattle, 1964), esp. 19-20; and Nicholas V. Riasanovsky, *Nicholas I and Official Nationality in Russia, 1825-1855* (Berkeley and Los Angeles, 1961), esp. 12, 134-43, 167-68, and *Russia and the West in the Teachings of the Slavophiles* (Cambridge, Mass., 1952), 91-119, 136-40.

⁴³ Jenkins, *Pro-Slavery Thought*, 3; David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, N.Y., 1966), 125-50; and Semevskii, *Krest'ianskii vopros*, 1: 1-12.

⁴⁴ On the debate over slavery during the Revolutionary era, see David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770-1823* (Ithaca, N.Y., 1975), 82-326, and *The Problem of Slavery in Western Culture*, 365-445; Jordan, *White over Black*, 287-304, 429-81; William W. Freehling, "The Founding Fathers and Slavery," *AHR*, 77 (1972): 81-93; Robert McColley, *Slavery and Jeffersonian Virginia* (Urbana, Ill., 1964), 114-40; Duncan J. MacLeod, *Slavery, Race, and the American Revolution* (Cambridge, 1975), 14-47; John Chester Miller, *The Wolf by the Ears: Thomas Jefferson and Slavery* (New York, 1977); and Tise, "Proslavery Ideology," 80-110. On the peasant question in Catherine II's Russia, see Semevskii, *Krest'ianskii vopros*, 1: 14-213; Beliaevskii, *Krest'ianskii vopros v Rossii nakazanie vosstaniia E. I. Pugacheva*; Jones, *The Emancipation of the Russian Nobility*, 135-42; Augustine, "Economic Attitudes and Opinions"; Dukes, *Catherine the Great and the Russian Nobility*, 91-95; Mavrodin, *Klassovaia bor'ba*, 166-83; and A. I. Pashkov, ed., *A History of Russian Economic Thought: Ninth through Eighteenth Centuries* (1955), trans. John M. Letiche et al. (Berkeley and Los Angeles, 1964), 475-575.

on the grounds of necessity, precedent, and the dangers of sudden emancipation, not the virtues of slavery. "I have never yet heard one," he exaggerated, "who does not admit that slavery is an evil and an injustice and who does not at least affect to deplore the evil."⁴⁵ Then, during the thirty years preceding the Civil War, the South produced an extraordinary torrent of proslavery propaganda—propaganda that was for the most part bold, unapologetic, and insistent on the positive virtues of slavery. Whereas previous proslavery advocates had stressed the "practical" arguments of expedience, economy, and race, the new polemicists, who included many of the South's best minds, increasingly based their case on the higher ground of social theory: slavery was simply the best way to organize society. By the 1850s, Southern whites seemed so committed to their peculiar institution that a lone dissenter such as Hinton Helper found it expedient to leave the South altogether.⁴⁶

In Russia, however, the trend that had begun in the eighteenth century continued into the nineteenth. Although some noblemen still spoke out in defense of serfdom and unarticulated sentiment in favor of maintaining the status quo remained strong, especially in the provinces, the balance gradually tipped in favor of those dissatisfied with the institution. By the 1840s, free-labor ideas had spread widely among the educated nobility of Moscow and St. Petersburg, and public defense of serfdom became increasingly rare. (Significantly, virtually all of the defenses of serfdom cited above date from before 1840.) Thus, Russia never experienced the kind of militant proslavery movement that reigned in the antebellum South. If the entire American South seemed to rally around slavery, educated Russians—including many high level government officials—became increasingly convinced that serfdom was a backward system that must, somehow, be abolished.⁴⁷ In 1839 the annual report of the Third Department, or political police, suggested that the time had come for the government to begin preparing for eventual emancipation rather "than to wait until it begins from

⁴⁵ Timothy Flint, *Recollections of the Last Ten Years* (1826), ed. C. Hartley Grattan (New York, 1932), 329. For similar observations, see Morris Birkbeck, *Notes on a Journey in America, from the Coast of Virginia to the Territory of Illinois* (1817; reprint ed., Ann Arbor, Mich., 1968), 16; Harriet Martineau, *Society in America* (1837), ed. Seymour Martin Lipset (abridged ed., New York, 1962), 189; and Frances Anne Kemble, *Journal of a Residence on a Georgian Plantation in 1838-1839* (Chicago, 1969), 130.

⁴⁶ See Jenkins, *Pro-Slavery Thought*, 48-300; Genovese, *The World the Slaveholders Made*, pt. 2; Fredrickson, *The Black Image in the White Mind*, 43-96; Smith, *In His Image*, 129-64; and Takaki, *A Pro-Slavery Crusade*, *passim*. On the South as a closed society, see Clement Eaton, *The Freedom-of-Thought Struggle in the Old South* (New York, 1964).

⁴⁷ Among the great Russian novelists of the nineteenth century, Nicholas Gogol was virtually alone in his defense of serfdom, and some, such as Ivan Turgenev, eluded heavy government censorship and openly ridiculed the institution. For Gogol's defense of serfdom as a God-given institution designed for the well-being of all, see his "Russkii pomeshchik," originally published in his *Vybrannye mesta iz perepiski s друз'iami* (1847), reprinted in N. V. Gogol, *Sobranie sochinenii v semi tomakh*, 6 (Moscow, 1967): 316-23. For Turgenev's most damning portrait of serfdom, see his *A Sportsman's Notebook*, trans. Charles Hepburn and Natasha Hepburn (London, 1950), and, for Turgenev as an opponent of serfdom and autocracy, see Harry Hershkovitz, *Democratic Ideas in Turgenev's Works* (New York, 1932). In contrast, leading antebellum Southern writers—one cannot call them great—commonly extolled the peculiar institution. For treatment of a prominent example, see Jon L. Wakelyn, *The Politics of a Literary Man: William Gilmore Simms* (Westport, Conn., 1973). Because most Southern writers advocated slavery, Hannah Stern Goldman, who has stressed the parallels between antibondage themes in American and Russian nineteenth-century fiction, was forced to compare Russian novels with those written in the North; see her "American Slavery and Russian Serfdom: A Study in Fictional Parallels" (Ph.D. dissertation, Columbia University, 1955).

below, from the people." The report called for quiet deliberation, "without noise and without loud words," but concluded that "everyone is agreed" on the need for reform.⁴⁸ When in 1857 Alexander II made public his decision to go ahead with emancipation, noblemen grumbled and dragged their feet, but there was no public opposition, no threat that serfowners might refuse to accept the reforms.⁴⁹

THAT A BASIC CONTRAST EXISTS here is clear; the obvious question is how to account for it. Although that question cannot be definitively answered in a few pages, several factors can be discerned that contribute to an explanation. They also reveal much not only about American slavery and Russian serfdom but about how social elites behave when their privileges are under attack as well.

The most obvious is that of race. Not only did proslavery spokesmen often couch their arguments in racial terms but Southern whites were also, in general, so imbued with the consciousness of race that they found it impossible to contemplate emancipation of blacks in a white society. Travelers to the antebellum South found that many whites who were distinctly unenthusiastic about slavery balked at the notion of turning blacks loose in the South. As one poor white told Frederick Law Olmsted, "I reckon the majority would be right glad if we could get rid of the niggers. But it wouldn't never do to free 'em and leave 'em here. . . . Nobody couldn't live here then."⁵⁰ That slavery in the United States was racial instead of merely social clearly served to inhibit the growth of moderate opposition and to make Southern whites more receptive to the proslavery appeal.

If Russian defenders of serfdom invented essentially racial arguments to serve their cause and regarded peasants as a different people, a qualitative gap still separated both the perceptions of the differences between masters and bondsmen in the two countries and the perceptions of the threat inherent in emancipation. Blacks were not merely different: they were outsiders in a country where everyone else was an insider or a potential insider. Hence, free blacks seemed like such an anomaly. Blacks were aliens—Africans deposited against their will in a foreign land—and, despite protestations to the contrary, most whites always regarded them as such. How else could one speak of sending "back" to Africa people who in most cases were third- and fourth-generation Americans? Although a Southern planter might refer to his slaves as "my people," the term

⁴⁸ "Iz otcheta III Otdeleniia o vnutrennem sostoiianii i o krest'ianskom dvizhenii v strane v sviazi s pozharimi," in A. V. Predtechenskii, ed., *Krest'ianskoe dvizhenie v Rossii v 1826–1849 gg.: Sbornik dokumentov* (Moscow, 1961), 344–45. "I have never been either an ultra-liberal or a carbonari . . . but I have always detested personal slavery, and I still detest it, and deplore its continuation among us and everywhere I see it," wrote Prince M. S. Vorontsov, whose father had defended serfdom; Vorontsov to P. D. Kiselev, Tiflis, October 20, 1847, in *Arkhiv kniazia Vorontsova*, 38: 149–50.

⁴⁹ Semevskii, *Krest'ianskii vopros*, 1: 236–end, 2: *passim*; Terence Emmons, *The Russian Landed Gentry and the Peasant Emancipation of 1861* (Cambridge, 1968), 29–35; Daniel Field, *The End of Serfdom: Nobility and Bureaucracy in Russia, 1855–1861* (Cambridge, Mass., 1976), esp. 102–40; and Nicholas V. Riasanovsky, *A Parting of the Ways: Government and the Educated Public in Russia, 1801–1855* (Oxford, 1976), 262–63.

⁵⁰ Olmsted, *A Journey in the Back Country* (1860; reprint ed., New York, 1970), 203. Olmsted noted that "these views of slavery seem to be universal among people of this class. They were represented to me at least a dozen times." Also see Fredrickson, *The Black Image in the White Mind*, 56–90 *passim*.

"the people" was always reserved for whites who formed the body politic. In a world where all free men were politically equal—and white—the prospect of freeing black slaves alarmed slaveholders and nonslaveholders alike.⁵¹

Russian peasants, however, were not outcasts but merely the lowest level of a stratified society. White Americans could think of the United States as an all-white country, but Russia without peasants was inconceivable. Not only did they constitute over four-fifths of the population, they were the essence of Russia. When Russians spoke of "the people," they meant precisely the peasants. For the peasants—whether bound or free—to constitute "the people" did not threaten the Russian established order in the way that citizenship for blacks threatened the white South, because Russia was a hierarchical society, composed of legally established castes or estates, without the slightest pretension to democracy or equal rights. Emancipating blacks in the United States entailed the radical step of extending citizenship. Emancipating serfs, however, constituted a less extreme measure, for an emancipated serf would still be a peasant, and no one imagined that emancipation would make a peasant the equal of a noble. In short, the combination of race and democracy served to reinforce the commitment to slavery.⁵²

There was another way in which the lack of democracy served to undercut the Russian nobility's commitment to serfdom. Because Russia had a bureaucratic government, organized on essentially military lines of command, the gentry was not in a position either to shape or to resist government policy. During the decades preceding emancipation the top ranks of government—the tsars and many of their close advisors—were committed to a policy of gradual reform. Southern whites would have elected new leaders, but Russian *pomeshchiki* had no such choice; they simply sulked or ignored the matter while successive government committees considered how to handle the "peasant question." Even when faced with emancipation in the late 1850s, reluctant noblemen did not think of refusing; instead, they contented themselves with minor obstructions and delays while insisting that the final settlement be as favorable as possible to their economic interests.⁵³

Such was not, of course, the response of Southern slaveholders when slavery was assaulted. Surely one important clue to Southern response lies in the nature

⁵¹ Several authors have, in varying fashions, stressed the connection among race, democracy, and slavery in the antebellum South. See Carl N. Degler, *Neither Black nor White: Slavery and Race Relations in Brazil and the United States* (New York, 1971), 256–60, and "The Irony of American Negro Slavery," in Harry P. Owens, ed., *Perspectives and Irony in American Slavery* (Jackson, Miss., 1976), 3–12; Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 369–86; and MacLeod, *Slavery, Race, and the American Revolution*, 148–84. On Southern democracy, see William J. Cooper, Jr., *The South and the Politics of Slavery, 1828–1856* (Baton Rouge, 1978), esp. 23–42.

⁵² For a similar interpretation, see Degler, *Neither Black nor White*, 256–60.

⁵³ Semevskii, *Krest'ianskii vopros*, 1: 236–end, 2: *passim*; Field, *The End of Serfdom*, 102–40, 172–232, 265–323, 359–60; P. A. Zaionchkovskii, *Otmena krepostnogo prava v Rossii* (3d ed., Moscow, 1968), 1–123; and W. Bruce Lincoln, *Nicholas I: Emperor and Autocrat of All the Russias* (London, 1978), 187–95. Noblemen's attitudes during the preparations for emancipation can be traced in the pages of *Zhurnal zemlevladel'tsev*, which appeared twice monthly in 1858 and 1859. On the prereform bureaucracy, see S. Frederick Starr, *Decentralization and Self-Government in Russia, 1830–1870* (Princeton, 1972), 9–53; and P. A. Zaionchkovskii, "Gubernskaia administratsiia nakanune Krymskoi voyny," *Voprosy istorii* (1975), no. 9: 33–51.

of the challenge: it was open, and it came from without. Opponents of slavery were able publicly—through meetings, petition drives, pamphlets, newspaper articles and editorials, and political speeches—to challenge both the legitimacy of slaveowning and the morality of slaveowners. The outspoken defense of slavery thus arose largely in reaction to these attacks upon it. Equally important was the sectional nature of the opposition. Because antebellum slavery was Southern and abolitionism Northern, the attack on slavery appeared to be an attack on the South itself. The defense of slavery would undoubtedly have been quite different but for the sectional character of the assault on it.⁵⁴

Russia, however, was not divided into free and unfree sections. Although serfs were more numerous in some areas than in others, serfdom was a national institution. Equally significant, Russians, because of the nature of their government, never conducted a public debate over the peasant question. Proposals for abolishing or modifying serfdom, although widely discussed in the highest circles, were always confined to secret committees whose proceedings were not made public. Heavy censorship meant that only the most Aesopian remarks critical of Russian institutions could appear in print, and nothing like the American abolitionists' denunciation of slaveholders as sinners was possible.⁵⁵ As a result, serfowners never had—or got—to defend themselves as American slaveowners did. Even had they wanted to, they would not have been able to play the same ideological role as American proslavery spokesmen: there was no Russian "public" to which to appeal, and censorship of arguments defending serfdom was almost as rigid as censorship of polemics against it. The absence of a free press and of any tradition of democratic debate on policy thus precluded a full development of proserfdom thought.⁵⁶

Thus, the flowering of the antebellum defense of bondage in the U.S. South, in contrast to its withering in Russia, is at least partially explicable in terms of four concrete differences between the two societies, four characteristics that were present in the United States and absent in Russia: (1) a racial distinction between owner and owned; (2) a democratic political system; (3) freedom of the press; and (4) the sectional nature of servitude. These four are functionally related to a fifth, which subsumes the other four under it and is the most important of all. This crucial difference involves the independence of the master class and the strength of its civilization.

Southern planters were stronger, were more independent, than their Russian counterparts in relation both to their government and to their property. Because slaveowners lived under a democratic political system whose government repre-

⁵⁴ Jenkins, *Pro-Slavery Thought*, 65–66, 104; Fredrickson, *The Black Image in the White Mind*, 3; Morrow, "The Proslavery Argument Revisited," *passim*; Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966), 121–22; Jesse T. Carpenter, *The South as a Conscious Minority, 1789–1861* (New York, 1930), 21–76, 173–220; and Cooper, *The South and the Politics of Slavery*: 58–65.

⁵⁵ Former Decembrist Turgenev emphasized precisely this contrast between Russian silence and an outspoken American abolitionism; see his *La Russie et les russes*, 3 vols. (Brussels, 1847), 2: 113.

⁵⁶ Semevskii, *Krest'ianskii vopros*, 1: 28–33, 393–410, 419–28, 2: 325–40, 349–61; and Field, *The End of Serfdom*, 39–40.

sented their interests, because blacks were outsiders in a white society, and because slavery in the antebellum period was a sectional institution, Southern slaveowners constituted a local ruling class with a powerful sectional culture. As Eugene D. Genovese has cogently argued, they created a confident civilization based on the conservative virtues of paternalism, *noblesse oblige*, order, and social cohesion that contrasted sharply with the slave societies of the Caribbean.⁵⁷ It also contrasted sharply with that of imperial Russia.

If Southern slaveowners were able to develop their own independent sectional culture, one might argue that the central problem of Russian *pomeshchiki* was their dependence—on both their serfs and their government. Of course, Russian noblemen, both as individuals and as a class, were powerful men, and it may seem paradoxical to speak of them as a dependent group. But they lacked almost all of the elements of independence enjoyed by American planters. Unlike slaveowners, most of whom lived on their farms or plantations (or in nearby towns) and took an active role in governing both their slaves and their communities, Russian noblemen maintained a largely absentee mentality. This was dictated in part simply by prevailing demographic conditions, which made it much more difficult for prominent Russian *pomeshchiki* to develop the kind of local ties that were common among American planters. Whereas the vast majority of Southern slaves were held in units of under one hundred slaves, almost half of all serfs lived on holdings of over one thousand peasants, and four-fifths of them lived on holdings of over two hundred.⁵⁸ Wealthy, influential noblemen usually owned numerous estates, scattered among several provinces, each of which they visited only occasionally, if at all.

Even holders of smaller numbers of serfs frequently lived in Moscow, St. Petersburg, or a provincial capital, leaving the management of their estates to stewards. Although noblemen won their freedom from compulsory state service during the second half of the eighteenth century, they continued to look upon themselves as a serving class, and most noblemen continued to regard government service as the norm, retiring to one of their estates only as old men. As a result, they never developed the kind of corporate spirit or local commitments that were common among Western European aristocrats—or Southern planters. Of course, a large number of Russian landlords were not absentee owners; many of the less exalted noblemen owned only a few serfs, and at the bottom of the noble hierarchy was a significant group of petty landowners who were reduced to working in the fields alongside their peasants.⁵⁹ But emphasizing their phys-

⁵⁷ Genovese, *The World the Slaveholders Made*, esp. chap. 2, *The Political Economy of Slavery: Studies in the Economy & Society of the Slave South* (New York, 1965), esp. 28–31, Roll, Jordan, Roll: *The World the Slaves Made* (New York, 1974), 1–97, and “Slavery—The World’s Burden,” in Owens, *Perspectives and Irony in American Slavery*, 27–40. Also see Moore, *Social Origins of Dictatorship and Democracy*, 122; William K. Scarborough, “Slavery—The White Man’s Burden,” in Owens, *Perspectives and Irony in American Slavery*, 103–35; Rollin G. Osterweis, *Romanticism and Nationalism in the Old South* (1949; reprint ed., Baton Rouge, 1967); Robert E. Shalhope, “Race, Class, Slavery, and the Antebellum Southern Mind,” *JSH*, 37 (1991): 557–74; and Clement Eaton, *The Growth of Southern Civilization, 1790–1860* (New York, 1961), esp. 1–48, 98–124, 295–324.

⁵⁸ A. Troitskii, *Krepostnoe naselenie v Rossii, po 17-i narodnoi perepisi* (St. Petersburg, 1861), 45, 65.

⁵⁹ See Blum, *Lord and Peasant in Russia*, 349, 375–76; Emmons, *The Russian Landed Gentry*, 4–5; and Richard Pipes, *Russia under the Old Regime* (New York, 1974), 175–78. In 1858, 77.5 percent of all *pomeshchiki* in Euro-

ical presence risks obscuring their psychological absence. Not only were the richest and most powerful lords, whose lifestyle and conduct influenced the social aspirations of all noblemen, usually far removed from their estates; but, more importantly, even most resident *pomeshchiki* possessed an absentee mentality. Their hearts were still in Moscow or St. Petersburg, not in the countryside; the latter belonged to the peasants. As Daniel Field has aptly written, the Russian village "was a peasant world; the pomeshchik was almost an outsider even on his ancestral estate."⁶⁰

The consequences of this contrast—between a resident planter class and a largely absentee class of servitors—are numerous; one of the most important concerns the nature of relations between masters and bondsmen. But the consequence of interest here involves the defense of bondage. Unlike American planters, Russian noblemen were simply not in a position to defend forced labor; they lacked the independence to do so. They also lacked much of the incentive, for many *pomeshchiki* were essentially *rentiers*, deriving an income from, but otherwise unconcerned about, their estates.⁶¹ Whereas emancipation threatened the entire world of Southern slaveowners, it primarily threatened the Russian noblemen's livelihoods. If a way could be found to safeguard their immediate economic interests, the noblemen had relatively little to lose from abolition. Therefore, during the preparations for emancipation in the late 1850s, they concentrated not on opposing the new order but on securing the best possible terms for themselves under it. And the terms they ultimately secured were generous indeed.⁶²

pean Russia owned two hundred or fewer serfs (one hundred or fewer "male souls") and were therefore unable, according to the exaggerated standards of the time, to live in proper gentry style; 43.6 percent owned forty or fewer serfs (the poorest of these had lifestyles that differed little from those of prosperous peasants); and 3.4 percent of all noblemen owned no serfs at all; Troinitskii, *Krepostnoe naselenie*, 45.

⁶⁰ Field, *The End of Serfdom*, 22. On the serving mentality and lack of independence of the Russian nobility, see Romanovich-Slavatinskii, *Dvorianstvo v Rossii*, esp. 115–208, 402–10, 495–500; S. A. Korf, *Dvorianstvo i ego soslovnnoe upravlenie za stoletie 1762–1855 godov* (St. Petersburg, 1906), esp. 212–14, 287–363, 391–94, 449–59, 635–51; Max Beloff, "Russia," in A. Goodwin, ed., *The European Nobility in the Eighteenth Century: Studies of the Nobilities of the Major European States in the Pre-Reform Era* (London, 1953), 172–89; Pipes, *Russia under the Old Regime*, 98, 172–79; and Robert David Givens, "Servitors or Seigneurs: The Nobility and the Eighteenth-Century Russian State" (Ph.D. dissertation, University of California, Berkeley, 1975).

⁶¹ The contrast between American planters and Russian *pomeshchiki* is clearly revealed in the instructions and rules they composed for running their estates. In contrast to the American regulations, which usually provided detailed prescriptions for the care and handling of slaves, the Russian instructions focused on agricultural matters, and on the extraction of income from the peasants. Compare, for example, *pomeshchik* Alekseev Gvozdev's "Prikaznye punkty . . ." (composed ca. 1811–21), in A. Cherepnin, ed., "Prikazy starostam," *Trudy vysochaishhe uchrezhdennoi Riazanskoi Uchenoi Arkhivnoi Komissii za 1904 god*, 19: 82–93, with the "Rules on the Rice Estate of P. C. Weston, S.C." (1856), in Ulrich B. Phillips, ed., *Plantation and Frontier*, vols. 1–2 of John R. Commons et al., eds., *A Documentary History of American Industrial Society* (Cleveland, 1910), 1: 115–22. For a contemporary view of the absence of *pomeshchik* paternalism, see Turgenev, *La Russie et les russes*, 3: 68. Michael Confino, however, has taken an opposing position in his important study; see his *Domaines et seigneurs en Russie vers la fin du XVIII^e siècle: Étude de structures agraires et de mentalités économiques* (Paris, 1963), esp. 103–04.

⁶² See the works cited in notes 49, 53, above. Also see Zaionchkovskii, *Otmena krepostnogo prava*, 125–50; and B. G. Litvak, *Russkaia derevnia v reforme 1861 goda: Chernozemnyi tsentr, 1861–1895 gg.* (Moscow, 1972). Emancipation in Central European countries, where seigneurs generally continued to maintain their class prerogatives, served as a very different kind of example to the Russian lords from that provided for American planters by the West Indian emancipation. On the Russian government's interest in Central European agrarian reform measures, see Blum, *The End of the Old Order*, 368.

USEFUL CONCLUSIONS CAN BE DRAWN from both the similarity of the Russian and American defense of servitude and the difference in the development of that defense. The logic of defending unfree labor produced similar arguments—and similar assumptions about the nature of the good society—despite major cultural and economic differences between Russia and the United States and despite unique features of American slavery and Russian serfdom that might have influenced the nature of the defense. In short, slave-owning classes shared certain important assumptions that cut across variations in region, culture, and economy.⁶³

Nevertheless, these variations operated to accentuate the defense of bondage in the United States and to undercut it in Russia. Racial, political, cultural, and demographic differences acted to produce two contrasting master classes: one that was independent, autonomous, and socially dominant, and another that was dependent, timid, and socially marginal. While the antebellum South was a slaveholders' world, prereform Russia was a peasant world. As a consequence, the former was able to sustain a proslavery ideology that was unique in volume, subtlety, and sophistication, while the latter developed arguments that were never as finely wrought and that gradually lost their force and persuasiveness. The Russian defenses of serfdom were more obviously self-serving and lacked the boldness, imagination, sweep, and abstraction of their American counterparts. Quite simply, Russians did not make as compelling a case for their form of bondage.⁶⁴

If analysis of the contrasting courses of master class ideologies helps us understand better the impact of specific social and cultural attributes on systems of forced labor, it also raises a more general question: how will beneficiaries of a particular social system—in this instance, slaveowners—behave when that system is threatened? Although a dominant class can be expected to defend its prerogatives, this defense is likely to be more stubborn and persistent in some circumstances than in others. I would suggest that, in general, commitment to a social system will be greatest when an entire way of life rather than simply an investment is at stake. (Note, for example, how little resistance was offered by planters to compensated emancipation in the British West Indies, where plantations were pre-eminently speculations for absentee investors.⁶⁵) Ultimately,

⁶³ See Genovese, *The World the Slaveholders Made*, pt. 1, esp. 3–7, 112; and Davis, *The Problem of Slavery in Western Culture*, 29–121, 223–61.

⁶⁴ There is reason to believe that the United States—not Russia—was unusual here. For the suggestion that the antebellum South, as the most paternalistic of New World slave societies, developed the most elaborate and well-rounded defense of slavery, see Genovese, *The World the Slaveholders Made*, pt. 1, esp. 100–03. For the failure of Jamaican planters to develop the argument that slavery was a positive good, see Philip D. Curtin, *Two Jamaicas: The Role of Ideas in a Tropical Colony, 1830–1860* (Cambridge, Mass., 1955), 62–69.

⁶⁵ See Lowell Joseph Ragatz, *The Fall of the Planter Class in the British Caribbean, 1763–1833: A Study in Social and Economic History* (London, 1928), 3–80, 453–54; Curtin, *Two Jamaicas*, 15–18, 55, 62–69, 92–95; and George M. Fredrickson, "After Emancipation: A Comparative Study of White Responses to the New Order of Race Relations in the American South, Jamaica, & the Cape Colony of South Africa," in David G. Sansing, ed., *What Was Freedom's Price?* (Jackson, Miss., 1978), 26–80, 86. C. Vann Woodward has likewise suggested that "the end of slavery in the South can be described as the death of a society, though elsewhere it could more reasonably be characterized as the liquidation of an investment"; Woodward, "The Price of Freedom," in *ibid.*, 97.

Southern slaveowners rose to the defense of their peculiar institution—with both words and arms—because they had more than an economic interest in the outcome. Russian *pomeshchiki* failed to do so because they lacked the means and because, as they recognized, abolition raised a potential threat to their pocketbooks that, given the balance of forces, they could successfully overcome. From their own points of view, both planters and *pomeshchiki* made the “correct” decision.

AHR Forum
Marc Bloch and Comparative History

ALETTE OLIN HILL
and
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Finally, of what use is linguistics? Very few people have clear ideas on this point, and this is not the place to specify them. But it is evident, for instance, that linguistic questions interest all who work with texts—historians, philologists, etc. Still more obvious is the importance of linguistics to general culture: In the lives of individuals and societies, speech is more important than anything else.¹

FEW HISTORIANS OF THE TWENTIETH CENTURY have had as much impact upon their profession as has Marc Bloch. Although his career was cut short in 1944 (he was executed by the Germans as a member of the French Resistance), his writings have remained very much alive in subsequent years as historians have striven to expand the horizons of their discipline. Comparative history was the theme of the convention held by the American Historical Association in December 1978, and Bloch's name was frequently invoked by the participants in its programs. Yet the discussions at San Francisco revealed some ambiguity among scholars about the method and its potentialities. Consider, for example, the titles of some key sessions: "Can 'Comparative History' Be Defined?"; "How Has 'Comparative History' Been Practiced?"; and "Is There an Interdisciplinary Comparative Method?" Questions were more in evidence than answers. Some of the ambiguity stems from Bloch's own misconceptions about the model from which he derived his ideas on the comparative method—the model of historical linguistics.

IN HIS FAMOUS ARTICLE, "Pour une histoire comparée des sociétés européennes," Marc Bloch asserted that of all of the social scientists only the linguists had ac-

¹ Ferdinand de Saussure (1857–1913), *Course in General Linguistics*, ed. Charles Bally and Albert Sechehaye, trans. Wade Baskin (New York, 1966), 7. The original French edition, *Cours de linguistique générale* (1916) was compiled from student notes. For a recent criticism of structural linguistics, especially the tradition associated with Saussure, see Jacques Derrida, "Linguistics and Grammatology," in his *Of Grammatology*, trans. Gayatri Chakravorty Spivak (Baltimore, 1976), 27–73. Also see René Thom, "Topologie et linguistique," in André Haeffliger and Raghavan Narasimhan, eds., *Essays on Topology and Related Topics: Mémoires dédiés à Georges de*

curately differentiated the techniques involved in comparison.² True comparison, he declared, entails "two widely different intellectual processes."³ Bloch derived these two processes, which we will call "universal" and "historical," from the French linguist Antoine Meillet (1866–1936). In "The Definition of the Comparative Method," Meillet wrote, "There are two different ways of practicing comparison: one can compare in order to draw from comparison either *universal laws* or *historical information*. These two types of comparison, equally legitimate," he claimed, "differ absolutely."⁴

Meillet, one of the most respected and prolific scholars in comparative Indo-European linguistics during the late nineteenth and early twentieth centuries, published monographs on the grammars of the Greek, Latin, Germanic, Iranian, Old Slavonic, and, most notably, Armenian languages. In addition, he produced in 1903 a theoretical tract on the comparative study of all known Indo-European languages, *Introduction à l'étude comparative des langues indo-européennes*. A pupil of Ferdinand de Saussure, he was appointed professor of comparative Indo-European grammar at the Collège de France in 1906.⁵ His interests included sociology as well as linguistics; beginning in 1901, he collaborated with Émile Durkheim on the journal *L'Année Sociologique*.⁶

Bloch relied upon Meillet's *La méthode comparative en linguistique historique*, published in 1925, for his decision to follow the historical rather than the universal linguists.⁷ He described the two methods, or "processes," as follows:

Rham (Berlin, 1970), 226–27. For a convincing argument as to why historians should acquaint themselves with linguistics, see Nancy S. Struever, "The Study of Language and the Study of History," *Journal of Interdisciplinary History*, 4 (1974): 401–15.

² Bloch, "Pour une histoire comparée des sociétés européennes," paper delivered at the Sixth International Congress of Historical Sciences, held in Oslo, August 1928, and printed in *Revue de synthèse historique*, 46 (1928): 15–50. There are two English versions: "A Contribution Towards a Comparative History of European Societies," trans. J. E. Anderson, in Bloch, *Land and Work in Mediaeval Europe: Selected Papers by Marc Bloch* (New York, 1969), 44–81; and "Toward a Comparative History of European Societies," trans. Jelle C. Riemersma, in Frederic C. Lane and Jelle C. Riemersma, eds., *Enterprise and Secular Change: Readings in Economic History* (Homewood, IL, 1953), 494–521. Anderson's translation in *Land and Work in Mediaeval Europe* contains Bloch's original footnotes, and for that reason is generally more useful for the student. The Lane-Riemersma edition in *Enterprise and Secular Change*, however, has an excellent introduction demonstrating the influence of Émile Durkheim on Bloch's thought, and the editors have also provided the reader with a concise outline of Bloch's paper, "Toward a Comparative History," trans. Riemersma, 489–93, 494.

³ Bloch, "Towards a Comparative History," trans. Anderson, 45, but see "Toward a Comparative History," trans. Riemersma, 495 ("Pour une histoire comparée," 16).

⁴ Meillet, *The Comparative Method in Historical Linguistics*, trans. Gordon B. Ford, Jr. (Paris, 1967), 13 (italics added). We have labeled Type I "universal" and Type II "historical" because Bloch clearly relied upon Meillet for his use of two types of comparative analysis, and Meillet used these two terms, "universal" and "historical." It seemed to us, moreover, that a descriptive label, in addition to "Type I" and "Type II," would aid the reader in following the argument throughout the article. At present, most linguists would not find the term "universal" appropriate; they prefer "typological linguistics," although they do have occasion to use the term "universal grammar." See Joseph H. Greenberg, *Language Universals, with Special Reference to Feature Hierarchies* (The Hague, 1966), *Language Typology: A Historical and Analytic Overview* (The Hague, 1974), and, especially, *Universals of Human Language*, 4 vols. (Stanford, 1978).

⁵ For a full account of Meillet's achievements, see the obituary by Joseph Vendryes, "Antoine Meillet," in Thomas A. Sebeok, ed., *Portraits of Linguists: A Biographical Source Book for the History of Western Linguistics, 1746–1963*, 2 (Bloomington, Ind., 1966): 201–40. For Saussure's influence on Meillet, especially see *ibid.*, 215–16, 217.

⁶ Alf Sommerfelt, "Antoine Meillet, the Scholar and the Man," in Sebeok, *Portraits of Linguists*, 241–49; for Meillet's connection with Durkheim, see *ibid.*, 245. Also see Vendryes, "Antoine Meillet," *ibid.*, 223–24.

⁷ Bloch, "Towards a Comparative History," trans. Anderson, 76 n. 3 ("Pour une histoire comparée," 16 n. 1).

Type I (Universal Comparison): "The historian selects some societies so widely separated in time and space that any analogies observed between them with respect to such and such phenomena can obviously not be explained either by mutual influence or by a common origin."⁸

Type II (Historical Comparison): "This is to make a parallel study of societies that are at once neighbouring and contemporary, exercising a constant mutual influence, exposed throughout their development to the action of the same broad causes just because they are close and contemporaneous, and owing their existence in part at least to a common origin."⁹ In history proper, this is the equivalent of the historical study of languages (for example, Indo-European languages); whereas comparative history in the broad sense would more or less correspond to linguistics in general. In both history and language, it appears true that of these two comparative methods the one with the more limited horizon is also the richer in results. Because it is more capable of rigorous classification, and more critical about the objects it compares, it may hope to reach conclusions of fact that are less hypothetical and more precise. This at any rate is what I hope to be able to show; for it is certainly this method that I intend to elaborate."¹⁰

As an example of Type I comparison Bloch used Sir James Frazer's study of the cult of Diana in Roman times, which at Lake Nemi involved the ritual murder of the priest officiating at the temple. Frazer sought out similar rites among "barbarous" peoples at other places and times with a view to establishing a common psychological trait that would make plausible the religious rite at Nemi.¹¹

As described by Bloch and illustrated by his choice of Frazer's example, Type I need not involve any genetic relationship among the things compared. From the point of view of the historical linguist, this sort of comparison would be virtually profitless. One can compare the Japanese and English languages, for instance, and find borrowings dating from fairly recent times that would reflect cultural interpenetration, but a comparison in the ultimate sense would yield negative results since the two languages do not have a common origin.¹² What

⁸ Bloch, "Towards a Comparative History," trans. Anderson, 46 ("Pour une histoire comparée," 17).

⁹ Bloch's phrase here, "in part at least to a common origin" ("partiellement du moins, à une origine commune"), seems unnecessarily qualified considering that a common origin is a basic assumption in historical linguistics. See Paul Thieme, "The Comparative Method for Reconstruction in Linguistics," in Dell Hymes, ed., *Language in Culture and Society: A Reader in Linguistics and Anthropology* (New York, 1964), 585-98; and the references in note 12, below.

¹⁰ Bloch, "Towards a Comparative History," trans. Anderson, 47-48 ("Pour une histoire comparée," 19-20).

¹¹ Bloch, "Towards a Comparative History," trans. Anderson, 46 ("Pour une histoire comparée," 18-19).

¹² See Meillet, *The Comparative Method in Historical Linguistics*, 19; and Leonard Bloomfield, *Language* (New York, 1933), chap. 18: "The Comparative Method," 297-320. For a more recent statement, see Henry M. Hoenigswald, *Language Change and Linguistic Reconstruction* (Chicago, 1965), 68-71, 119-43. We are indebted to Henry Hoenigswald for his helpful suggestions and the references he placed at our disposal. Also see Thieme, "The Comparative Method for Reconstruction in Linguistics," 585-98.

Frazer sought was a common human substratum or deep structure of mentality in the Chomskyan sense—in short, a universal trait with world-wide application.¹³ The search for language universals is a valid one, but the significance of the few that have been identified is questionable. In spite of their successes, modern linguists have significant problems to settle before their methodology can be recommended without reservation. Not too long ago, W. V. Quine criticized Noam Chomsky's lack of "explicitness of criteria and awareness of method" in crediting native speakers with a "precise sense of grammaticality." "The problem of evidence for a linguistic universal," Quine concluded, "is insufficiently appreciated."¹⁴

Bloch rejected universal comparisons of Type I in favor of historical comparisons of Type II. In the article of 1928 on "*histoire comparée*," he wrote, "I propose to compare the various European societies—especially in Eastern and Central Europe—societies that are contemporary, that live close to one another, and that go back if not to one common origin, at any rate to several."¹⁵ If by "Eastern and Central Europe" Bloch meant the German, Slavic, and Hungarian societies, he proposed to study two groups that descended from a common linguistic ancestor (the Germans and the Slavs) and one group (the Hungarians) that has had a separate linguistic history. The German and Slavic languages have a "common origin," Proto-Indo-European, whereas Hungarian belongs to an entirely unrelated linguistic family, Finno-Ugrian. (There is, however, no *necessary* connection between language and ethnic origin.¹⁶)

Bloch's research proposal was of a sort familiar to comparative linguists (that is, those whose methods are Type II), accounting for similarities between the Slavs and the Germans on the basis of a genetic relationship. As spoken by families long resident in the area adjacent to Austria, Hungarian would doubtless show evidence of German borrowings, and similarly Hungarian institutions would be expected to reflect Austrian practices and forms. One might assume that in this case Bloch would search out what was native to Hungarian soil and what was merely borrowed from German-speaking Austrians. In studying the earliest institutions of German- and Slavic-speaking areas, Bloch would presumably find that some similarities could be accounted for by a common origin, since the speakers of Slavic and German were at one time members of the same tribe—the Indo-Europeans. This assumption is based upon the structuralists' theory that all institutions behave as languages do.¹⁷

¹³ Noam Chomsky effected a revolution in linguistics with the publication of his *Syntactic Structures* in 1957. For the nature and significance of his work, see Gilbert Harman, ed., *On Noam Chomsky: Critical Essays*, Modern Studies in Philosophy, gen. ed. Amelie Oksenberg Rorty (Garden City, N.Y., 1974).

¹⁴ Quine, "Methodological Reflections on Current Linguistic Theory," in Harman, *On Noam Chomsky*, 104–17, esp. 108–09.

¹⁵ Bloch, "Towards a Comparative History," trans. Anderson, 48 ("Pour une histoire comparée," 20).

¹⁶ For a forceful corrective to the older historiography, which tended to confuse language, race, and nation and to oversimplify East-West relations, see Frantisek Graus et al., *Eastern and Western Europe in the Middle Ages*, History of European Civilization Library, gen. ed. Geoffrey Barraclough (London, 1970). In his Introduction, Barraclough cited Ranke and pinpointed the use of linguistic divisions in the study of history as one of the main reasons for the assumed superiority of Western over Eastern Europe and the stigmatization of the Slavs as "silent spectators"; *ibid.*, 7. Also see *ibid.*, 22–25.

¹⁷ Michaël Lane, ed., *Introduction to Structuralism* (New York, 1970), 18. Also see Spivak, Preface to Derrida, *Of Grammatology*, liv–lxvii, and Derrida, *Of Grammatology*, chap. 2: "Linguistics and Grammatology," 27–73.

Although Bloch planned to study societies, not languages, Meillet's techniques were appropriate to his purpose. But, when he shifted his discussion to the comparative study of feudalism, he demonstrated the limits of his understanding of those techniques:

If I am studying the manorial system in the Limousin, I shall be continually impelled to consider setting side by side information drawn from other manors; in common or garden language, I shall be comparing them. But I shall not consider myself to be engaged in what is technically called "comparative" history, for I shall be taking the different objects studied from a cross-section of a single society in which, looked at as a whole, there is a considerable degree of unity. In practice, it has become customary to reserve the term "comparative history" almost entirely for the comparative examination of phenomena that have taken place on different sides of a State, or national, frontier.¹⁸

Contemporary linguists did not differentiate so sharply between local and international units of comparison. Dialect geographers were building their atlases exactly as Bloch described for his survey of manors in the Limousin.¹⁹ One of the most important of these was Jules Gilliéron (1854–1926), whose work Meillet greatly admired.²⁰ Dialect geography was quite as respectable and just as important on the small-scale level of, say, a canton in Switzerland as on the large-scale level of Indo-European, a linguistic territory that even in medieval times stretched from Iceland to the Indian subcontinent. Some linguists have argued that the fundamental work comes at the finest level of the grid and that the comprehensive comparison of dialects and then of languages can only be undertaken with a degree of accuracy and expectation of success when the meticulous work of collecting local speech patterns has been accomplished.²¹ In actual prac-

¹⁸ Bloch, "Towards a Comparative History," trans. Anderson, 45–46 ("Pour une histoire comparée," 17).

¹⁹ For a discussion of the problem of differentiating dialects from languages, see Henry M. Hoenigswald, "Criteria for the Subgrouping of Languages," in Henrik Birnbaum and Jaan Puhvel, eds., *Ancient Indo-European Dialects: Proceedings of the Conference on Indo-European Linguistics Held at the University of California, Los Angeles, April 25–27, 1963* (Berkeley and Los Angeles, 1966), 1–12. Furthermore, Hoenigswald pointed out that "the criteria of subgrouping have constituted one of the most frequently discussed topics in comparative linguistics from the middle of the last century to this day"; *ibid.*, 1 n. 1.

²⁰ See Mario Roques, "Jules Gilliéron," in Sebeok, *Portraits of Linguists*, 65–73. Meillet's enthusiasm for dialect geography in general and for Gilliéron in particular can be found in the words of Joseph Vendryes, Meillet's pupil and colleague: "Meillet a été des premiers ... à comprendre ce qu'il y avait de génial dans les idées de Gilliéron. Il a suivi et encouragé ses travaux; il en a lui-même profité pour perfectionner sa propre méthode. Il s'en est inspiré en effet pour écrire son article sur *les effets de l'homonymie dans les langues indo-européennes* ... et l'influence s'en manifeste aussi dans son livre sur les *Dialectes indo-européens*"; "Antoine Meillet," 220. Although Gilliéron was devoted to comparisons on the small scale, Meillet used these principles to study the whole corpus of Indo-European.

²¹ Robert A. Hall, Jr., "The Reconstruction of Proto-Romance," *Language*, 26 (1950): 6–27. Hall recommended a more accurate description of local areas before the work of comparison should begin in earnest, and this sort of description has had strong advocates among historians. See Pierre Goubert, "Local History," *Daedalus: Journal of the American Academy of Arts and Sciences*, 100 (1971): 113–27, reprinted in *Historical Studies Today*, Proceedings of the American Academy of Arts and Sciences (New York, 1972), 300–14. Goubert cited a number of examples demonstrating that regional and provincial studies in France had successfully challenged historical generalizations. One example must suffice in this context: "The growth of the seignorial system (which the Marxists call feudal) in France seemed to be quite common; an edict by Louis XIV was sometimes cited to show how the last allodial tenures fared under that king. R. Boutruche, in his study on allodial tenure in the Bordeaux region, has drawn our attention to the fact that completely free lands and peasants sometimes survived the Middle Ages. In his book on the Lower Auvergne, Abel Poitrineau shows, on the basis of notarial documents, that the proportion of allodial tenures sometimes approached 50 per cent in many rural communities of eighteenth-century Auvergne. This phenomenon will probably be found to have been common in other parts of the country as well (mainly in central, eastern, and southern France). The result of these many

tice Bloch's comparisons of local manors and of much larger units were equally impressive. Bloch made it clear that he did not consider comparative history to be mainly a matter of comparing states, an approach that he called "a gross simplification."²² Yet he did not stay within the Type II comparison, either in practice or in statements of theory. Once he had distinguished universal comparison as practiced by James Frazer from the historical comparison of Meillet, he went on to develop his own form of comparison, which differed markedly from that of Type II. Later in his career he appeared to come back to Type II, but he apparently never made his preference clear. The attraction of Type II comparison—that of historical linguists since the Neogrammarians of Leipzig in the 1870s—proved irresistible to Bloch because of its epoch-making results, but he did not practice it consistently, ostensibly because he did not compare languages or dialects but other social entities.²³

Bloch's lack of rigor in following the linguistic model of Meillet can be explained by his belief that "the history of social organisation is . . . a much more difficult problem" than comparable studies in linguistics; by contrast he called the "problem of linguistic affiliations" relatively simple.²⁴ And he cited Meillet on the sharp distinction between language and history:

"Up to now," writes Meillet, "no case has been discovered giving cause for the belief that the morphological system of a given language is the result of the intermingling of the morphologies of two distinct languages. In all cases so far observed, a language presents a continuous tradition," whether this tradition be "of the current type—the transmission of the language from the adults to the children"—or whether it arises "from a change of language." But let us suppose that at a given moment examples are discovered of this phenomenon so far unknown—"real mixtures" between languages. When that happens (I am still quoting M. Meillet) "linguistics will have to devise new methods." Now the historian of societies finds that the facts themselves impose upon him this formidable hypothesis of "mixtures," which, if realised in the linguistic field, would be such a disturbing influence in that humane science which is rightly most self-confident.²⁵

Meillet insisted that the success of the comparative method presupposed a common origin. Furthermore, he said that the method also depended upon the "arbitrary character of the sign."²⁶ Bloch did not discuss this second, fundamental

researches is the inescapable (but unanticipated) conclusion that a part of France during the Ancien Régime (and, by definition, the Middle Ages as well) did not undergo the seignorial system." *Ibid.*, 118. One can cite examples from Bloch's own writings to support this contention—for example, the need for detailed studies in the context of his definition of "European feudalism"; Bloch, *Feudal Society*, vol. 2: *Social Classes and Political Organization*, trans. L. A. Manyon (1961; Chicago, 1964), 446. For Bloch's recognition of the survival of allodial lands, see his "The Rise of Dependent Cultivation and Seignorial Institutions," in M. M. Postan, ed., *The Cambridge Economic History of Europe*, vol. 1: *The Agrarian Life of the Middle Ages* (2d ed., Cambridge, 1966), 270.

²² Bloch, "Towards a Comparative History," trans. Anderson, 46 ("Pour une histoire comparée," 17: "c'est là une simplification un peu grosse" [italics added]).

²³ For a summary of the history of linguistic development before and after the "revolution" of the Neogrammarians at Leipzig, see Holger Pedersen, *The Discovery of Language: Linguistic Science in the Nineteenth Century*, trans. John Webster Spargo (Bloomington, Ind., 1962), chap. 7: "The Methods of Comparative Linguistics: A Survey of Their Development," 240–310.

²⁴ Bloch, "Towards a Comparative History," trans. Anderson, 68 ("Pour une histoire comparée," 41).

²⁵ Bloch, "Towards a Comparative History," trans. Anderson, 68 ("Pour une histoire comparée," 41). Bloch drew here on Meillet's *La Méthode comparative en linguistique historique*, 82, 83; see Bloch, "Towards a Comparative History," trans. Anderson, 80 n. 29 ("Pour une histoire comparée," 42 n. 1).

²⁶ Meillet, *The Comparative Method in Historical Linguistics*, 14: "Only the totally arbitrary character of the sign makes possible the historical and comparative method which will be studied here."

aspect of the subject in this context.²⁷ If Meillet's common origin and the "arbitrary character of the sign" are both essential to the proper exercise of Type II comparison, then it appears incongruous for Bloch to have endorsed Meillet's method so strongly as a proper model for the historian. To assert, as Bloch did, that the subject matter of language affiliation is endowed with simplicity is not justified.²⁸ An examination of the studies of those linguists who have dealt with the problems of comparison, including how change takes place, reveals that there is nothing simple about it. Whereas a morpheme may be a *smaller* unit than an institution, the complexity of interpretation is not necessarily less. Whoever peruses the various types of phonetic, analogic, and semantic change in Leonard Bloomfield's *Language* or the intricate formulae in Henry M. Hoenigswald's *Language Change and Linguistic Reconstruction* will not readily conclude that simplicity inheres in the study of "linguistic affiliations."

WHAT OUGHT TO BE COMPARED IN ANY STUDY that claims to follow the method used in comparative historical linguistics is *all and only* the phenomena in a related group—for example, the Romance languages. One would not make a comparison of French and Italian, or Spanish and Rumanian, and hope to reconstruct an accurate version of Proto-Romance. Only a study of all of these languages, integrating the background data on the dialects of each, would begin to yield important results.²⁹

A classic example of the fallacy of simply comparing two languages was provided by Ferdinand de Saussure in his *Course in General Linguistics*, in which he cited certain forms of Latin *genus*, Greek *génos*, and Sanskrit *ganas*, all of which mean "race" (see Table 1).³⁰ These words are clearly cognate, for they correspond to a high degree in both phonemic components and in semantic value.

²⁷ In a short article, Bloch showed that he was aware of this concept in linguistics, but he did not explain how it might be applied in comparative history: "Enfin—M. Meillet, entre autres, a fortement insisté sur ce dernier trait—les notations du langage sont, en règle générale, purement arbitraires; il n'y a pas de raison intrinsèque pour que 'beau' exprime la beauté et 'laid' la laideur. Le nombre des combinaisons de cette espèce étant, en pratique, à peu près illimité, il apparaît clairement que les concordances linguistiques ne peuvent que très rarement s'expliquer par le hasard; elles doivent, pour la plupart, être interprétées comme la preuve, soit d'influences réciproques, soit d'une origine commune; or c'est, nous le verrons, surtout à retracer les filiations des langues que s'est, pendant longtemps, appliquée la linguistique comparée." Bloch, "Comparaison," *Revue de synthèse historique*, 49 (1930): 32–33.

²⁸ Bloch seemed ambivalent on the point of simplicity, for elsewhere he said, "We must recognise that the facts of agrarian life are no tidier than those of language: just as there are always certain forms and usages which straddle dialect boundaries, so there are no geographical areas whose limits are exactly coterminous with one particular set of agrarian forms and techniques." See Bloch, *French Rural History: An Essay on Its Basic Characteristics*, trans. Janet Sondheimer (Berkeley and Los Angeles, 1966), 62. The French edition, *Les Caractères originaux de l'histoire rurale française* (1931), originated in a series of lectures Bloch delivered at the Institute for the Comparative Study of Civilisations in Oslo in 1929 and recast into book form.

²⁹ For an eloquent case for the necessity of strictly applying the comparative method and the pitfalls of using it superficially, see Hall, "The Reconstruction of Proto-Romance," 6–27.

³⁰ In his biography of Saussure, Jonathan Culler has reproduced this example, and, where Saussure gave the locative plural (instead of the nominative plural) in Sanskrit as *ganasu*, Culler has substituted *ganassu*; Culler, *Ferdinand de Saussure* (Harmondsworth, Eng., 1977), 50. Culler may have taken this spelling from the variorum edition of Saussure's work: Saussure, *Cours de linguistique générale*, ed. Rudolf Engler (Wiesbaden, 1967), 5, 6. The form *ganassu* should have been supplied with an asterisk since it is a reconstruction. The attested locative plural is *ganahsu*. For a complete paradigm of this type of noun in Sanskrit, see William Dwight Whitney, *Sanskrit Grammar* (Cambridge, Mass., 1955), 154–55.

TABLE 1
Latin, Greek, and Sanskrit Cognates for "Race"

<i>Case</i>	<i>Latin</i>	<i>Homeric Greek</i>	<i>Sanskrit</i>
SINGULAR			
NOMINATIVE	genus	génos	ḡanas
GENITIVE	generis	géneos	ḡanasas
DATIVE	generi	génei	
LOCATIVE			ḡanasi
PLURAL			
NOMINATIVE	genera	génea	ḡanāṅsi
GENITIVE	generum	géneōn	ḡanasām

SOURCE: Ferdinand de Saussure, *Course in General Linguistics*, trans. Wade Baskin (New York, 1966), 2.

NOTE: We have supplied the nominative plural of Sanskrit *ḡanas* (*ḡanāṅsi*) where Saussure gave the locative, *ḡanasu*, because he cited the Latin and Greek in their nominative forms (*genera* and *génea*). Saussure probably omitted the Sanskrit nominative plural because it contains an intrusive nasal. Actually, he cited the locative plural incorrectly: it should be *ḡanaḥsu*. (Perhaps we should blame his students for their inattention rather than the master; see note 1, above.)

Yet we would be misled about their morphological development if we were to examine the Greek and Latin evidence only and omit the Sanskrit. In the genitive singular, for example, the Latin has an interior /r/, whereas the Greek has a zero phoneme. With only these two languages before us, we would assume either that the Greek had lost an original /r/ or that the Latin had infixes one. Neither of these phenomena is attested for the phonological history of these languages. But, when we consider the Sanskrit genitive singular, *ḡanasas*, the puzzle is solved: the /s/ is an Indo-European retention of the stem consonant, which was rhotacized in Latin—that is, the /s/ became an /r/—and was lost in Greek. Both of these phenomena—rhotacism of intervocalic /s/ in Latin and loss of intervocalic /s/ in Greek—are attested, and numerous examples of each prove that the solution to the problem lies in the assumption of an original Indo-European /s/ where Latin has /r/ and Greek has zero.

Such cases are the norm, not the exception, in the reconstruction of Indo-European dialect interrelations. Another example is "Grassmann's Law."³¹ Taking the same word discussed above, the Germanic equivalent has an initial /k/ instead of /g/, as, for example, the English *kin*. Where Greek and Latin have a /g/ (that is, a voiced stop), English has a voiceless stop. Proto-Germanic, the ancestor of English, underwent a change called the First Sound Shift, in which all of the voiced stops, not just /g/, were unvoiced: /g/ > /k/; /d/ > /t/; and /b/

³¹ See Bloomfield, *Language*, 349–51. Hermann Grassmann (1809–77) published his findings in 1862; *ibid.*, 349.

> /p/.³² Similarly, the voiced aspirated stops became deaspirated stops: /bh/ > /b/; /dh/ > /d/; and /gh/ > /g/. Again, Sanskrit provides the evidence: Sanskrit *bhrātā* ("brother"), where Sanskrit retains the PIE aspirate, and English *brother*, where the aspiration is absent; the Sanskrit root *dhā-* ("place," "put") and English *do*; and Sanskrit *grabh* ("grasp") and English *grab*. The evidence from Greek and Latin, as well as from Germanic and Sanskrit, is overwhelming that indeed the aspirated voiced stops of Proto-Indo-European lost their aspiration in the Germanic group, becoming simple voiced stops.

Yet certain words that seem to be cognate defy this rule, which was a part of Grimm's Law describing the First or Germanic Sound Shift. Hermann Grassmann derived a corollary, which he could not have done had he restricted himself to the study of two languages. The Neogrammarians of Leipzig in the 1870s assumed that language change was regular in the highest degree, and, if a portion of the linguistic evidence did not fit the formula, this deficiency was not a reason to ignore the evidence or to label it anomalous. A way had to be found to restate the rule so that all examples would be subsumed under it. What Grassmann did was to look at the cases of Germanic, simple voiced stop consonants that would all theoretically go back to an original Indo-European voiced aspirated stop, like *do* in relation to *dhā-*. In the cases in which English /d/ was paralleled not by /dh/ but by /d/, he found that all of them occurred in the context of another aspirated voiced stop. For example, English *bid* is cognate with Sanskrit *bodh-* ("make known") instead of **bhodh-*. The initial plain, unaspirated stop should be an aspirate in Sanskrit—/bh/—just as the final consonant of English *bid* is paralleled by the final consonant /dh/ in the Sanskrit root. Since there is an observable rule in Sanskrit that two aspirates cannot stand in close proximity, Grassmann suggested that the initial consonant had indeed been aspirated originally—a /bh/—and that Sanskrit had deaspirated it. Greek has a similar rule, or a parallel pattern of phonemic change: two aspirates cannot stand in this way; one of them is deaspirated. Thus, Grassmann reformulated a part of Grimm's Law to cover these cases: the Indo-European voiced aspirated stops become deaspirated in Sanskrit and Greek, if they stand in the syllable adjacent to another aspirated stop. This theory cleared up a whole group of mystifying cognates in which the Germanic evidence seemed to conflict with the Sanskrit and the Greek.

If Grassmann had confined himself to Sanskrit and Greek alone, he might not have discovered this important exception to Grimm's Law. Germanic evidence, exemplified by English among others, retained a simpler reflex of the original development of these phonemes than did Sanskrit or Greek, both of which were generally thought to preserve more of the ancient features of the parent language. Thus, the comparison of only two of a related group of languages is unnecessarily restrictive and will possibly lead to fallacious conclusions.

³² The symbol ">" means "becomes" or "results in." The First or Germanic Sound Shift is also known as "Grimm's Law," after Jakob Grimm (1787–1863), who published his statement on consonant relations in 1822, in the second edition of his *Deutsche Grammatik*; Bloomfield, *Language*, 14.

In his article on "histoire comparée" in 1928, Bloch espoused the Type II method of the linguists in preference to the Type I search for universals, but he did not show that he fully understood Type II. For example, he did not make it clear that *all* societies with a similar institution must be studied if there is reason to suspect a genetic relationship among them. Instead, he spoke of studying "neighbouring" societies. To be sure, neighboring societies would doubtless show cultural similarities, but it might be possible to find a society at some distance that had a similar institution—for example, the *rājā* of ancient India, the *rex* of ancient Rome, and the **rīgs* of Celtic-speaking Europe. Since these words are cognate—that is, they derive from a single Proto-Indo-European root—comparing them as royal institutions would be plausible, even though, at the present time, the cultures that inherited these terms have spread apart and are no longer "neighbouring" in the geographical sense.

Bloch said, moreover, that he wished to trace the institutions under consideration "if not to one common origin, at any rate to several." The Neogrammarians of Leipzig, who are credited with the major breakthrough in comparative historical linguistics in the nineteenth century, operated upon the assumption that related forms should lead back to a single form, not to several. Bloch had absorbed their premise of the goal of reconstruction, but he did not appear to insist upon the assumption of a single origin.³³

Finally, in Bloch's own work there are cases in which he compared institutions in only two societies—for example, in his study of the *ministeriales* in France and Germany and of the so-called *rois thaumaturges* in France and England.³⁴ It could be argued that in each of these works the greater part of the evidence was confined to two societies and, therefore, Bloch was indeed using "all and only" the pertinent data. Most linguistic comparisons, however, involve a great many more groups, whether at the level of dialect or of language. One wonders if these fine works by Bloch—one an article, the other a book—did not perhaps set a precedent for his successors to confine their studies to two societies, rather than "all and only."

Marc Bloch's remarks on the methodology of comparative history lack precision and coherence. One can only conclude that his conflicting statements derive from his admiration for linguistic research and a regrettable lack of time to study its full implications. So great was Bloch's influence on subsequent scholarship that the flaws in his statements on the value of the linguistic model have

³³ That the goal of reconstruction was a *sine qua non* of the Neogrammarians, as opposed to the less rigorous comparison of preceding linguists at Leipzig and elsewhere, is well known and can be established by reference to any number of scholars who endorsed it—among them, Saussure and Meillet. One prominent American who studied at Leipzig in the late 1880s and early 1890s was Carl Darling Buck (1866–1955). In the preface to his monograph devoted to the vowels of Oscan, an Italic dialect, he insisted that reconstruction, not mere comparison, was the final goal of the historical linguist. Buck was one of the first faculty members of the then newly founded University of Chicago, and his influence was felt by many later scholars in the United States. See George S. Lane, "Carl Darling Buck," in Sebeok, *Portraits of Linguists*, 265–77, esp. 270.

³⁴ Bloch, "A Problem in Comparative History: The Administrative Classes in France and in Germany," trans. J. E. Anderson, in Bloch, *Land and Work in Mediaeval Europe*, 82–123, and *The Royal Touch: Sacred Monarchy and Scrofula in England and France*, trans. J. E. Anderson (London, 1973).

been repeated in the work of his disciples.³⁵ A methodological tradition has developed that needs to be critically examined.

IN THE EDITORIAL PREFACE TO THE FIRST ISSUE of the distinguished journal *Comparative Studies in Society and History*, Sylvia L. Thrupp applauded the historical linguists for their methodology and then advised future contributors to "select whatever approach to comparative study" that appeared "most appropriate to their purposes. Some may prefer the approach through testing or application of theory which has often been identified with 'the comparative method.' Where theory is precise, and there is a limited body of data of such a character that all of it can be rigorously checked from the point of view of each competing theory, this is undeniably the best plan. Its success in historical linguistics triumphantly proves the point. Very few of the recurrent cultural problems, however, present themselves under these conditions." Yet the "triumph" of the historical linguists was not owed to the simplicity or the uniformity of their subject matter but to the precision of the method they developed. Thrupp went on to suggest two other approaches: she rejected the "purely empirical" as leaving too free a rein to "personal idiosyncrasy," but she recommended "a third or mixed" approach as probably appropriate for the majority of contributors, "drawing to some extent on theory as a means of control over empirical observation."³⁶

In these instructions, Sylvia Thrupp defined neither "theory" nor "the comparative method," leaving these abstractions to the contributor's imagination. It need not be assumed, however, that the methods worked out by linguists are inappropriate for social scientists because of the intractability of the subject matter. Henry Sumner Maine (1822–88) was so impressed by the work of comparative philologists that he put into practice some of their principles in his studies of ancient jurisprudence, works that are still respected by both historians and anthropologists.³⁷ Claude Lévi-Strauss took the theory of phonemic struc-

³⁵ J. Ambrose Raftis, "Marc Bloch's Comparative Method and the Rural History of Mediaeval England," *Mediaeval Studies*, 24 (1962): 349–68, esp. 351. Raftis observed that Bloch was "never able to leave us a complete study of his methodology for comparative history," yet he asserted that a methodology could be obtained by reading Bloch's articles in *Annales* during the 1930s. The overwhelming number of articles on Bloch have made a similar claim in order to give Bloch's inconsistencies a specious coherence.

³⁶ Thrupp, "Editorial," *Comparative Studies in Society and History: An International Quarterly*, 1 (1958): 2–3.

³⁷ For two of his principal works, see Maine, *Ancient Law: Its Connection with the Early History of Society and Its Relation to Modern Ideas* (London, 1861), and *Village-Communities in the East and West: Six Lectures Delivered at Oxford, to Which Are Added Other Lectures, Addresses, and Lectures* (New York, 1876). For an illuminating evaluation of Maine's work, see Kenneth E. Bock, "Comparison of Histories: The Contribution of Henry Maine," *Comparative Studies in Society and History*, 16 (1974): 232–62. Bock disproved the commonly held notion of Maine as an "evolutionist" by demonstrating his admiration for the comparative philologist Max Müller, who was Maine's contemporary (1823–1900). Bock did not, however, point out that Müller's work was erroneous, although he did refer the reader to an important paper by Carl Darling Buck for "an informative discussion of the implications of comparative grammar for other studies." For Buck's article, see his "The Relations of Comparative Grammar to Other Branches of Learning," in Howard J. Rogers, ed., *History of Language, History of Literature, History of Art*, vol. 3 of *Congress of Arts and Science: Universal Exposition, St. Louis, 1904* (Boston, 1906), 32–52. Maine might well have contributed more to the integration of linguistic theories into the field of ancient history if he had had at his disposal the reliable work of Buck and other students of the Neogrammarians rather than only the dubious work of Müller, who was considered a popularizer at best; unfortunately, Maine's career barely overlapped the introduction of superior methods by the linguists at Leipzig.

ture and applied it to kinship relations, thereby achieving distinction in the field of ethnology.³⁸ More recently, linguistic techniques have been applied by a group of French psychoanalysts led by Jacques Lacan, who had come to the conclusion that the structure of the unconscious was the structure of language.³⁹

J. G. A. Pocock has demonstrated a profound appreciation of linguistic problems in the interpretation of historical data:

It needed only the step—which a historian should take instinctively—of viewing “language” as a product of history and as possessing history of its own, to reach the point where it could be seen, first, that the exploration of language might yield historical results, might produce second-order statements about languages used which would be historical statements; second, that this activity could be considered a historical agent, helping to produce changes in linguistic consciousness and so in the history of language-use itself.⁴⁰

In his review of the first volume of *History and Theory*, Pocock declared, “Since the historian’s mode of thinking, his assumptions, his problems, possibly even his logic, vary from tradition to tradition, period to period, and society to society, he must cease to be a *confère* whose problems we share and become a phenomenon whose location and behaviour we study; and this study may be undertaken as a branch of comparative history.”⁴¹ Interestingly enough, although Pocock referred explicitly to linguistics and to comparative history, he did not make the leap that Lévi-Strauss was bold enough to try—namely, the application of a method used by the linguists to the subject matter of history. And again we are left uncertain as to what “comparative history” and its connection to linguistics really consist of. Pocock’s own practice of comparative history as reflected in “The Origins of Study of the Past: A Comparative Approach” owes nothing to the historical linguists, nor does he allude to them as the source of his inspiration.⁴²

A recent study of Marc Bloch and the comparative method stresses the logic of comparatism, but its author, William H. Sewell, Jr., noted the following limitations on the application of this logic:

The most important limit of the comparative method is not that it can be applied only when we are trying to explain phenomena which transcend the boundary of any single social system, but that it aids us only in one step, and that the easiest and most mundane step, of the explanatory process. *The comparative method is a method, a set of rules which can be methodically and systematically applied in gathering and using evidence to test explanatory hypotheses.*

³⁸ Lévi-Strauss, “Language and the Analysis of Social Laws,” in his *Structural Anthropology*, trans. Claire Jacobson and Brooke Grundfest Schoepf (Garden City, N.Y., 1967), 54–65.

³⁹ Jan Miel, “Jacques Lacan and the Structure of the Unconscious,” in Jacques Ehrmann, ed., *Structuralism* (Garden City, N.Y., 1970), 94–101. Miel stressed the appropriateness of linguistics as a model for “the scientific study of the unconscious” and concluded this part of his discussion with the statement that “Lacan’s revolution in psychoanalysis has many affinities with the thought of Lévi-Strauss”; *ibid.*, 98–99. Psychiatry or psychoanalysis may not be classed as “social sciences,” but, since Bloch was interested in the subject, the lack of “social” with “science” seems irrelevant. For Bloch’s interest, see his address, “Technical Change as a Problem of Collective Psychology,” trans. J. E. Anderson, in Bloch, *Land and Work in Mediaeval Europe*, 124–35.

⁴⁰ Pocock, *Politics, Language, and Time: Essays on Political Thought and History* (New York, 1973), 12.

⁴¹ Pocock, Review of *History and Theory*, in *Comparative Studies in Society and History*, 4 (1962): 535.

⁴² Pocock, “The Origins of Study of the Past: A Comparative Approach,” *Comparative Studies in Society and History*, 4 (1962): 209–46.

*It does not supply us with explanations to be subjected to test: this is a task for the historical imagination.*⁴³

Sewell's analysis of Bloch devolves upon the assumption that a coherent definition of "the comparative method" has been agreed upon. Unlike others,⁴⁴ however, Sewell demeaned the "method" as being useful at only the lowest level of the explanatory process. Yet his overall analysis of Bloch as a "hypothesis-tester," a label that would fit all scholars, does not lead us closer to what comparative historians can claim as a method. Sewell relied upon Bloch, and Bloch relied upon Meillet, whose major goal was *reconstruction*; but, since Bloch was not a consistent follower of Type II methodology, Sewell's statement that the comparative method helps us only at "the easiest and most mundane step" is readily understandable. The comparative method used by Type II linguists is indispensable at the lowest and highest (or smallest and largest) levels of comparison—none of them necessarily "easy." The counterpart of reconstruction for the working historian is the origin of a given institution, and "origin," like the word "cause," has become unpopular for various reasons. Bloch's own warning against the "idol of origins" is one of the best known.⁴⁵

What attracted historians to linguists was the apparent success of the latter in inventing formulae that would account for human behavior and in predicating sources that were sometimes later confirmed by the discovery of texts. For example, in his doctoral dissertation Saussure studied the vowel patterns of Proto-Indo-European, from which he postulated the existence of hitherto unexplained entities, which he called the *coefficients sonantiques*.⁴⁶ These phonemes, which were barely perceptible in the extant remains of Indo-European, were later confirmed by the discovery of Hittite texts and their subsequent decipherment in 1915. Saussure "had discovered, by a purely formal analysis, what are now known as the laryngeals of Indo-European."⁴⁷ A review of the history of various theories surrounding the laryngeals should convince historians that they should not assume that their own discipline is more complex than linguistics, either in theory or in practice. Linguists, moreover, may claim that their theoretical statements have tended to be more congruent with their handling of the data than have those of comparative historians.

Finally, an intriguing feature of Sewell's article is its distinction between the "comparative perspective" and the "comparative method." The former calls for "viewing historical problems in a context broader than their particular social,

⁴³ Sewell, "Marc Bloch and the Logic of Comparative History," *History and Theory*, 6 (1967): 217 (author's italics).

⁴⁴ Ambrose Raftis, for example, concluded that there was a comparative "method" derivable from Bloch's articles; see note 35, above.

⁴⁵ Bloch, *The Historian's Craft*, trans. Peter Putnam (New York, 1953), 29.

⁴⁶ Edgar Polomé, "The Laryngeal Theory So Far: A Critical Bibliographical Survey," in Werner Winter, ed., *Evidence for Laryngeals* (The Hague, 1965), 9–78. Polomé quoted Saussure's study, *Mémoire sur le système primitif des voyelles dans les langues indo-européennes*, cited its date of publication as 1879; Jonathan Culler said it was 1878; but Meillet stated that, although it was actually published in December 1878, the title page shows the date as 1879; Polomé, "The Laryngeal Theory So Far," 10 n. 1; Culler, *Ferdinand de Saussure*, 67; and Meillet, "Notice," in Sebeok, *Portraits of Linguists*, 93–94.

⁴⁷ Culler, *Ferdinand de Saussure*, 68.

geographical, and temporal setting."⁴⁸ Whereas the "comparative method" lends itself to "rules," the "comparative perspective" supposedly leads to "insights." Perhaps so; but, before adopting Sewell's universalism, it would seem essential first to comprehend the misunderstanding of the "comparative method" that emanated from Bloch's early work.⁴⁹

AT THE END OF HIS CAREER, Marc Bloch actually attempted the Type II method. In the *Cambridge Economic History of Europe* he traced the medieval lord back to "rustic chieftains" who inhabited much of Western and Central Europe in pre-Roman times.⁵⁰ Here Bloch dealt directly with the problems of origins, and he seems to have used "all and only" the appropriate evidence. By so doing, he obviously resembled the linguist who searched for the hypothetical ancestor of the latter-day branches of Proto-Indo-European. "Our object being to inquire into the origins of the rural *seigneurie* in Western and Central Europe, our first task must necessarily be to form as clear an idea as possible of what it was like when fully developed. You cannot study embryology if you do not understand the grown animal."⁵¹ It is striking that Bloch's "rustic chieftain" and the historical linguists' Proto-Indo-European are both shadowy reconstructions pieced together from all available remains.⁵² In *The Historian's Craft*, however, Bloch expressed a strong aversion to the "Idol of Origins." Here he argued that too often historians have used the origin of an institution as an explanation and analysis of its growth and development, thus unduly limiting the task of explaining the context, the relationships, and the structures of human institutions.⁵³

In his *Feudal Society*, as in the *Cambridge Economic History of Europe*, there is evidence of Type II comparison, but at the end of *Feudal Society* there is a suggestion

⁴⁸ Sewell, "Marc Bloch and the Logic of Comparative History," 218. Sewell's distinction between a more rule-oriented "method" and a general approach or outlook is echoed in other works dealing with this problem; see, for example, Shmuel N. Eisenstadt, "Social Institutions: Comparative Study," in David L. Sills, ed., *The International Encyclopedia of the Social Sciences*, 14 (New York, 1968): 421-29.

⁴⁹ For a damaging review of universal comparison in the field of anthropology, see E. E. Evans-Pritchard, "The Comparative Method in Social Anthropology," in his *The Position of Women in Primitive Societies and Other Essays in Social Anthropology* (London, 1963), 13-36. He showed how various eminent anthropologists have forced their evidence into a Procrustean bed in order to follow what they assumed was the validity of universal comparison. "The statistical use of the comparative method and on a world-wide scale has been abandoned in this country and, it would appear, is obsolete in Holland. In England, however, the establishment of laws or universals, in the sense of propositions to which there are no exceptions, by comparative analysis continued to be advocated by [Alfred R.] Radcliffe-Brown, one of the most dedicated and influential teachers in our subject, though he did not use statistics, and his version of the comparative method was in practice mainly a return to the illustrative method. Indeed, I have to say, with regret, that [Andrew] Steinmetz would have stigmatized much of what he wrote as idle speculation, and [Herman J.] Nieboer would have regarded it as an example of what he strongly protested against, the capricious practice of some writers of thinking up some plausible explanation of some social phenomenon and then searching round for illustrations which seem to support it and neglecting the rest of the material relating to the topic under consideration." *Ibid.*, 23.

⁵⁰ Bloch, "The Rise of Dependent Cultivation and Seigneurial Institutions," 235-90. For the term "rustic chieftains," see *ibid.*, 284. Also see *ibid.*, 290: "A very ancient structure of rural chiefdoms was the essential nucleus, and about it the centuries deposited their successive layers one by one." And, once again, Bloch referred to Meillet; *ibid.*, 273.

⁵¹ Bloch, "The Rise of Dependent Cultivation and Seigneurial Institutions," 235.

⁵² *Ibid.*, 237-38. Bloch again, however, muddled the waters by alluding to "comparative methods"; *ibid.*, 237 (italics added).

⁵³ Bloch, *The Historian's Craft*, chap. 1, pt. 4: "The Idol of Origins," 29-35.

of Type I—namely, a comparative study of institutions in the universal sense, which he did not endorse in 1928. Here Bloch defined “feudalism” in such a way that both Japan and Europe could be examined without regard to their possible historical links.⁵⁴ Hence, he still seems to be the father of comparative history, even though he did not define the method he recommended in 1928 (Type II) in such a way as to clarify it for the use of practicing historians. Did Bloch finally understand what he was advocating? In *Feudal Society* he wrote that the burden of comparative history was too great for one man but that fortunately useful groundwork had previously been laid. “The task is facilitated by the existence of excellent studies which already bear the hall-mark of the soundest comparative method.”⁵⁵ In 1928 he described only two methods, and in 1939 he iterated his endorsement of Type II; but in 1940, in *Feudal Society*, his reference to the “soundest comparative method” leaves us in limbo, for the use of the superlative would imply that more than two types of comparison were being judged.⁵⁶ In any case, Bloch did not say at this point what “the soundest comparative method” was—whether it was Type I, Type II, or a third, undefined method. Bloch seemed unwilling to commit himself to Type II because of the apparent complexity of social institutions as compared with the supposed clarity of linguistic affiliations.

In making this distinction between linguistic data as relatively simple and historical data as relatively complex, Bloch said, “Now the historian of societies finds that the facts themselves impose upon him this formidable hypothesis of ‘mixtures.’”⁵⁷ Yet, in the *Cambridge Economic History of Europe*, he traced the seignorial system in its various manifestations back to a common origin, the “rustic chieftain.” The rustic chieftain may have been an accurate reconstruction, or perhaps it will turn out to be a “mixture.” In fact, since the late nineteenth century, comparative historical linguists have known that Proto-Indo-European was composed of dialects. In both cases, however, we do not have to resort to the theory of a “mixture”; rather, we may conclude that the search for common origins has not been entirely successful and that more work needs to be done.⁵⁸

⁵⁴ Bloch, *Feudal Society*, 446–47 (*La Société féodale*, vol. 2: *Les Classes et le gouvernement des hommes* [Paris, 1949], 250–51).

⁵⁵ Bloch, *Feudal Society*, 446 (*La Société féodale*, 250: “La tâche sera facilitée par d’excellentes études, marquées déjà au coin de la plus saine méthode comparative”).

⁵⁶ For his endorsement of Type II comparisons in 1939, see Bloch, “Problèmes d’histoire comparée,” *Annales d’histoire sociale*, 1 (1939): 439–40: “La vérité profonde est, d’ailleurs, qu’un tour d’horizon d’une pareille ampleur était, vraisemblablement, beaucoup trop ambitieux. J’ai essayé de le montrer naguère: ce n’est pas vers l’exemple du *Rameau d’Or* que, pour assurer son avenir, l’histoire comparée de la structure sociale doit aujourd’hui se tourner. Ses modèles, il lui faut les demander, beaucoup plutôt, aux méthodes, plus sûres, des linguistes. De même que ceux-ci ont commencé par élaborer, solidement, une linguistique indo-européenne ou intersémitique, par exemple, le premier soin qui s’impose à nous est de bâtir—sans négliger, bien entendu, d’élargir, par instants, notre vision—une histoire comparée des sociétés européennes.”

⁵⁷ Bloch, “Towards a Comparative History,” trans. Anderson, 68 (“Pour une histoire comparée,” 42).

⁵⁸ Meillet, *The Comparative Method in Historical Linguistics*, chap. 2: “Common Languages,” 25–35. Meillet stated, “For all the groups now established and studied in a proper manner, the way to make a comparison is to posit an initial ‘common language’ (German *Ursprache*)”; *ibid.*, 25. Here again Meillet insisted upon reconstruction as the *sine qua non* for comparison, but he was quick to point out that we are seldom able to compare

Bloch's use of the word "facts" raises a serious objection. To quote one recent scholar on the subject, "Historical facts are . . . not given. They are rather postulates designed to explain the characteristics of contemporary data relating to the past or future."⁵⁹ The new methods of historians frequently require a different set of "facts" if they are going to be programmed into the computer; new topics of study or the juxtaposition of old subjects in a different pattern is sometimes mandatory if one is to employ an untried method or pose questions that have hitherto been overlooked.

That Bloch's treatment of the comparative method at the end of *Feudal Society* is inconsistent with his earlier pronouncements is comprehensible given his final definition of feudalism. That definition was obviously not meant to be a product of Type II comparative technique so much as it was an attempt to grasp the *structure* of the society, a goal that need not involve a search for origins. Arguments about methodology, both structural and historical, continue to the present time.⁶⁰ Linguists have not yet solved this problem either, although the most persuasive and successful historical linguists were also structuralists—that is, they considered the data from the synchronic and the diachronic perspectives, as did Bloch himself.

SEPARATING EARLY STRUCTURALISM FROM THE WORK that has appeared since Marc Bloch's death in 1944 is necessary at this point. In the preface to Jacques Derrida's famous *Of Grammatology*, Gayatri Spivak wrote, "A structure is a unit composed of a few elements that are invariably found in the same relationship within the 'activity' being described. The unit cannot be broken down into its single elements, for the unity of the structure is defined not so much by the substantive nature of the elements as by their relationship."⁶¹ This definition of structure has an ancient and venerable tradition in the history of philosophy. The point can be demonstrated by placing Aristotle's definition of tragedy side by side with Bloch's definition of feudalism:

our hypothetical reconstructed forms with "known reality." He then elaborated upon the limitations of the comparative method, limitations that reflect the complexity of linguistic analysis, not its simplicity.

⁵⁹ Traian Stoianovich, *French Historical Method: The Annales Paradigm* (Ithaca, N.Y., 1976), 36, 36 n. 25. Also see *ibid.*, 103–04.

⁶⁰ Stoianovich, for example, stated, "On the subject of comparative method, however, there is a modicum of disagreement. One of the early leaders of the *Annales* School, Marc Bloch, held that 'there is no true understanding without a certain range of comparison; provided, of course, that comparison is based upon differing and, at the same time, related realities.' On the other hand, the cultural anthropology of Claude Lévi-Strauss, who has disciples in the *Annales* School, teaches that a comparative method is rarely likely to be useful since the data introduced for purposes of comparison may be temporally or spatially so close that one cannot be sure of dealing with separate phenomena, or the allegedly related realities may be so heterogeneous that they in fact involve a comparison of incomparable things." *French Historical Method: The Annales Paradigm*, 37. And Eisenstadt declared, "Some skepticism has recently been voiced about the extent to which such comparison may be of any value beyond the range of societies that are culturally and geographically very similar"; "Social Institutions: Comparative Study," 425.

⁶¹ Spivak, Preface to Derrida, *Of Grammatology*, lv.

A tragedy, then, is the imitation of an action that is serious and also, as having magnitude, complete in itself; in language with pleasurable accessories, each kind brought in separately in the parts of the work; in a dramatic, not in a narrative form; with incidents arousing pity and fear, wherewith to accomplish its catharsis of such emotions.⁶²

A subject peasantry; widespread use of the service tenement (i.e. the fief) instead of a salary, which was out of the question; the supremacy of a class of specialized warriors; ties of obedience and protection which bind man to man and, within the warrior class, assume the distinctive form called vassalage; fragmentation of authority—leading inevitably to disorder; and, in the midst of all this, the survival of other forms of association, family and State, of which the latter, during the second feudal age, was to acquire renewed strength—such then seem to be the fundamental features of European feudalism. Like all the phenomena revealed by that science of eternal change which is history, the social structure thus characterized certainly bore the peculiar stamp of an age and an environment.⁶³

That there is more agreement among scholars about the term “tragedy” than there is about “feudalism” does not invalidate the suggestion that both of these definitions stress the relationship of the elements composing the “activities.” If tragedy is a well-defined literary genre and feudalism is a vaguer word defined in radically different ways, it is nonetheless true that Bloch’s definition sought to get at the structure of the society that he called “feudal”—that is, to show the relationship of the parts—just as did Aristotle in his description of tragedy.

Definitions of structuralism that apply to the post-World War II generation tend to be synchronic rather than diachronic, by and large. Both Bloch and the postwar structuralists went to the linguists for a methodology that would permit them to explore questions in new and more effective ways. Modern structuralists have kept the objective defined herein—that is, a focus upon relating parts to the whole—but they have refined their definitions. Recently, Michael Lane summarized these refinements:

1. All patterns of human social behaviour are codes, with the characteristics of languages.
2. Man has an innate structuring capacity which determines the limits within which the structure of all types of social phenomena can be formed.
3. Relations can be reduced to binary oppositions . . . in that any universe . . . can be divided into classes of α and $\sim\alpha$.⁶⁴

⁶² Aristotle, *Poetics*, trans. Ingram Bywater, in *The Basic Works of Aristotle*, ed. Richard McKeon (New York, 1941), 1460.

⁶³ Bloch, *Feudal Society*, 446.

⁶⁴ Lane, “Introduction,” in Michael Lane, ed., *Introduction to Structuralism* (New York, 1970), 18. Note that Lane, in point 3, stated that “any universe” can be divided into α and $\sim\alpha$ (or p and not p). This is certainly true, but it does not prove that binarism lies at the heart of the problem. Although popular in many quarters, binarism as a tool has led scholars into simplistic explanations. Some modern logicians have discarded it as a necessary part of any system. As John E. Pfeiffer wrote, “Modern logic has abandoned one of Aristotle’s most basic principles: the law of the excluded middle, meaning that a statement must be either true or false. In the

These dogmas show the durability of basic structuralist principles; but they also show that structuralists have followed the linguists to the extent that now the data themselves are perceived as having the character of language. On this point, Bloch might well demur, and so might many other scholars in any field of endeavor.

Nevertheless, it seems safe to say that social scientists and others have been impressed with the insights of the linguists, and this phenomenon goes back more than a century. In his seminars, Leopold von Ranke used philological analysis to interpret historical documents, and "comparative philology" was the precursor of "comparative linguistics."⁶⁵ Just so, Bloch turned to Meillet. Lévi-Strauss paid homage to Saussure as the linguist who first set forth the principles of modern structuralism and then actually put those principles into operation in anthropology. Linguistics is one of the few humane sciences that have been judged capable of competing with the physical sciences in putting forth hypotheses that have later been confirmed by "facts."

Bloch turned not only to linguistics but also to a variety of other disciplines—sociology, anthropology, archeology, psychology, geography, and medicine—in order to gain a more profound understanding of the problems that engrossed him.⁶⁶ As Bryce Lyon has said, "Marc Bloch lived life as he wrote history: he always chose the difficult and spurned the easy. Like some historians, he could have effortlessly described simple events and dodged issues, but such history disgusted him."⁶⁷ Hence, Bloch's use of linguistics can be seen as a contribution toward an interdisciplinary approach that, though perplexing, was nonetheless worthwhile as a noble and unfinished experiment.

In short, neither Bloch nor his successors ever really attempted Type II comparison, with the exception of his analysis of seignorial forms, which led him to the "rustic chieftain." Type II was dismissed as being unworkable in history because institutions and societies were considered to be unmanageable units or because the evidence seemed to lead back to "mixtures," a phenomenon that even the linguists said would make the comparative method impossible. Neither Bloch nor his followers, however, tried to define what units in historical data might correspond to the phoneme and the morpheme in language, and without these units linguists could never have embarked upon the study of syntax and

new system a statement may have three values: true, false, or indeterminate. A close analogy to this system in the legal field is the Scottish trial law, which allows three verdicts—guilty, not guilty, or 'not proven.' " Pfeiffer, "Symbolic Logic," in Morris Kline, ed., *Mathematics in the Modern World: Readings from Scientific American* (San Francisco, 1968), 211.

⁶⁵ Theodore H. Von Laue, *Leopold Ranke: The Formative Years* (Princeton, 1950), 14, 15: "Philology and grammar were the established tools by which men could lift the film of unfamiliarity from past ages and distant civilizations. . . . The profit of these studies to Ranke was fourfold: a thorough knowledge of ancient literature and languages, an ultimate acquaintance with ancient historiography, particularly Thucydides, a method of historical criticism, and finally a technique for advanced training, the seminar." Ranke, however, never called his class a "seminar"; rather, he used the Latin word *exercitationes* ("exercises" or "practices"); see Edward Gaylord Bourne, *Essays in Historical Criticism* (New York, 1901), 268.

⁶⁶ For an even more open-ended call to interdisciplinary history, using a universal or Type I comparative orientation, see Erich Rothacker, "Die vergleichende Methode in den Geisteswissenschaften," *Zeitschrift für vergleichende Rechtswissenschaft*, 60 (1957): 13–33.

⁶⁷ Lyon, Foreword to Bloch, *French Rural History*, x.

semantics with such an apparent degree of success.⁶⁸ In effect, historians have made comparisons at the syntactic and semantic levels without first establishing the components of these larger structures.⁶⁹

William Sewell thought that Bloch's "rules about the units of comparison" needed elaboration: "First, *the units to be used in an inquiry will vary not only with the aspect of social life being studied and with the 'historical instant,' but also with the particular explanatory hypothesis we are trying to test by our comparison. . . . And, second, the units to be used for comparison need not be geographical units.*"⁷⁰ By contrast, linguists are at a distinct advantage, for their units—the phoneme, the morpheme, the taxeme (a unit of syntax), and the sememe (a unit of semantics)—are applicable to all languages of the world, whether they are genetically related or not, and at any "historical instant."

Until systematic attempts to formulate such units are made, it will be impossible to say whether or not historians can use Type II comparison. There may well be a variety of ways to approach the comparison of societies that do not fall into Type II or even into Type I, and the results may be enlightening and useful to the reader. It would, however, be interesting, after a half century of discussion about the comparative method, if Type II were actually put into practice and either proved or disproved as a valid tool for the working historian.

⁶⁸ Bloomfield defined the phoneme as "a minimum unit of distinctive sound-feature"; *Language*, 79 (Bloomfield's italics). And he defined the morpheme as "a linguistic form which bears no partial phonetic-semantic resemblance to any other form [and] is a simple form"; *ibid.*, 161 (Bloomfield's italics). For a full explanation of these rather cryptic definitions, see *ibid.*, chap. 5: "The Phoneme," 74–92, chap. 6: "Types of Phonemes," 93–108, chap. 13: "Morphology," 207–26, chap. 14: "Morphologic Types," 227–46.

⁶⁹ The most significant advances in nineteenth-century linguistics came first in phonology and then in morphology, areas stressed by Bloomfield, whose book *Language* was first published in 1933. Although syntax and semantics were known in the nineteenth century, advanced study of these two areas became more prevalent in the twentieth century, especially after World War II. Noam Chomsky's *Syntactic Structures*, published in 1957, started a new wave of interest in syntax, as did his other works, including *Aspects of the Theory of Syntax* (Cambridge, Mass., 1965), and "Explanatory Models in Linguistics," in E. Nagel et al., eds., *Logic, Methodology, and Philosophy of Science: Proceedings of the 1960 International Congress* (Stanford, 1962), 528–50. For an analysis of Chomsky's work, see John Lyons, *Noam Chomsky* (New York, 1970). Also see Emmon Bach, *An Introduction to Transformational Grammars* (New York, 1964); and Andreas Koutsoudas, *Writing Transformational Grammars: An Introduction* (New York, 1966). For recent developments in semantics as well as in the more comprehensive and rapidly developing field of semiotics, see John Lyons, *Semantics*, 2 vols. (New York, 1977); Roman Jakobson, *Coup d'oeil sur le développement de la sémiotique* (Bloomington, Ind., 1975); Umberto Eco, *A Theory of Semiotics* (Bloomington, Ind., 1976); Michel Galmiche, *La Sémantique générative* (Paris, 1975); Terence Hawkes, *Structuralism and Semiotics* (Berkeley and Los Angeles, 1977); and, above all, the various works of Thomas A. Sebeok. In addition to editing, contributing to, and writing many books in the field, Sebeok is also the editor-in-chief of *Semiotica: Journal of the International Association for Semiotic Studies* (The Hague, Paris, and New York, 1969–) and the executive director of the Semiotic Society of America, which publishes a bulletin called *Semiotic Scenz*.

⁷⁰ Sewell, "Marc Bloch and the Logic of Comparative History," 213 (Sewell's italics).

Comments:

LINGUISTICS, as Alette O. and Boyd H. Hill point out, has long had a reputation as the most rigorous of the human sciences. This reputation has often attracted anthropologists, historians, and other social scientists to linguistics as a model for their own research—a more appropriate one, perhaps, than the model of Newtonian physics generally preferred by economists and quantitative sociologists. The Hills' essay is an account of one attempt—not in their eyes a very successful one—to model historical investigation on linguistics. Their exposition of Antoine Meillet's historical linguistics is clear and informative, and their demonstration that Marc Bloch's comparative history did not conform to the linguists' rules is entirely convincing. Bloch's analogy between comparative history and historical linguistics was, to put it bluntly, misconceived and misleading.

What conclusion should we draw from this fact? Here I part company with Alette and Boyd Hill. They think that Bloch's mistaken analogy was a fatal flaw, that Bloch's own writings on comparative history "lack precision and coherence" (page 837), and that a strict application of the methods of historical linguistics is the only way to make comparative history more precise. The method they advocate has two essential features. First, the goal of comparative history should be the reconstruction *in a prehistorical past* of the remote common origin of later-divergent but genetically related social structures—a sort of *Urzivilisation* that would correspond to the linguists' *Ursprache*. Second, historians should define "units of historical data" that "correspond to the phoneme and the morpheme in language" (page 845). Without such elementary units ("institemes"? "technologemes"?) the study of more complex social structures—the historical equivalents of syntax and semantics—is doomed to failure.

AS A PROGRAM FOR COMPARATIVE HISTORY, this position is unacceptable. The project of defining units of historical data that correspond to phonemes and morphemes hardly seems promising, especially since Alette and Boyd Hill fail to provide any clues as to what such units might be. And to maintain that comparative history should be confined to the reconstruction of common origins, while more plausible on the surface, is equally unacceptable.¹ In the first place,

¹ One of the strictures the Hills offer as a part of their reconstructive program is quite valuable, although perhaps not for the reasons the Hills think. This is their "all and only" rule—that historians must examine "*all and only* the phenomena in a related group" (page 834). Although I do not think it is possible to delineate "a related group" of the sort they envisage (a group of societies descended from a remote common origin), the Hills are right to remind us that comparisons of just two societies can often be misleading. More often than not, the proper comparative universe contains appreciably more than two cases. Bloch was certainly aware that comparisons should embrace an entire class of phenomena. In 1928, for example, he pointed out at some length that no attempt to explain the rise of representative institutions in the fourteenth and fifteenth cen-

it would mean returning to an obsessive concern with origins, a concern that Bloch criticized with particular effectiveness. But the Hills' strategy goes beyond a general fascination for origins. They want historians to focus their attention on prehistoric origins, to construct by means of controlled speculations a past society about which we have no direct evidence. That this strategy has been fruitful in historical linguistics is undeniable. But to advocate such a program for comparative history is to ignore fundamental differences between languages and societies. The Hills' program not only would make comparative history irrelevant to all of the major issues of current historical research but also would commit us to a strategy that cannot possibly succeed even on its own chosen terrain of reconstruction.

A strict application of linguistic methods to comparative history is impossible because social structures differ in character from linguistic structures; it simply is not true that "all institutions behave as languages do" (page 831). The essential difference lies not in their complexity² but in the arbitrary character of the linguistic sign. That is to say, there is no intrinsic connection between the sign (for example, the word "chicken") and the thing signified (a particular type of bird); the same thing could just as well be signified by another sign ("sparrow" or "floor" or "instead" or "Hauptpunkt" or any other combination of sounds that the human ear can distinguish). It is this arbitrariness that makes linguistic structures so stable. How could the Indo-European language have survived the epochal change from nomadism in Central Asia to settled agriculture and the rise of cities in Central or Southern Europe without undergoing changes more fundamental than the loss of an intervocalic /s/ or the "First Sound Shift"? How can inhabitants of societies as different in their fundamental constitution as Hindu India, Ancient Greece, and present-day California or Scandinavia express their thoughts in languages with the same essential structures? Precisely because the system of sounds and the rules for combining them into comprehensible statements which we call the Indo-European language bear no intrinsic relationship to the world inhabited by either the Indo-Europeans or their scattered descendants.

When we turn to social structures, this crucial feature of arbitrariness is absent. While we can analytically separate the rules of social structure from the activities to which they apply, the relationship between rules and activities is any-

turies can hope to succeed without taking into account *all* of the cases: the French *États*, the German *Stände*, the Spanish *Cortes*, the Italian *Parliamenti*, and the English Parliament; Bloch, "A Contribution Towards a Comparative History of European Societies," trans. J. E. Anderson, in Bloch, *Land and Work in Medieval Europe: Selected Essays of Marc Bloch* (New York, 1969), 55-56.

² The Hills go to great lengths to refute what they see as Bloch's insinuation that linguistic problems are less complex than historical problems. But Bloch claimed nothing of the kind. He did say, when speaking of the linguists' success in reconstructing the Indo-European language, that "the history of social organisation is in this respect a much more difficult problem. The fact is that a language presents a much more unified and easily definable framework than any system of institutions: hence the relative simplicity of the problem of linguistic affiliations." Bloch, "Towards a Comparative History," 68. In this passage Bloch is not claiming that linguistic structures are simpler than institutional structures in some global sense, only that they are "more unified and definable" and therefore that *affiliations* are easier to trace. Anyone charged with the task of drawing up a historian's equivalent of "Grassmann's Law" would surely agree.

thing but arbitrary. Take, for example, the rules of the *seigneurie*. These could only apply to a system of settled agriculture; in a nomadic or hunting and gathering society, they would be nonsense. As Bloch and others have demonstrated, moreover, the rules of *seigneuries* are dependent upon (and therefore change with) technologies, man-land ratios, the availability of money, religious views about slavery, and so on. In all of these and many other ways, the rules of *seigneuries* have definite and nonarbitrary relations to a world outside themselves. At the same time, the social existence to which seignorial regulations apply is constituted by those regulations in a way that a chicken is not constituted by the syllables that form the word or by the linguistic rules that enable us to define a chicken as "a common domestic fowl." The structure of the rules of the *seigneurie* and the social activities to which they apply are bound by a whole complex of mutual determinations.

Epochal changes in social life leave linguistic structures virtually unchanged, while such changes as migration from Central Asia to Greece or the Gangetic plain or the change from nomadism to settled agriculture obliterate ancient institutional structures or transform them beyond recognition. To postulate that such changes could be described by historical equivalents of Grimm's Law or Grassmann's Corollary is to ignore the difference between language and society—a difference so fundamental as to make the linguists' method of reconstruction utterly illusory as a program for comparative history. Thus, while it is fair to criticize Bloch for loose thinking when he likened his style of comparative history to historical linguistics, it is misguided to criticize him for not taking his own loose and inaccurate analogy more literally.

Alette and Boyd Hill, of course, claim that Bloch actually carried out their program of reconstruction in his essay on the rise of seignorial institutions, tracing the origins of the *seigneur* back to the primitive "rustic chieftain." But, on closer inspection, this claim turns out to be false. Bloch did not treat "rustic chieftainship" as a *common* origin of *seigneuries* in the sense that Indo-European was the common origin of Sanskrit, Greek, Latin, and the Germanic and Slavic tongues—that is, a single unified structure that slowly evolved into a divergent family of structures with precisely identifiable relations to the *Ur*-structure. By introducing "rustic chieftains," Bloch was merely pointing out that some form of local potentates existed in European villages long before the emergence of the *seigneurie* proper. Far from regarding them as a single common origin of the *seigneurie*, Bloch remarked that "the more we knew about [these village chiefdoms], the more varied we should probably find them."³ The truth is that Bloch himself considered the linguists' strategy of reconstruction to be improbable as a guide for historical research. "Comparative history," he remarked ironically, "may reveal hitherto unnoticed interconnections between human societies, but it would be excessive optimism to expect that it would lead to the discovery of

³ Bloch, "The Rise of Dependent Cultivation and Seignorial Institutions," in J. H. Clapham and Eileen Power, eds., *The Cambridge Economic History of Europe*, vol. 1: *The Agrarian Life of the Middle Ages* (2d ed., Cambridge, 1966), 286.

fragments of an ancient and hitherto unknown parent society."⁴ While Bloch was sometimes imprecise in his theoretical statements, he also knew when to step back from misleading abstractions or forced analogies.

THE OBVERSE OF ALETTE AND BOYD HILL'S CONTENTION that comparative history must follow the rules of historical linguistics in order to be coherent is their contention that Bloch's comparative history, because it failed to follow these rules, was incoherent. Once again, I disagree. The point of my own article, which the Hills cite, is that, for all of Bloch's lack of rigor as a theorist, he elaborated *in practice* a comparative method that was consistent, coherent, and appropriate to the subject-matter of history.⁵ All of Bloch's comparative history, as I argued there, was based on a unified logic—a modified experimental logic of hypothesis testing. Because of Bloch's underlying consistency, his practice of comparison can be used to elaborate a set of methodological rules that differ essentially from the rules of historical linguistics and that Bloch's own theoretical writings express only partially and imperfectly. The rules implicit in Bloch's historical practice constitute a valid methodology for comparative history, one that, in my opinion, is still insufficiently understood. My article, by working out and stating these implicit rules, was an attempt both to demonstrate the coherence of a great master's historiographic practice and to make his method more accessible to the community of historians.

It is true that Bloch's analogy between comparative history and comparative linguistics is misconceived. But, by fixing on surface inconsistencies in Bloch's argument and by failing to examine seriously his practice, Alette and Boyd Hill have failed to discover the deep consistencies that give Bloch's work its continuing power and interest.

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⁴ Bloch, "Toward a Comparative History of European Societies," trans. Jelle C. Riemersma, in Frederic C. Lane and Jelle C. Riemersma, eds., *Enterprise and Secular Change: Readings in Economic History* (Homewood, Ill., 1953), 516. The Anderson translation of this passage in *Land and Work in Medieval Europe* is hopelessly garbled.

⁵ William H. Sewell, Jr., "Marc Bloch and Comparative History," *History and Theory*, 6 (1967): 208–18.



TWENTIETH-CENTURY ACTIVITY in comparative study of the past is a potentially important theme in intellectual history. There has been able writing on it from anthropologists, but much more is needed from the various points of view of other specialists. Right now there is room for what Marc Bloch called premature syntheses, designed to map out the main problems of a subject without

burdening the reader with microscopic detail. In these days of fast food service there is demand for quickie versions of that genre in the form of journal articles. Some of the supply is from people who are kept too busy teaching to give their ideas a workout in new research but who want a chance to get them into circulation. In "Marc Bloch and Comparative History," the *American Historical Review* got both less and more than its editor bargained for. It is not a "think piece"; it is a piece of genuine myth-making—unpolished but, unconsciously, in the grandest tradition. Marc Bloch, comparative methods, and the wisdom of great linguists—all are symbols. As scholarship, the essay is careless and superficial; as myth, these faults are essential, uncorrectable, and fascinating.

Consider what Alette O. and Boyd H. Hill call the "logic" of their argument. It is a sequence of five positions, as follows: (1) Bloch, seeking wisdom, derives his conception of comparative methods from the greatest French linguist of his time; (2) through some flaw in his mind or character he fumbles, he misapplies them; (3) he becomes the "father" of a flawed bag of tricks labeled "comparative history"; (4) this flawed thing is after his death perpetuated and spread by "disciples"; (5) wisdom grows among the linguists, and anthropologists pick up some of it; but historians dither, or they sit blindly in the shadow of the authors' necessarily imperfect mythic hero.

Now, anyone familiar with Bloch's writings and with the intellectual temper of the historians' world that he graced can tread all of this supposed reasoning into the ground. Indeed, it is enough to knock out the first position, by evidence long in print. The rest of the structure then collapses of its own accord, but each part of it, if one has patience, can be reduced to dust by the weight of indisputable evidence. I spent a lot of time trying to draft a comment advising the Hills to look at one or another of their scraps of alleged evidence with more sensitivity to context and so forth. This became a dreary and depressing chore. Only when a metaphor came to my mind, suggesting that their picture of Bloch has some affinities with the figure of Moses muddleheadedly losing his way in the desert and failing in his divinely ordained mission, did I begin to see that perhaps I was missing the whole point of the paper. Impatient with its dodging of all of the larger issues that surround the development of comparative methods and with its cavalier handling of the meager sources cited, I had failed to realize that what is important to me is, to the Hills, irrelevant. From the mythic standpoint the five positions are indissolubly linked by the ahistorical errors they embody; these are like articles of faith.

Their first position, for example, assumes not only that Bloch received his conception of comparative methods (including his distinction between procedures applicable to comparison of analogous phenomena in societies that were unrelated and those appropriate to comparison of similar phenomena in the closely related societies of the western half of Europe) from the book that Antoine Meillet published on comparative method in linguistics in 1925 but also that Bloch subsequently fell so deeply under the spell of linguistics as to be planning, by 1928, to switch his research from France to Eastern and Central European language communities. Here the Hills suppose that he would somehow be

transposing Meillet's "techniques" from the study of language families to the study of societies. A reader unfamiliar with Bloch's work would not be able to spot its distortion in this account, but the progress of his thinking on comparison is readily traceable in the three monographs that Bloch had published in 1913, 1920, and 1924. In the last of these, his book on the history of popular belief in royal healing powers in Western Europe, the need to refute an engaging hypothesis of Sir James Frazer (entailing comparison between Polynesian chieftains as described by missionaries and early medieval English kings) led him to make a lucid methodological statement in terms of concepts of explanation that would be satisfactory to historians.¹ The Hills had not surprisingly overlooked both this and Bloch's explicit statement in his address at Oslo to which they refer, after praising Meillet's stand, that he would be speaking from the historian's point of view. Their idea about his supposed intention to switch his research to Eastern and Central Europe under Meillet's guidance also involves them, as anyone who checks the French text of the Oslo address may verify, in a double-headed monster howler in misreading a simple sentence and by following a careless translator into taking *occidentale* to mean "eastern."² Perhaps in the mythic world the sun sets in the east. More pertinent to nonmythic interests is what the misreading of that passage reveals: the Hills have entirely overlooked the four following sections of the Oslo address, in which Bloch illustrates the uses of comparison in European history.

The second position is set up by giving the two types of comparison that Bloch outlined at Oslo labels and criteria appropriate not to his own exposition of historical comparison but to Meillet's discussion of linguistic comparison. The Hills then berate Bloch for not following their own interpretation of what the latter demanded. They fall into numerous confusions that cannot be untangled in my allotted space. The most misleading, to nonmythic minds not knowing the text or the 1924 statement, is the idea that he "rejected" the broader, more general type of comparison. European medievalists did not in any case practice it, but he discerned four advantages to it, two of which were similar to those of more careful, closer comparisons. Procedures differed, but the results were not entirely different in kind. On the question of his supposed confusion regarding the problem of "origins," the Hills have a real romp, conflating three quite different kinds of inquiry.

The mythic Bloch, to fulfill his symbolic role, had to "father" something, and the child had to be unsatisfactory.³ Personally, I think Bloch was mistaken in hanging on to the term "comparative history," which is just about as silly as the term "comparative religion." Such terms had become common while he was a boy, and they inevitably became more and more ambiguous with time. It is inaccurate even to call him an adoptive father. He ignored the historical com-

¹ Bloch, *Les Rois thaumaturges: Étude sur le caractère surnaturel attribué à la puissance royale particulièrement en France et en Angleterre* (reprint ed., Paris, 1961), 51-59.

² See the Hills' seventh paragraph, page 831, above.

³ To the nonmythic reader, I commend Philippe Wolff, *Western Languages, 500-1500 A.D.* (New York, 1973), which demonstrates the continuance and deepening of what Marc Bloch regarded as "a happy alliance" between linguists and historians.

parisons of German sociologists, and it is impossible to judge the extent of his influence. The two great weaknesses of the third and fourth positions, to the nonmythic mind, are that many people, as I recollect clearly from my own contacts over the years, shared some of his views on method without even knowing his work and that comparative methods came to be more and more diversified, in all disciplines, with changes in the character of the kinds of enquiry they served.

This last observation may help explain why the Hills necessarily, with the aid of their usual misreading of sentences, have to berate me a bit. With all of its elastic imagination, the mythic mind on some points prefers inexorable rigidity. The chief purpose of the international quarterly for which I was initially and for over fifteen years responsible was to keep discussion going about how comparative methods, whenever and wherever they got into a rut, could be shaken into more fruitful lines. The Hills' contentment with the two-type classification as they interpret it is from that point of view oddly naive. I do, however, plead slightly guilty regarding what could be a slightly rational cause of their finding only one article, apparently, in the several hundred for which I was responsible, editorially speaking, that they liked. If I had obtained more from linguists they might have liked me better. The reason I did not was because those years saw very intensive internal debate between different branches of their discipline. The quarterly served those who needed it most.

I am writing more elsewhere on this whole subject and, meanwhile, wish Alette O. and Boyd H. Hill luck.

SYLVIA L. THRUPP
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Reply:

WE ARE GRATEFUL FOR THE COMMENTS on "Marc Bloch and Comparative History" because they confirm our suspicion that comparative history as a field is in profound need of definition. For the careful reader it should be apparent that we do not insist upon a "program" for all historians to follow. Indeed, we make it quite clear that Type II comparison is only one of many possible approaches to the study of societies. We merely point out that Bloch never constructed a Type II model for historians—and neither has anyone else. Furthermore, we do not agree with William H. Sewell, Jr., in claiming that Bloch's analogy was "misconceived and misleading" (page 847). "Misleading," yes, but "misconceived," no. Bloch's suggestion about the possibility of using the methods of the comparative linguists was most exciting. That he did not carry through on it in no way negates its boldness and is no reason to discourage historians from attempting to do so now. If they do not wish to, that is their choice, but anthropologists have investigated the techniques of linguists and found them to be useful.

One prominent linguist has summarized the way in which anthropologists have employed linguistic methods and models:

Linguistic methods and models can neither be rejected out of hand, [n]or be made to apply by sleight of hand. As to the latter, attempts at liberal transplantation have usually failed, through being based on surface analogy alone. Where fundamental principles have been understood, there has been some success. The ethnographic work of Goodenough, Conklin, C. O. Frake, and others, for example, has seen its task as one of understanding the nature of culture and of cultural description from the same vantage point as that from which modern linguistics understands the nature of language and linguistic description. Rather than seek "vowel triangles," "phonemes," or "deep structures" of culture, it has sought to understand and apply the methodological principles which lead to the discovery of phonemic and other structures—principles of contrastive relevance, distributional class and paradigmatic set, distinctive feature, transformational relation—and to discover their sphere of applicability in other cultural phenomena.¹

Dell Hymes's statement shows that, rather than transferring wholesale a set of terms from one field to another, the nonlinguist must apply the principles that underly linguistic structures in order to invent a set of terms that will be useful to other disciplines, in this case history.

THE PRINCIPAL SOURCE of Professor Sewell's criticism lies in a commonly held assumption that history, unique in its subject-matter, is in no way analogous to

¹ Hymes, "Anthropology and Sociology: An Overview," in Thomas A. Sebeok, ed., *Current Trends in Linguistics*, vol. 12: *Linguistics and Adjacent Arts and Sciences* (The Hague, 1974), 1458 (italics added).

language. Many other historians, making similar assumptions, appear to dismiss Bloch's statements on methods when it suits them while drawing upon his inconsistencies as a kind of *carte blanche* to pursue any sort of comparison under the sun. We take Bloch at his word, both in his statements on method and in his history, rather than deriving a "method" that others claim can only be mystically gleaned from his history. If there is a "method" in Bloch's writings, why has it not been set forth coherently by his interpreters? Sewell's characterization of him as a "hypothesis tester" is scarcely precise enough to be helpful. Why has he dogmatically asserted, once again, that historians cannot make use of linguistics when other social scientists have moved into the area and applied its principles to their own advantage? We see no reason to warn scholars against trying Type II comparisons as if such an experiment posed a threat to all other comparative historians.

Professor Sewell further ridicules our article for its "obsession" with "origins" and, thereby, dismisses us as prehistorians (page 848). His remarks are, to say the least, seventy-five years out of date. We agree with the position taken in 1904 by Carl Darling Buck, an Indo-European linguist of the first rank:

I have said that the comparison of related forms was not an end in itself, but a means of reconstructing the parent form. But I do not wish to imply that these parent forms are of great intrinsic interest or that the reconstruction of the parent speech is the ultimate aim. No one is ambitious to speak this hypothetical language, nor does it . . . furnish the key to the problems of primitive linguistic development. Indeed, this language which we arrive at by reconstruction is itself a highly developed form of speech, which has behind it thousands of years of history which is forever inaccessible to us.

Its value lies rather in the light which is thereby reflected on the history of each individual language belonging to the group. Each language contributes its share of evidence for the reconstruction of the parent speech, and each in turn is illuminated by it. The real object throughout is to trace the development of a linguistic phenomenon from its earliest attainable stage to its latest expression. Comparative grammar is simply a history of a group of related languages, and when that is said, its relation to the history of an individual language of the group is obvious. They are not different sciences, one merely auxiliary to the other, but represent a wider and a narrower range of the same subject. Whatever differentiation exists is consequent only upon a division of labor. The historian of the Greek language, for example, is, from the purely linguistic standpoint, a specialist within the Indo-European field. And if the wider outlook of comparative grammar is essential to the intelligent study of the history of the individual language, it is no less true that comparative grammar depends for its very existence upon the investigation of the special facts and conditions of each language.²

So much for our "fascination" with an "Urzivilisation."

As to Professor Sewell's criticism of the "arbitrariness of the sign" (page 848), a feature of Antoine Meillet's method that Bloch did not include in his 1928 article, our point was to show that this omission revealed Bloch's unwillingness or inability to comprehend the full message of the comparative linguists. We do not have space here to explore this area of theoretical concern, but we urge the

² Buck, "The Relations of Comparative Grammar to Other Branches of Learning," in Howard J. Rogers, ed., *Congress of Arts and Science, Universal Exposition, St. Louis, 1904*, vol. 3: *History of Language, History of Literature, History of Art* (Boston, 1906), 35-36.

reader not to take Sewell's discussion of the "arbitrariness of the sign" as the last word on the subject. This concept is under study not only by linguists but also (and of more relevance to historians, perhaps) by semioticians.³ A sign need not be a word: a sign can be anything that signifies something else. "Not only are words signs, but also gestures, images, nonlinguistic sounds, like the chimes of Big Ben. Obviously devices (such as flags) created by man in order to indicate something are signs, but so are, in ordinary language, the thread of smoke that reveals a fire, the footstep in the sand that tells Robinson Crusoe a man has passed along the beach, the clue that permits Sherlock Holmes to find a murderer."⁴ Thus, regalia, media of exchange, architecture, kinship relations, dress codes, and rituals can be highly arbitrary and also obviously useful to historians. Very few historians have made use of semiotics any more than they have made use of the Type II methods of the linguists, but we believe that such experiments can only enrich the historical profession.

It is flattering to find that Professor Sewell viewed our article as such a menace that he needed to reassure all comparative historians that our so-called program cannot possibly work and should be avoided at all costs. What is disconcerting is that he so often takes our statements out of context and generally twists our language to his own purpose. For example, we emphatically did not state that "comparative history should be confined to the reconstruction of common origins" (page 847). Nor did we say that "Bloch's comparative history, because it failed to follow these rules, was incoherent" (page 850). Our position from the outset has been that Bloch did not follow his own stated plan of 1928, but we nowhere said that Bloch's practice of history was thereby ruined. We believe that Bloch's historical works need no defense, nor do we need to defend our remarks about the confusion engendered by his statements on methods.

As for Sylvia L. Thrupp's bizarre comment, originally entitled "Myth-Making in the Mountains,"⁵ only one point deserves a reply: she is quite correct that "occidentale" means "western." Accordingly, we should have constructed our hypothetical analogy around Spain and France, using the Basque-speaking area as the non-Indo-European equivalent to the Hungarians as against the Germans and Slavs. The result of our inquiry would, nevertheless, have been the same.

³ See Roman Jakobson, *Coup d'oeil sur le développement de la sémiotique* (Bloomington, Ind., 1975); and Umberto Eco, *A Theory of Semiotics* (Bloomington, Ind., 1976). Saussure's "l'arbitraire du signe" was modified by Émile Benveniste and was then redefined by Alfons Nehring; see Benveniste, "Nature du signe linguistique," *Acta Linguistica: Revue internationale de linguistique structurale*, 1 (1939): 23-29; and Nehring, "The Problem of the Linguistic Sign," *Acta Linguistica*, 6 (1950): 1-16. Sewell's discussion of the word "chicken" does not begin to come to grips with the subtleties of the problem, which has not yet been satisfactorily resolved in all of its complexity. Semioticians have enriched the debate about "l'arbitraire du signe" by introducing the tripartite scheme of Charles Sanders Peirce (icon, index, symbol) into the study of sign systems. For a brief description of the work of Saussure and Peirce, see Eco, *A Theory of Semiotics*, 14-16. It is immediately apparent by comparing Eco's analysis with that of Sewell that Sewell has reduced "arbitrariness of the sign" to a simple relation between "chicken" and "common domestic fowl" whereas Eco has emphasized the "image" or concept that a word or other sign evokes in the mind of the interpreter. This observation alone should be proof to those conversant with the literature on "l'arbitraire du signe" that Sewell's discussion is simplistic at best.

⁴ Umberto Eco, "Looking for a Logic of Culture," in Thomas A. Sebeok, ed., *The Tell-Tale Sign: A Survey of Semiotics* (Lisse, 1975), 11.

⁵ *Editor's Note*: The format of the *AHR Forum* does not include titles either to the "Comments" or to the "Reply."

The rest of Thrupp's comment caricatures the issues in our article. We are, apparently, hillbillies who have no business questioning her editorial preface to *Comparative Studies in Society and History* or attempting an analysis of Marc Bloch, our "Moses in the desert." If we are the McDonald's of the Rockies, it should have been easy for Professor Thrupp to outclass us with haute cuisine from La Lutèce, but her comment gives us nothing to feast upon. It is her response, not our article, that amounts to "myth-making" and metaphor. We did not deal in symbol but in textual data, yet she loftily dismisses the substance of what we revealed about Bloch and the tradition of comparative history since 1928.

IT IS APPARENT IN THE COMMENTS of Professors Sewell and Thrupp that they are mainly concerned with preserving the reputation of their own past work, which they perceive as implicitly undermined by our article. Interestingly enough, they now turn against Bloch himself: Sewell says that Bloch's analogy between comparative history and comparative linguistics was "misconceived and misleading," something he did not say in "Marc Bloch and the Logic of Comparative History," which begins with an encomium to Bloch's 1928 article; and Thrupp chides Bloch for failing to incorporate the works of German sociologists in his writings and for perpetuating the term "comparative history," which she considers "silly" (page 852). Bloch seems overnight to have become a scapegoat, since he can no longer be revered as St. Mark. We do not think the idol has feet of clay, but then we never idolized him in the first place. The reaction of Sewell and Thrupp is symptomatic of a lingering malaise in the methods of comparative history, a field we do not consider "silly" but merely in need of the healing touch of some dedicated scholars who are more interested in the real Bloch than in the defense of their own earlier vision of him.

According to Professor Sewell, "The Hills' program not only would make comparative history irrelevant to all of the major issues of current historical research but also would commit us to a strategy that cannot possibly succeed even on its own chosen terrain of reconstruction" (page 848). We do not wish to throw out all forms of comparative history that do not conform to Type II methods. On the contrary, we would like to see Type II attempted and let all scholars judge for themselves whether or not the results are worthwhile. Certainly, this exercise will not destroy other types of comparative history, nor will such an experiment necessarily be "irrelevant to all of the major issues of current historical research."

ALETTE OLIN HILL
BOYD H. HILL, JR.
University of Colorado

Reviews of Books

GENERAL

FRANK E. MANUEL and FRITZIE P. MANUEL. *Utopian Thought in the Western World*. Cambridge: Belknap Press of Harvard University Press. 1979. Pp. vi, 896. \$25.00.

For more than a quarter century Frank E. Manuel and Fritzie P. Manuel, husband and wife, have been colleagues in a great enterprise—the study of utopian thought in the Western world. This tremendous book is the sum of their prodigious labors.

They begin with the Judeo-Christian myth of paradise, pass rapidly over ideal visions of society held in the classical and the medieval eras, and then begin a more leisurely march with a long consideration of Thomas More, whose *Utopia* of 1516 coined the word for the genre. They come at last to the present with its frenetic quests for utopia that include not only the many varieties of Marxism but also the cult of the Reverend James Jones that ended in mass suicide on November 18, 1978, in Guyana.

What generalizations can we make from their crowded account of the multitudinous visions? Not many. Utopias are efforts to create worlds where the woes of the authors' times are borne away. (In modern "dystopias," which the Manuels touch on, the woes triumph!) But beyond a love of education, simplicity, and community, few utopians have agreed with each other about the shape of their dreams.

Utopias seem to fall into three classes with a lot of blurring at the edges. Some, like Thomas More's, were never intended to be realized. They are mythic inventions meant to jolt contemporaries into thinking that *something* should be done. Bacon's *New Atlantis* was this sort, and so are many modern tales of science fiction. The writers supposed that some things in their ideal societies might be imitated; More certainly believed in 1516 that England could treat thieves more kindly. But they did not expect their whole vision to come to life on earth. More

said that most things in his *Utopia* were to be wished for rather than awaited.

More common have been the utopias of zealots and planners, men as diverse as Thomas Münzer and Jean Meslier, Saint Juste and Robert Owen, and, though the Manuels do not mention him, Brigham Young. These men tried to wrench the ideal to earth in the here and now and believed that its power—divine or rational and perhaps violent—would flatten the old and raise a new world.

Still others have believed that society was moving grandly on its way to an end as irresistible as it was inevitable. Here are Condorcet and Karl Marx, the social Darwinists, and the Freudian-Marxists like Herbert Marcuse of our own times.

Their legions march through this gargantuan book, and they illustrate a great paradox of our civilization. We have never quite been satisfied with what we are, and the myth of paradise haunts us and woos us to change. But as often as not, the striving for paradise has created hell on earth. The honest yearnings of one age are seen, when brought to pass in another, to be disasters for the human spirit. Thomas More's *Utopia* with its rigid, communal conformity might seem like heaven to the chaotic sixteenth century. But to those who have seen the new orders of our century with their soul-destroying monotony, More's vision is less enthralling. He made no room for artists. The Manuels offer proof enough that, in utopias, planners are kings and artists, if they exist at all, are servants or worse.

In this book the Manuels have themselves built a city of the mind. Their scope is vast, their lines are sure, their vistas serene. Any specialist may see a crack here and there. But no one in our lifetime will better perform their task.

RICHARD MARIUS
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ULRICH MICHAEL KREMER. *Die Reformation als Problem der amerikanischen Historiographie*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz,

number 92.) Wiesbaden: Franz Steiner Verlag. 1978. Pp. viii, 265. DM 64.

The author of this Berlin dissertation describes his intention as an effort "to trace [in nineteenth- and twentieth-century American Reformation historians] the removal of utopian social goals from the spirit of the Reformation and to that extent the Europeanization of American Reformation historiography." Ulrich Michael Kremer treats historians who have in his opinion played a role in this latter process, classifying them as following: the founders (J. Priestley, P. Schaff, A. C. McGiffert, W. Walker); social historians (P. Smith, E. Erikson, H. Grimm, E. Schwiebert, J. Schapiro, R. Cole); Catholic historians (P. O'Hare, C. Hayes, J. P. Dolan, G. H. Tavard, H. McSorley, J. McCue); Reformed historians (G. P. Fisher, W. A. Mueller, J. T. McNeill, R. Kingdon, R. Bainton, G. H. Williams); and Lutheran historians (C. P. Krauth, R. H. Fife, G. W. Forell, L. W. Spitz, J. Pelikan, W. Pauck).

Despite the book's title, this study is less a treatment of Reformation historiography than an essay on American religious consciousness as reflected in the above scholars. The author presents Americans as having been dominated for centuries by "this-worldly optimism about progress" and concern for "universal freedom and tolerance"—a secularization in his view of the original "utopian" vision of the Puritans. He finds these typically American values constantly projected back onto the Reformation and believes that it was only under the sobering crisis of the Depression that American utopianism buckled and the theological ideas of neo-orthodoxy became attractive to the American mind. This also made possible a more profound understanding of the Reformation. According to the author, the essential agents in this latter process were American Lutherans and Catholics, who had long drunk from the sobering wells of European theological scholarship. American Lutherans are especially praised by the author for their role in helping the nation overcome false optimism, anti-intellectualism, and lack of historical consciousness and for making ecumenism possible in sectarian America.

Kremer has presented a vision of American Reformation scholarship that is very flattering to the theologians and church historians of the old country. He has, however, also limited the usefulness of his book by dealing almost exclusively with theologians and church historians and by ignoring so much American Reformation historiography of the last twenty years. Had he pursued his subject on a broader basis and into the 1970s, he would have found American Reformation scholarship far more varied in method, international in bibliography, ambitious in scope, and lucid in style and presenta-

tion than its German counterpart. He would also have discovered that the "Europeanization" of American Reformation scholarship has been a very mixed blessing.

STEVEN OZMENT
Harvard University

VACLAV MUDROCH. *The Wyclif Tradition*. Edited by ALBERT COMPTON REEVES. Athens, Ohio: Ohio University Press. 1979. Pp. xvii, 91. \$8.00.

The subject of this short study is the attention writers of the sixteenth, seventeenth, and eighteenth centuries gave to John Wyclif. The author expects his survey of this literature will bring to "light views hitherto unknown to historians" (p. xiv)—historians in general, yes, but Wyclif students, no. Vaclav Mudroch did not extend his study to the nineteenth and twentieth centuries because most of the later biographies of Wyclif were done by Protestant clergymen who had little new to offer, while the more scholarly writing was by "historian-scientists" who wrote for members of their own profession.

Among the writers of the sixteenth century whom Mudroch introduces are the Dominican Bernard of Luxembourg, John Bale who hailed Wyclif the "morning star," and John Foxe whom the author refers to as "the only trustworthy guide to Wyclif's and other martyrs' lives for the English Protestant for many years to come" (p. 10).

There was more about Wyclif from seventeenth-century writers and it was usually offered with greater detachment. Among these writers were Nicolas Vignier, Robert Bellarmine, and Thomas James. James sought to tailor his treatment of Wyclif to make him acceptable to Anglicans who could not stomach what the Oxford schoolman had written about dominion and disendowment and who, along with other Protestants, puzzled over his ambivalence on the nature of the Eucharist. A vigorous debate between the Frenchman Antoine Varillas and Gilbert Burnet, bishop of Salisbury, who denounced his adversary's latest work on Wyclif as the "falsest coyn that can be struck" (p. 41), attested to the continued interest in the English reformer.

The eighteenth century, the "Age of Reason," treated Wyclif with still greater objectivity but had much less to say. Religious toleration had brought in its wake growing indifference to the doctrinal controversies that had fired earlier generations. Apart from his Protestant apologists, the few writers who did notice Wyclif were ironically the *philosophes*, who were apt to praise anyone who had run afoul of the medieval church. The one Protestant writer deserving of particular mention was John Lewis, who produced the best biography of Wyclif yet to appear.

The author, an émigré Czech whose life ended before he saw his study in print, came to his interest in Wyclif via the reformer's influence on Jan Hus and his Bohemian contemporaries. Mudroch must have been an inspiring teacher. Albert Compton Reeves, one of his devoted students, edited this study. The volume offers unusually extensive notes, a bibliography, and an index.

JOSEPH H. DAHMUS
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PATRICIA JAMES. *Population Malthus: His Life and Times*. Boston: Routledge and Kegan Paul. 1979. Pp. xv, 524. \$43.50.

After nearly two decades of painstaking effort, Patricia James has provided us with her biography of T. R. Malthus, the first full account of the life, times, and works of the great economist. James is to be commended not only for having undertaken the task of searching out the scattered materials upon which she bases her study but also for setting down what appears to be a reliable and well-written account of Malthus's life to replace the few short and error-laden essays upon which readers have previously had to rely.

The author has thoroughly immersed herself in the period. Given the sparseness of material concerning Malthus directly, she has filled the book with information concerning most of the people of any importance whom he knew, the places he lived in or visited, and the ideas with which he came into contact. She also seems to have quoted in part, and frequently entirely, every one of Malthus's surviving letters. While this certainly sets the scene, the reader at times comes to believe he has been perhaps too generously provided for.

Ideally, Malthus's biographer ought to have a good understanding not merely of economics but of the history of economic thought. In both these respects, we are somewhat let down. We do not get a satisfactory sense of what Keynes had in mind when he complained that the triumph of Ricardo's system over that of Malthus had "constrained the subject for a full hundred years in an artificial groove." Although James agrees, not being an economist she is unable to provide a persuasive defense of this view. Although she does provide us with a good understanding of the *Essay on Population* and with some sense of the uproar that greeted its publication, she is less able to provide a satisfactory account of the much-neglected but important *Principles of Political Economy*. Such critical questions as the debate on Say's Law, for example, are passed over all too superficially.

Still her book will prove useful to historians of economic thought. Her discussion of the relationship

of Malthus and his foremost disciple, the Scottish clergyman-economist Thomas Chalmers, displays a socially inept Malthus neglecting the Scotsman's admiring letters and thereby causing great offense. Reading the correspondence, we can be persuaded that such faults were indeed obstacles to the formation of a Malthusian school to rival that of Ricardo. But when the author provides the same explanation for McCulloch's profound distaste for Malthus's economics, she is less convincing. She does not explore the meaning of Malthus's system as a full social and economic outlook, which would alone sufficiently account for McCulloch's antagonism as well as for the later fierce and at times scurrilous attacks of Marx, who never waited in vain for a letter of reply from Malthus.

In England biographers and historians have generally been placed into two separate classes, and it may be thought unfair to judge James by the standards of the latter. As a biographer, she writes effectively and engagingly. She has certainly performed useful service in collecting the facts of Malthus's life and in correcting the errors that have become almost fixed. But Malthus was first of all an economist, and the reader will find the book's treatment of economic questions disappointing. Perhaps the publication of this fine biography will stimulate the writing of a thorough treatment of the Malthusian system.

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MICHAEL RUSE. *The Darwinian Revolution: Science Red in Tooth and Claw*. Chicago: University of Chicago Press. 1979. Pp. xv, 320. \$20.00.

In 1910 the geologist J. W. Judd told "the story of a great revolution in science" in a textbook entitled *The Coming of Evolution*. Seventy years later Michael Ruse, a philosopher of science, has furnished students with a similar account, drawing on recent Darwin scholarship to tell the "story" of how "the organic origins problem" was solved in Great Britain between 1830 and 1875.

It is gratifying to find a contemporary philosopher of science taking a detailed interest in his nineteenth-century counterparts, and for this reason *The Darwinian Revolution* deserves serious consideration from historians interested in John Herschel and William Whewell. As a philosopher Ruse has also studied the works of his story's other dramatis personae, among them Charles Lyell, Adam Sedgwick, Richard Owen, T. H. Huxley, and the hero, Charles Darwin. A discerning reader will not allow the author's evident dislike for some of his characters, and his unabashed admiration for others, to

detract from the enduring value of his philosophical discussions.

As narrative history, on the other hand, the book may well interest students chiefly because much of the writing is qualitatively indistinguishable from their own compositions. Granted the merits of a racy style in reviewing complex theoretical problems for lay persons, there is still no excuse for Ruse's surfeit of clichés, mixed metaphors, and historical howlers. Feathers are ruffled, woods are missed for trees, and cake is had and eaten on more than one occasion. Darwin's barnacle books are "pregnant with veiled hints" about evolution, and Ruse's own narrative is structured around "threads" that "throw light" on the course of events. "The 1860s were a very different time from the 1840s" when, according to another context, time did not stand still. Herbert Spencer (1820-1903), who "was Victorian in more things than sex," "filled volume after volume as only nineteenth-century writers could." Et cetera. "Essentially," as Ruse is fond of saying, the writing and editing bear many marks of haste.

More serious still, the story's plot is as deficient as its style. Ruse never attempts to justify the term "revolution," presuming evidently that readers will find metaphors as unproblematic as he does. The "Darwinian Revolution" is taken loosely as "the whole series of ideas and events leading up to and away from" the *Origin of Species* in 1859 (pp. ix-x), a "movement" whose scientific, philosophical, religious, and sociopolitical "strands" are "intertwined" but ultimately distinct and separable, as indeed they are treated in the book. The movement gathers momentum after "the hero" enters the scene in 1830—the scientific community was "moving toward evolutionism"; natural and revealed religion were "preparing the way," even "unconsciously"; Darwin was following "his path to discovery"—and the movement retires like Darwin himself, exhausted and triumphant, in 1875.

None of which is a great advance on Judd's historiography of seventy years ago. But it reminds the critic, ironically, of the shrewd judgment of the philosopher who observed, "Nothing is ever quite so straightforward as it appears in textbooks" (p. 65).

JAMES R. MOORE
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NEAL C. GILLESPIE. *Charles Darwin and the Problem of Creation*. Chicago: University of Chicago Press. 1979. Pp. xiii, 201. \$16.50.

Drawing on the work of Michel Foucault and Thomas Kuhn, Neal C. Gillespie examines Darwin's theory of evolution in terms of the epistemic crisis and paradigm shift from "creationism" to "positivism" in nineteenth-century Anglo-Ameri-

can science. Gillespie's essay demonstrates that an understanding of the Darwinians must take full account of the fact that they lived in an age of not one but two conflicting epistemes, each invoking different standards of scientific knowledge and theories of nature. More significantly, Gillespie argues that the Darwinian epistemic shift was not sudden or discontinuous; to the end of his life Darwin continued to "promote one episteme while never breaking free of another" (p. 7). This fact makes the study of Darwin and his age fascinating and yet complex and resistant to simplistic classifications.

Two chapters examine the views of the Anglo-American scientists who continued to hold to some notion of special creation or, under the growing influence of positivism, retreated to "scientific nescience" concerning the true causes of species origination and examine how, for Darwin's developing positivism, all of these became dangerous obstacles to scientific advance. Two further chapters detail Darwin's *scientific* attack and his *theological* objections in the *Origin* to special creation, teleology, and nescience. The scientific arguments have been rehearsed many times, but Gillespie's treatment of Darwin's theological preoccupations underlines the fact that Darwin's career and historical role are more complicated and ambiguous than is often recognized. "To read the *Origin* solely as a positivistic work . . . is to reinterpret it from a post-Darwinian perspective" (p. 124).

Darwin's "double vision" is evident in his continuing worry over the moral implications of the waste and cruelty of nature. Forced to break the link between the moral and natural world, Darwin paradoxically sought to preserve it as well. The cost involved removing God from nature and denying any idea of a divine providence. God's relation to the world became increasingly inexplicable—that is, impossible for Darwin—yet, as Gillespie shows, into the 1860s and 1870s "elements of the creationist and positivistic epistemes coexisted in Darwin's mind in a loose, paradoxical and curiously unantagonistic way" (p. 125).

The heuristic use of the concepts of "episteme" and "paradigm change" helps illuminate both Darwin's personal development and the move in Victorian science toward positivism. However, the principal value of this study lies in Gillespie's demonstration that there was indeed a *real* conflict of Victorian science and religion and that the conflict was over *ideas* as well as over political power and leadership. Gillespie underscores the unsoundness of the contention of some historians—for example, Owen Chadwick—that "no enquiry in the realm of the physical could produce results in the realm of the spiritual." The Darwinian epistemic shift was an event of incalculable significance for religion. Although it is unquestionably true that the

Darwinian revolution did not require the surrender of religion as such, Gillespie leaves us to ponder further his simple yet suggestive remark: "That religion could continue under such terms often concealed from participants what had actually occurred" (p. 153).

JAMES C. LIVINGSTON
College of William and Mary

MICHAEL ADAS. *Prophets of Rebellion: Millenarian Protest Movements against the European Colonial Order*. (Studies in Comparative World History.) Chapel Hill: University of North Carolina Press. 1979. Pp. xxvii, 243. \$19.00.

Studies of millenarianism have assumed the proportions of an industry in recent years, but few of the emerging articles and monographs have been genuinely comparative; most anatomize a single case or string several together without exploring links between them. Thus Michael Adas's *Prophets of Rebellion* is an especially welcome contribution, for it explicitly compares five quite different non-Western movements: the Java War of Dipanagara's forces against the Dutch (1825–30); the Maori Pai Māre movement (1864–ca. 1867); the Birsā rising in East-Central India (1899–1900); the Maji Maji rebellion in German East Africa (1905–06); and the Saya San rebellion in Burma (1930–32).

The selection accomplishes several goals at once. It makes accessible to American scholars information on movements relatively little studied outside Europe (the Java War and Pai Māre have been notably slighted in the past). The five also include three different colonial regimes—Dutch, British, and German—as well as a century of colonial experience during which the character of European penetration changed. Finally the cases are dispersed over a geographical-cultural arc incorporating diverse economies, social structures, and topographies.

Adas has wisely chosen to avoid merely stringing cases together. Save for introductory narratives of each that provide a frame of reference, the book is structured around themes addressed to all five: causes, mobilization, impact, and so forth. In the course of the analysis, Adas displays a truly impressive grasp not only of the historical materials but also of social scientific literature as well. Other works on non-Western millenarianism may have encompassed a larger number of movements; none has been as carefully structured.

If anything negative may be said, it is that the theoretical perspectives tend to be applied in a less intensive manner than might be desired. The range of themes is so large that few receive as full a treatment as they require. Thus, the section on causation is based upon a relative deprivation model, and, al-

though Adas is clearly aware of the criticisms directed against relative deprivation, neither the model nor the evidence is presented in sufficient detail to ease misgivings. On the other hand, there are striking and original discussions one wishes had been prolonged. The analysis of relationships between millenarian prophets and their more bellicose subordinates illuminates our knowledge of the ambiguous links between leaders and followers and of the roots of violence in millenarian uprisings. The discussion of that tangled subject—the rationality of non-Western societies—is as balanced as it is insightful. These finely wrought "islands of theory" (always, by the way, related to the data) are at once a delight and a frustration—clarifying while at the same time crying out for more extended treatment. One area that does receive full development is the discussion of magic as a device for neutralizing European military power. The phenomenon of talismans against bullets (repeated in North America during the Sioux "Ghost Dance") fascinated earlier writers but was often merely the occasion for a recitation of exotic anecdotes. Here the matter is explored with a refreshing combination of sympathy and intellectual rigor.

Since all five instances ended in defeat, there is a temptation to categorize such eruptions of discontent as (in E. J. Hobsbawm's dismissive phrase) "pre-political." As Adas suggests, however, they varied widely in the military challenges posed as well as in the amelioration of conditions that followed defeat. And in the end what commends this superb study is precisely the rare accommodation of both uniqueness and generality, building a broad, cross-cultural understanding of millenarian movements without sacrificing the special circumstances of each instance.

MICHAEL BARKUN
Syracuse University

MICHAEL HOWARD, editor. *Restraints on War: Studies in the Limitation of Armed Conflict*. New York: Oxford University Press. 1979. Pp. viii, 173. \$16.95.

Michael Howard's introduction to this fine collective work repeats some points made in his *War in European History* (1976). Western Europe's now "traditional" restraints on war began with agreements by the seventeenth-century's "new" monarchies to limit—in organizations that possessed "exceptional sanctions for the enforcement of obedience"—the actions of their own armed forces. The solution to the destructiveness of revolutionary and religious wars was their prevention. The destructiveness of colonial war was to be mitigated by the conquerors' Christian consciences rather than by international law.

As described in paralleled chapters by Geoffrey Best, Bryan Rantft, and D. C. Watt, the chief problem before 1945 was to adjust the resulting rules of land, sea, and air warfare to protect civilians and neutrals against the increasing power and range of the new weapons of the Industrial Revolution. War itself was "outlawed" in 1928. Less than two decades later a series of international tribunals found particular individuals guilty of crimes against peace and humanity and of belonging to political organizations that had conspired to commit those crimes.

By making 1945 the turning point of their book, the authors may inadvertently reinforce the common Western view of the problem as one of restraining military technology. Though deterrence itself is not a legal problem, it has made many other legal problems ambiguous. Nobody knows, John C. Garnett remarks, whether conventional weapons can be used in an unlimited way in a bargaining process that stops just short of nuclear war or whether such weapons are also subject to new restraints, such as those that prevented attacks on Vietnamese dams, which, under World War II rules, were quite legitimate targets. Laurence Martin's chapter on limited nuclear war points to other ambiguities. Using "tactical nuclear weapons to achieve a militarily meaningful demonstration of resolve" has to be discussed because soldiers and politicians may not think it a wholly fanciful topic.

D. P. O'Connell's discussion of war at sea since 1945 also suggests that many of the new restraints on sea power stem less from the traditional sources of maritime law than from those changing concepts of war criminality by which, G. I. A. D. Draper remarks, all self-declared wars of national or racial liberation are "just" and their perpetrators incapable of criminality. Whether the new revolutionaries will, like Europe's new monarchies, eventually adopt rules to protect themselves from later revolutionaries is an open question. Meanwhile both the developed black hats and the underdeveloped white hats have savaged almost every possible "Rule of the Humanitarian Law of Armed Conflict."

Nitpicking so short a work on such a major topic is relatively easy. Almost everyone will have some quarrels with the lists that each author has suggested for further reading. But this work is so well done on a major issue of historical and human concern that it is far more important to recommend it for careful and thoughtful reading.

THEODORE ROPP
Duke University

ANCIENT

ELLIS RIVKIN. *A Hidden Revolution*. Nashville: Abingdon. 1978. Pp. 336. \$12.95.

This deeply personal and religious statement of the definition and meaning of the Pharisees, a sect in ancient Judaism, emerges from nearly four decades of reflection, including two of active research. The Pharisees are presented as "a class of audacious revolutionaries." Their principal doctrine concerned a two-fold revelation of God to Moses at Mount Sinai, part in writing, part orally formulated and transmitted, and, later on, taught by the Pharisees themselves.

This thesis is worked out through a careful study of the three principal sources that refer to the Pharisees, passages of the Gospels, allusions in the historical writings on Josephus, and sayings, tales, and fables in the latter rabbinic literature about sages who lived before the destruction of the Temple of Jerusalem in A.D. 70. Rivkin analyzes the information in these several sources and then undertakes a historical reconstruction. The third part of the work is his synthesis of the results of the thoroughgoing analysis of the sources. The Pharisees were "a scholar class, championing the twofold Law (Torah) and enjoying great power and prestige"—that is the sum and substance of the matter. From this definition, Rivkin proceeds to ask when the Pharisees came into existence. It was in 280 or 180 B.C., and the book of Ben Sira, which represents a "pre-Pharisaic and pre-twofold Law society" indicates the date must be the later of the two. The chief result of this Pharisaic revolution was to incorporate within Judaism "major structural components and major conceptual notions prevalent in the Greco-Roman world." The book is written vividly, but with control and taste. It is a model of first-class historical argumentation and exposition. The author's personal conviction of the authority for his own religious life of the facts he relates in no way intervenes in his lucid mode of thought. What is advocated is a historical position, and, only secondarily, a pious evaluation of that position.

The principal difficulty is that the allegations of all the sources constitute the starting point for argument and analysis. The literary problems to be solved before the sources are to be believed as fact simply are not taken up. The fact, for example, that the Mishnah and the later rabbinic writings come four or more centuries after the events of which they speak and the men to whom they attribute sayings, does not stop Rivkin from using them any way he wants. True, Rivkin recognizes that the Mishnah comes long after the Pharisaic "revolution" has run its course. But the character of the Mishnah's evidence for that "revolution" nonetheless is taken to be definitive. Josephus is cited as a totally accurate reporter of events. The same is so for the other sources assembled to form the book's thesis. It would not be fair to call the work wholly gullible and credulous, let alone fundamentalist. Rivkin

takes a position quite independent of his sources. But the position is built out of essentially unanalyzed sources. The problems of using those sources for historical reconstruction are not systematically and rigorously confronted. So for Rivkin, the sources present facts, and the facts define the problem. In my view, the sources themselves constitute the first and principal problem. Still, this fundamental difference of opinion on method in no way prevents recognizing Rivkin's imaginative and often profound contribution to the interpretation of Pharisaism as a religious movement.

JACOB NEUSNER
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NOBERT BROCKMEYER. *Antike Sklaverei*. (Erträge der Forschung, number 116.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1979. Pp. xv, 392.

Few historians doubt that slavery played a fundamental role in Greco-Roman antiquity, but its study has been far less important in historical writing than its position in ancient society seems to warrant. A volume that attempts to summarize and synthesize modern research into ancient slavery—even one that fails to make a major intervention in current debates—is therefore to be welcomed.

Norbert Brockmeyer's *Antike Sklaverei* is an extended research report. It begins with a relatively brief overview of research methodologies and perspectives, focusing on problems of definition, current research problems, and general viewpoints regarding the place of slavery in ancient society. Much attention is given to a supposed seminal division between Marxist and non-Marxist work. A much longer section follows, providing a more detailed, chronological overview of slavery and related institutions from the Bronze Age to the later Roman Empire.

Brockmeyer provides the reader, especially the nonspecialist reader, with much useful bibliography, some idea of the varied research projects currently being pursued (his sections on Eastern European work and that of the Mainz Academy will be of special interest to English-speaking readers), and a good deal of useful information on particular aspects of slavery. The bibliographical notations, the brief summaries of individual positions, and the analyses of recent work on ancient source material are not always as critical or informative as they might have been, but such failings are perhaps unavoidable in a relatively brief survey of so large and varied a subject.

The work's conceptual failings are more disturbing. The counterposing of Marxist and non-Marxist perspectives, which at first sight seems rational and even progressive, breaks down upon closer in-

spection. Marxist historians of slavery have hardly been so unified in their points of view as to form a single, easily defined grouping, and Brockmeyer's tendency to view Marxists as uniformly eastern European is not helpful. Nor is work done within a broadly Marxist tradition necessarily sharply antithetical to other research; the essays of M. I. Finley, for example, demonstrate the fruitful possibilities of a sophisticated synthesis of non-Marxist and Marxist points of view.

For all his emphasis on Marxism, Brockmeyer has not completely assimilated an essential element of the Marxist tradition. Although he does recognize the importance of the Marxist insistence that empirical research is inadequate when unaccompanied by a broad focus on the general role of slavery within ancient society as a whole, he fails to carry this insight through in this volume. His definitions of slavery seem inadequate, and the wider social position of slavery tends to be forgotten; what is too often substituted is a rather eclectic sampling of research on particular points, which are left unconnected and without a broader context. Chapters that do attempt the broader project, those on the Roman Empire (chaps. 12–14), are the best in the book and hint at what might have been accomplished elsewhere.

These conceptual failures derive, of course, to a large extent from the state of the field itself. It is surely impossible to summarize the "facts" of a field in which there is so little agreement on the very nature of the main problems and their relationships to the wider questions of ancient history. Brockmeyer might well have written a better book had he devoted it entirely to the question of research perspectives and current controversies over the very nature of ancient slavery, better delineating the strengths and weaknesses of the various points of view and showing more precisely where the field actually stands at this moment. This task remains to be accomplished.

ROBERT A. PADGUG
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ROBIN SEAGER. *Pompey: A Political Biography*. Berkeley and Los Angeles: University of California Press. 1979. Pp. 209. \$25.00.

The career of Pompey the Great is a major puzzle in the complicated history of the Late Republic, the best-illuminated era of ancient history. First came his meteoric rise to a triumph in his twenties, against the disapproval of the fearsome dictator Sulla. Then he was hatchetman for the Senate in Italy and Spain, consul with Crassus in 70, and thereafter a leading figure until his fatal duel with Caesar.

Robin Seager, senior lecturer at Liverpool, has written a close-packed survey of the era, well buttressed with references to sources and modern studies but too detailed in fact to appeal to a beginning student. While one may differ from his view on any one disputed point—and there are many in the period—his assessments are always reasonable and carefully argued. His tale, however, is an oft-told one, recently by Erich Gruen in *Last Generation of the Roman Republic* (1974); the main problem still remains, namely, what were Pompey's own goals and plans?

The name of Pompey is often introduced in Seager's pages, but one never does get a clear view of the man himself. By implication the view arises that Pompey, by and large, reacted to each turn of politics on its own, seeking to retain his position as leading citizen of Rome, anxious to have men's good opinion, really afraid of the dangers of assassination. In his last pages Seager more directly supports one ancient judgment "that Pompeius could tolerate no equal in power and esteem, Caesar no superior" (pp. 186–87) but also cites the poet Lucan's inability "to make up his own mind where the truth lay."

Why this ambiguity? Caesar spoke for himself in his commentaries; Cicero's views are abundantly evident in his speeches and letters; Pompey, alas, is only a man, in the mirror especially of Cicero's work, who almost never stands alone on the stage. Beryl Rawson, parenthetically, has recently surveyed the uneasy relations of Cicero and Pompey in *The Politics of Friendship* (1978). Maybe Pompey did not wish to speak out clearly, for he was a man who rose through war, not politics; but this seems a judgment of unnecessary despair. J. van Ooteghem wrote a 600-page study of Pompey that J. P. V. D. Balsdon summed up (*Journal of Roman Studies* [1955]), "as concerns Pompey and the problems about Pompey, the book leaves things very much as they were before its publication." Much the same can be said of the present work, which could have been better edited (for example, repetition of facts and footnotes on pages 2 and 4, 36 and 38) and more carefully proofread. There is a good bibliography and an adequate index of proper names.

CHESTER G. STARR
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THOMAS N. MITCHELL. *Cicero: The Ascending Years*. New Haven: Yale University Press. 1979. Pp. xii, 259. \$17.50.

Since other biographies favor the well-documented years from 63 onwards, Thomas N. Mitchell's monograph on Cicero's early life fills a real

need. Four long chapters analyze Cicero's background and upbringing, his long apprenticeship under such figures as Lucius Licinius Crassus and the two Quincti Mucii Scaevolae, his advance up the ladder of magistracies, and his consulship. While Mitchell leaves aside the technical development of Cicero's rhetoric, he portrays Cicero's formation as a politician and political thinker against the background of socioeconomic developments that affected the late Roman Republic. He uses prosopography carefully and takes a judicious position on the question of factions among the ruling elite.

On the other hand, Mitchell's view that the admitted strife for honors, *dignitas*, and influence within the nobility "was almost constantly overshadowed by a more basic concern to maintain the aristocracy as a whole in a stable position of power in the face of growing threats" (p. 19) lays too much stress on aristocratic unity. The disruptive ambition and opportunism of men like Marius, Sulla, Cinna, Lepidus, Pompey, Crassus, Catiline, and Caesar should not be seen as less significant, but as symptomatic of aristocratic politics in the late Republic, even in the 90s, which Mitchell views too rosily. Naturally, the nobles pre-eminent at a given time sought to preserve the status quo in the face of disruptive changes, but some aristocrats were always willing to exploit those changes in order to advance themselves, so that the aristocracy lacked the degree of unity necessary to treat problems adequately or retain control.

Overemphasizing aristocratic unity also leads to a distorted interpretation of Cicero's political ideals. Mitchell believes that Cicero's ideas reflected the interests and policies of the oligarchy and "may also safely be regarded as representative of the conservative *nobilitas* as a whole" (p. 197). That Cicero's conceptions of the Republic and the ideal aristocrat were molded by association with Lucius Crassus and the Scaevolae does not mean that the rest of the nobility equally shared these ideals. The men about whose higher thoughts anything is known are revealed primarily through Cicero, who, naturally, presents a sample biased in favor of those with whom he sympathizes. While a majority of nobles would have shared Cicero's concept of their rights, they are less likely to have shared his view of their duties. Although limited, Cicero's vision of the *via optima* went beyond that of most contemporaries.

On many other points Mitchell's analysis is judicious and astute: compare Sulla's march on Rome, Cicero's attitudes towards Cinna and Sulla, and the trial of Verres. After his treatment of the notorious Rullan land bill no more need be said. Similarly valuable is the discussion of the *Senatus Consultum Ultimum*. The social and economic background of the Catilinarian Conspiracy is deftly filled in, and the presentation of Catiline, his actions, and his motives

is difficult to improve. Clearly, this book is required reading for those interested in Cicero and the late Roman Republic.

ALLEN M. WARD
University of Connecticut

J. H. W. G. LIEBESCHUETZ. *Continuity and Change in Roman Religion*. New York: Oxford University Press. 1979. Pp. xv, 359. \$49.00.

This book is well written and its thesis well researched. Beginning with the era of the late Roman Republic, J. H. W. G. Liebeschuetz traces the development of chosen aspects of Roman belief, both religious and philosophical, down to the fourth century. He emphasizes philosophical problems of morality and indeed gives particular attention to stoicism as a kind of religious creed.

Because of his choice of evidence, however, the author gives scant attention to nearly two centuries of the period he treats. In his introduction (p. xv) Liebeschuetz admits partiality in his choice of evidence. He selects from and follows the writings that give expression to the "religious attitudes of the Roman establishment . . . worth studying even in isolation." From what has he isolated these ideas, many of which were alien to a Roman's notion of religion? Finding "it necessary to be selective," he has paid little heed to the bulk of the evidence on Roman religion, namely the religious lore nested in antiquarian authorities and the mass of epigraphic documentation recording the religious sentiments of high- and low-born Romans alike. The reader may be somewhat puzzled when the author discusses the general absence of inscriptional evidence occurring in the later third century since he has received no indication of the nature and extent of that precious material for the rest of period under discussion.

What Liebeschuetz has achieved is an intellectual history of selected literary authorities' views on some religious practices and moral beliefs. The authorities represent both learned disquisition (Cicero, Seneca, Arnobius, Lactantius) and belletristic endeavors (Lucan, Silius Italicus, Tacitus, Pliny the Younger, Apuleius) but none reflects the regular cult practices of Roman religion of all social classes. Besides treating moral problems, Liebeschuetz gives due space to such matters as magic and astrology.

Liebeschuetz's contribution lies in the theme of social crisis, political reform, and attendant beliefs affirmed or newly adopted by government to foster religion and morality. His treatment synthesizes many works of predecessors in promotion of this thesis. Having chosen a course of considerable selectivity, Liebeschuetz cannot claim to have exhausted his subject or, for that matter, to have dealt with Roman religion. On the other hand, he has suc-

ceeded in what he set out to do. Continuity and change are so well handled in terms of the intellectual history of some religious beliefs and moral problems that on reaching the conclusion the reader recognizes the coherence of the subject from polytheistic paganism to monotheistic Christianity.

ROBERT E. A. PALMER
University of Pennsylvania

HILDEGARD TEMPORINI. *Die Frauen am Hofe Trajans: Ein Beitrag zur Stellung der Augustae im Principat*. Berlin: De Gruyter. 1978. Pp. xii, 295. DM 112.

The two parts of this book amass the evidence for the spouses of Trajan and Hadrian, Pompeia Plotina and Ulpia Marciana respectively. Each part begins with family origins, continues with an elaborately documented narrative of the woman's career, and concludes with a lengthy discussion of her deification. There are full indexes of ancient sources, proper names, and modern authors together with selected illustrations and a stemma.

The foreword (p. ix) advises that the book began its life as a doctoral dissertation. Traces of that genre remain. Thus one notes gratuitous displays of learning in the bare *PIR* references to Livia (p. 27 n. 110) or Gaius and Lucius Caesar (p. 36 n. 178). Again, the notes frequently present long lists of modern treatments of the topic under consideration without any indication of the different viewpoints of those modern treatments. Undigested ancient evidence also appears, such as the long lists of coins (pp. 100-04); one notes in passing that numismatic evidence is usually far less reliable than the author's use of it would imply. Such accumulations of evidence ancient and modern often seem tangential: one questions the relevance for the author's topic of so much data for consuls from the provinces (pp. 20-21).

Footnote virtuosity per se does not constitute the quarrel with this book. Rather, one questions the tacit assumption prevalent among ancient historians that their craft consists of offering an annotated anthology, as it were, of facts. Description, of course, has an important role, but not at the expense of interpretation. And the false emphasis on facts ultimately distorts the sources it purports to treat faithfully. For example, it is incapable of filtering out the various unconscious distortions that ancient authors may have injected in their writings. Again, the only questions description asks are those that the ancients seem to suggest—these do not necessarily represent the most helpful questions. Keith Hopkins's penetrating remarks in this regard (*JRS*, 68 [1978]: 178-86) acutely demonstrate how a preoccupation with facts produces essentially ersatz history; my review agrees with his strictures.

Interpretation must go beyond discussing points of evidence. It should probe the relationships between those points and their larger social contexts. Consider the deification of the two women in Temporini's book. What was the social constituency for those deifications? Would a *colonus* be as impressed as a senator, and for what reasons? In a male-dominated society one wonders about the socioreligious import of such elevations. How did they affect the power structures of either gender? The book's methodological innocence never raises such questions.

Temporini has produced, in effect, a series of factual surveys of two imperial women and their spouses. As such it marks a valuable start. But the book is description, not history. It is sad that ancient historians so seldom recognize that one of the founders of their craft, Herodotus, emphasized (1.5.3-4) the necessity both of facts and interpretation in the historical enterprise.

CHARLES ROBERT PHILLIPS III
Lehigh University

TIMOTHY E. GREGORY. *Vox Populi: Popular Opinion and Violence in the Religious Controversies of the Fifth Century A. D.* Columbus: Ohio State University Press. 1979. Pp. xii, 245. \$20.00.

"No wild beasts are such enemies to mankind as are most Christians to each other." Timothy E. Gregory's book substantiates only too well these sardonic words of the fourth-century historian Ammianus. His theme is the religious controversies of the fifth century and the violent popular involvement therein. As illustration, Gregory analyzes four episodes: the episcopacy of John Chrysostom; Nestorius and the Council of Ephesus; the Latrocinium (or Robber Council); and the Council of Chalcedon.

The selective method has its dangers, chronological restriction in particular. The fifth century was not unique. Donatists in Africa or the riots in Constantinople under Anastasius (for easy example) could equally well have been exploited for a book on this theme. Nevertheless, Gregory is on balance right to confine himself to specific cases. The last thing the world needs is another tissue of generalities on the nature of violence.

Which is not to say that Gregory was wrong to offer some by way of introduction and conclusion. Historical texts, especially the sort he has to work from, do not always speak for themselves to the professional scholar, let alone the general reader whose attention this book fully deserves. Gregory's summarizing remarks are admirably level-headed. He is the slave of no "ism," rejecting the panaceas of Marxism and trendy sociology in favor of the plain facts of Byzantine history and human nature.

People, uneducated and otherwise, are easily seduced into violence when they have few outlets for legitimate protest. Religion afforded particular impetus since a man's personal salvation depended on both himself and the state getting their beliefs right.

A few details are inaccurate. The regular number of circus factions in the early Empire was four, not six (p. 28)—six was a temporary aberration under Domitian. The celebrated riot at Ephesus was not a popular one (p. 31): *Acts* 19: 23-28 shows that only the vested interests of the silversmiths were involved. The account of Chrysaphius's execution (pp. 166 and 194, n. 14) ignores Malalas's statement that he was liquidated because he was a prominent Green.

There are some trifling misprints: "unfortunately" (p. 96), "increased" (p. 157, n. 62), "Theophenes" (p. 186). The bibliography has the merit of being designed for use rather than ostentation (though J. R. Martindale's thesis [p. 239; also p. 14, n. 14] is an Oxford B.Litt., not a B.A.). The index, unfortunately, is too exiguous and arbitrary to be of much service.

The style is on the stodgy side, albeit mercifully jargon-free. But Gregory, thanks to his enviable mastery of a jumble of disparate and desperate sources and to his sanity of judgment and clarity of exposition, has written an important book on a subject that, as recent events from Ireland to Iran show, remains all too timely.

BARRY BALDWIN
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MEDIEVAL

JAMES MULDOON. *Popes, Lawyers, and Infidels: The Church and the Non-Christian World, 1250-1550.* (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1979. Pp. xi, 212. \$15.00.

As indicated by the title, this book deals with the intercultural contacts between Christian Europe and its non-Christian neighbors between 1250 and 1550. These Christian-infidel relations were conducted according to the theories and rules of canon law, which authorized or justified all such interaction for popes and Christian kings alike.

Innocent IV, a noted canon lawyer before he became pope, is the central figure in this study because he fused legal theory and papal practice. This influential pope and canonist believed that Christians could and should live in peace with their non-Christian neighbors and that the infidels had natural rights to land and their own government—they could not legally be deprived of their property and freedom simply because they were not Christians. Innocent IV saw the entire world in ecclesiastical terms, and to him all infidels were potential mem-

bers of the Church. He believed that the papacy had the right to send peaceful missionaries into all non-Christian societies and that military attack was justified if the infidels refused to accept these missionaries. Pope Innocent IV thus favored a policy of sending religious embassies to achieve peaceful relations with infidels. His Asian mission stressed reaching the Mongols by means of conversion; most of his letters to the Mongols are religious in nature and stress conversion to Christianity, not diplomatic and military alliance. Thus, in accord with the long tradition of Church-State conflict in Western Europe, Innocent IV and later canonists concluded that, for spiritual reasons, the papacy could intervene at will in all infidel societies just as it could in secular affairs.

In the succeeding centuries, Innocent IV's views on the legal rights of infidels in the face of Christian contact and expansion were generally continued. Even the Avignon popes were actively involved in relations with infidels, as revealed by letters in the Papal Registers. These letters are still basically religious, and many are nothing more than a reissue of previous papal pronouncements.

In the fifteen and sixteenth centuries, secular states began to replace the papacy as the leader of European society and, in the process, papal and canonist influence began to be pushed into the background. The Christian conquest of the Atlantic Islands and the Americas was considered inevitable, and the papacy emerged as the mediator as to whether Portugal or Spain would be in charge of protecting the peaceful Christian missionaries converting the hostile primitive natives. But after a religiously and legally correct conquest, the economic, political, and diplomatic interests of Portugal and Spain took precedence over the legal rights of the infidels and the views of the papacy. When this happened, European intercultural contact had passed beyond its medieval canon law framework and had entered the era of modern international law; secular rather than religious factors would determine how Christians related to their non-Christian neighbors.

This book is a well-documented study. The two basic sources are the papal letters of the thirteenth and fourteenth centuries and the writings of the canon lawyers of the fourteenth and fifteenth centuries. Much of the material is drawn directly from these two sets of primary sources. The footnote and bibliography references are solidly grounded in canon law studies as one would expect in a legal study of this nature. It is only when the author reaches beyond his canon law core of expertise that one notices omissions in his bibliography. He spends a considerable amount of time talking about the Mongol mission initiated by Innocent IV and continued by subsequent popes, but one fails to find

any of the numerous articles on this subject by Denis Sinor and Jean Richard. Their most important articles of the last four decades have been conveniently assembled and published by Variorum Reprints. Also conspicuous by their absence are the books of Gian Andri Bezzola, *Die Mongolen in abendländischer Sicht (1220-1270)* (1974), Jean Richard, *La papauté et les missions d'Orient au moyen âge (XIII-XV siècles)* (1977), and William Urban, *The Baltic Crusade* (1975). Recent works by the above authors would have added to the material on the Mongols and Lithuanians, although the last Richard volume may not yet have been available when this manuscript was originally submitted for publication.

The author's extensive work and publication in the area of medieval canon law make him well qualified to write this legal study. While others have dealt with medieval Europe's relations with various non-Christians from the viewpoint of one specific people or geographic area, this volume is the first to consider them all and to study and analyze these relations from the viewpoint of a common and continuous legal base. By going back to the canon law theory behind late medieval Christian-infidel contact and practice, this comprehensive legal study makes a valuable contribution to the understanding and legal justification of later European expansion in the age of exploration and discovery.

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PIERRE RICÉ. *Les écoles et l'enseignement dans l'Occident chrétien de la fin du V^e siècle au milieu du XI^e siècle.* (Collection Historique.) Paris: Aubier Montaigne. 1979. Pp. 462. 96 fr.

Pierre Riché's earlier book on medieval education, *Education et culture dans l'Occident barbare* (1962), one of the most impressive investigations into early medieval history in our time, has a worthy sequel in the present work. Like its predecessor, this study is based upon an extremely thorough winnowing of narrative, diplomatic, juridical, epigraphical, and archeological sources and provides a richer and more detailed portrait of the evolution of medieval education than we have previously had.

After an introduction summarizing his earlier work on the end of ancient education and the beginning of Christian schools, Riché treats successively of schools during the Carolingian Renaissance, schools from the middle of the tenth century to the middle of the eleventh century, masters, students, curriculum, and methods, and the education of the lay person. These discussions are followed by fifty pages of extracts from texts that illuminate particular points, twenty chronological tables that harmonize religious, political, and educational

events in various parts of the medieval West, an extremely thorough bibliography of secondary literature, an index to authors in the bibliography, and a general index of names.

The period that Riché has chosen to examine is an age that produced remarkably few works on educational theory and practice. Previous historians of education have generally preferred to use a few singular individuals (Cassiodorus, Isidore of Seville, Gregory the Great, Alcuin) as steppingstones across the turbid barbarian flood and have thus grossly underestimated the extent and richness of educational practice in the early Middle Ages. Riché's weighing of the slightest clues from sources as disparate as civil and religious legislation, monastic rules, lives of saints, and library catalogues forces scholars to re-examine the genesis and uniqueness of the twelfth-century Renaissance, just as his earlier work dimmed somewhat the luster of the Carolingian Renaissance. He here contends that "entre le VII^e et le XI^e siècle les conditions de l'enseignement n'ont guère changé" (p. 6) and demonstrates the validity of his view with masses of evidence not previously utilized. He has done so without resorting to the expedient of defining education so broadly as to include virtually all of life and culture; for him, education means masters and students, teaching and learning, curriculum and methods, books and study. He has done so without muddling together lists of evidence from different regions and times; tight chronology and geographical discrimination are characteristic of his work. He has done so without minimizing the impact of individuals; the index lists about six hundred contemporaries mentioned in the book. And he has done so without slighting the often-ignored subject of the education of lay persons; a fascinating section deals with the education of aristocrats both in Latin literacy and vernacular culture, and with the religious education of the laity through the liturgy, catechesis, preaching, and songs.

Riché's previous history of medieval education waited fourteen years for publication in an English translation. It will be a shame if the present work must wait so long.

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NATALIE FRYDE. *The Tyranny and Fall of Edward II, 1321-1326*. New York: Cambridge University Press. 1979. Pp. x, 301. \$25.00.

Natalie Fryde has written an excellent and most stimulating book soundly based both upon well-known chronicles and upon a wide range of record sources hitherto incompletely investigated. Now we have a revised interpretation of Edward II's later years comparable to those produced in recent years

for the middle period of his reign by J. Maddicott and J. R. S. Phillips.

The results are interesting and significant. Contrary to Stubbs's opinion that Edward was the first English king since the Norman Conquest untrained and uninterested in government business, Fryde demonstrates that an almost pathological greed for money (due to inherited debts and the poverty of his earlier years) drove him to a ruthless supervision of the Exchequer officials. Before his deposition he had accumulated a reserve equal to about one year's income. Though this was a considerable achievement, it is perhaps something of an exaggeration to describe such sums as "enormous treasure."

This seems to have been Edward's own personal achievement, and Fryde refutes Tout's contention that the famous Exchequer reforms of the 1320s were due to the Despensers. Deprived of even this achievement, there is nothing left but complete condemnation for this atrocious family and their hangers-on. The Despenser dominance of government in the 1320s must surely have been one of the most bestial (if not the most bestial) regimes in English history. The Despensers terrorized almost every social group in the land—their neighbors in Wales and the Marches, the Lancastrians, the city of London—and yet, as a result, lived in a nightmare of fear themselves. They pursued their victims with a vindictiveness hitherto unknown in English politics. Thomas of Lancaster's execution was unprecedented. Before this aristocrats had been executed only for collaboration with enemies of the realm. The prolonged imprisonment of nobles and their families, including children and elderly relations, perpetrated quite a new brutality in political relationships. Devastation, plundering, and the breakdown of law and order in many parts of the country were, on the evidence that Fryde produces, far, far worse than anything that occurred during the Wars of the Roses. It is little wonder that Isabella and Mortimer were able to conquer with an invasion force of only 1,500 men.

Fryde also deals with the story, based upon the famous letter of the papal notary, Manuel de Fieschi, that Edward II escaped from Berkeley Castle and ended his life in Italy. Unfortunately discussion of this problem is a little superficial in comparison with the rest of the book. This section needs to be read in conjunction with Tout's paper on the same subject (*The Collected Papers of T. F. Tout* [1934]) and the recent article in *Speculum* (July 1978) by C. P. Cuttino and T. W. Lyman that obviously appeared too late for Fryde to make use of.

Except for an excessive liking for passive verbs, this book is very well written. I strongly recommend it both for its style and its scholarship.

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RICHARD H. ROUSE and MARY A. ROUSE. *Preachers, Florilegia and Sermons: Studies on the Manipulus Florum of Thomas of Ireland*. (Studies and Texts, number 47.) Toronto: Pontifical Institute of Mediaeval Studies. 1979. Pp. xi, 476. \$24.00.

Medieval sermon studies are increasingly receiving the attention and recognition they warrant. This book by Richard H. Rouse and Mary A. Rouse is an outstanding contribution to the growing number of works enhancing our appreciation of this field. In 1306, Thomas of Ireland, fellow of the Sorbonne, published the *Manipulus Florum* (sheaf of extracts). As "an alphabetically-arranged topical compendium of *auctoritates*, designed for use in writing sermons" (p. ix), the *Manipulus Florum* was a typical product of its age. Consisting of some 6,000 extracts from patristic and ancient sources, classified according to 266 topics, the *Manipulus Florum* (including prologue and bibliography) would correspond to a modern work of 365 pages. Its audience was the average university-trained preacher who needed a useful reference manual for sermons that were becoming increasingly complex in structure and dependent upon the citation of authorities. The usefulness and popularity of the work are attested by 180 surviving manuscripts and some 47 printings between 1483 and 1887 (described in the appendix). Further, the *Manipulus Florum* became the vehicle whereby twelfth- and thirteenth-century sources were made available to many later writers including not only preachers but also theologians, mystics, lawyers, and vernacular poets.

The authors give initial attention to placing the text in its thirteenth-century background and demonstrate how aids to preaching, such as distinctions, concordances, and subject indexes, were linked to the growing importance of preaching. We learn here, for example, that medieval Europe had an effective biblical concordance only in the 1280s. New techniques had to be adopted: developments of book and page layouts and the emergence of reference systems, including the adoption of Arabic numerals and the acceptance of alphabetical order as a means of arranging words and ideas. The authors correctly stress the importance of alphabetization of materials (Thomas of Ireland's significant contribution) and point out that this was not necessarily in accord with the medieval view: "to discuss *Filius* before *Pater*, or *Angelus* before *Deus*, simply because the alphabet required it, would have seemed absurd" (p. 35).

Part 2 contains biographical material on Thomas of Ireland and a treatment of the structure, sources, and history of the *Manipulus Florum*. Thomas of Ireland intended his bibliography, the most elaborate of its type in this period (and here edited with the prologue for the first time), to be used in con-

junction with the original sources. This anthologist wanted to encourage his readers to go beyond the extracts to the complete works. For the medieval preacher whose library resources were scanty, the *Manipulus Florum* served a most important purpose. We are indebted to Richard and Mary Rouse for this fine study of a medieval compendium that, unoriginal as it was, is, nonetheless, an important key to the intellectual life of the age.

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PIERRE DESPORTES. *Reims et les Rémois aux XIII^e et XIV^e siècles*. Paris: Éditions A. and J. Picard. 1979. Pp. 743.

This work treats every possible aspect of the history of the town of Reims: society, economics, and politics (both ecclesiastical and secular), as well as monumental-topographic subjects. It is principally a history of the old *civitas* and immediately adjacent areas. Other parts for which there remain little or no documentation, notably the bourg of Saint-Rémi, are perforce somewhat neglected. The work is divided into three large chronological sections. That from ca. 1150 to 1270 was one of rapid growth marked by international commerce, especially in linens. The period from 1270 to 1338 represented the height and maturity of the community, and that from 1338 to 1435 was marked by population decline, intermittent warfare, and the gradual absorption of this archiepiscopal town by royal control. Pierre Desportes adduces pertinent analogies from other towns in France and elsewhere to illuminate the history of his town.

In terms of population, the author believes that the town reached its peak of about twenty thousand around 1270. The heavy mortality of the years from 1316 to 1320 reduced it to about sixteen to eighteen thousand in 1328. The plague of 1347 took about a quarter of the population and ushered in the troubled period of the later Middle Ages. A remarkable aspect of the population of Reims was the heavy proportion of clergy, about 12 percent. Reims was an archiepiscopal see, its prelate was its prince, and there were no less than three *officiales* with their own courts in town. A result was a relatively high percentage of clerks specializing in law and scribal services. Furthermore, a certain percentage of the ordinary population of the community was, although living in an altogether secular manner, in clericalure and therefore relatively exempt from ordinary taxation. This, together with the continued independence of the bourg of Saint-Rémi, helps to explain why Reims never really defeated the power of its ecclesiastical lord. Lastly, the Jewish popu-

lation was tiny after the first half of the twelfth century.

Because it is impossible to mention all the subjects described in the author's pages, I shall touch on one or two. As elsewhere in Europe, the teens and twenties of the thirteenth century saw an attack on usury both ecclesiastical and popular. Having profited from the old usurious investments, the well-to-do were obliged to change their tune from the 1230s on. They turned to house rents, census contracts, and, to some degree, contracts with interest as a penalty for nonfulfillment. At the same time, an oddity of Reims is that the crafts and trades never became as significant in this period as they did elsewhere, and the patrician families were more or less able to retain stable control of the community.

The important town families were in origin not dissimilar to those in other older *civitates* in western Europe. They were partly knightly and partly burgher, the former being more closely linked to the archbishop than the latter, who had won some self-government by the *Willelmine* charter granted by the archbishop-prince in 1182. The author asserts that the knightly or noble families tended to move out of town in the period around 1300, just before the beginnings of ennoblement for selected burghers who had served the king. The weakness of the documentation here makes the treatment of this subject not quite satisfying. In regard to the burgher patricians, also, the author asserts that the possession of a hereditary patronymic was a "privilege" of old families. I would not have used the word "privilege," but Desportes amply proves that patrician lines regularly employed family names generation after generation. The same was not true, or cannot be proved, of the crafts- and tradesfolk, and the author surmises that a reason for this is that because of emigration, higher mortality, and so forth, humbler families were not liable to have as long a life.

In fine, this is a good book that raises many problems and solves not a few. It is indispensable for those interested in urban history.

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CONSTANCE BRITAIN BOUCHARD. *Spirituality and Administration: The Role of the Bishop in Twelfth-Century Auxerre*. (Speculum Anniversary Monographs, number 5.) Cambridge, Mass.: Mediaeval Academy of America. 1979. Pp. viii, 144. Cloth \$11.00, paper \$5.00.

Constance Britain Bouchard's *Spirituality and Administration: The Role of the Bishop in Twelfth-Century Auxerre* is based on the author's dissertation. It should be noted, however, that the dissertation itself

must still be consulted for documents from the archives of Auxerre that were edited by Bouchard in an appendix to the dissertation. The published monograph discusses the lives of the bishops of Auxerre from 1092 to 1220. The chief source for the description is the *Gesta Pontificum Autissiodorensium*, a continuous series of episcopal biographies for Auxerre from the ninth century to the thirteenth. The chapters of Bouchard's book are chronologically arranged, and one each is devoted to the seven biographies of the *Gesta* that she used in the edition of L.-M. Duru: Humbaud (1092-1114), Hugh of Montaigny (1115-1136), Hugh of Mâcon (1136-1151), Alain (1152-1167), William of Toucy (1167-1181), Hugh of Noyers (1183-1206), and William of Seignelay (1207-1220). In most of these chapters, Bouchard provides some additional information from episcopal charters and from a second independent and contemporary narrative source from Auxerre, the Chronicle of Robert of St.-Marien. When possible, the biographers' bias is evaluated in the light of these outside sources.

It is Bouchard's intention to illustrate the twelfth-century change in the understanding of the concepts of spirituality and administration in the lives of the bishops of Auxerre. She concludes that "over the course of the twelfth century spirituality clearly experienced a relative decline in importance among the qualities desired in a bishop" (p. 142). Spirituality is used by Bouchard in the narrow sense of the word, meaning "the religious dimension of the inner life, that which tends towards God" (Bouchard, p. 14, and A. Vauchez, *La spiritualité du moyen âge occidental, VIII^e-XIV^e siècles* [1975] pp. 5-7. Bouchard misunderstands Vauchez's use of the terms "broad" and "narrow" spirituality.) The virtues and the degree of sanctity that the biographers attributed to the individual bishops in the *Gesta* provide Bouchard with the basis for her estimation of their spirituality, but she does not seem to feel very comfortable with the concept. She notes that by the time of William of Seignelay (1207-1220), "spirituality and administration were . . . essentially one" (p. 143). This astonishing conclusion is merely explained by references to the practically exclusive interest of William's biographer in the bishop's administrative abilities and his admiration for these skills. It is quite true that, as Bouchard points out in her introduction, "by the end of the [twelfth] century there were multiple perceptions of the spiritual life, with different religious goals for different orders of society" (p. 15), but this is not the same thing as saying that administration came to equal spirituality. The *Gesta*, valuable as they are, do not seem to provide sufficient material to analyze something as personal as an individual's inner life or his religiosity, let alone to show how a bishop's religiosity expressed itself in his everyday activity. Bouchard,

who seems to be attracted more by social and quantitative history than by intellectual history, appears not fully aware of the dimensions of her subject, for she never notices that the biographer of Humbaud (1092–1114) “saw a bishop’s excellence as residing not so much in his spirituality as in his administrative accomplishments” (p. 35). This anonymous writer had, therefore, the same attitude toward spirituality and administration as his successor of more than a hundred years later, the biographer of William of Seignelay.

There are some further criticisms: Bouchard usually fails to cite relevant critical editions of primary sources—bibliographical information is at a premium—and the volume is marred by numerous typographical errors. Nevertheless, Bouchard’s book is a welcome addition to the relatively small number of volumes available in English on medieval French history. One may not learn much about “spirituality,” but the monograph brings to life the complexities and difficulties that were faced by bishops in twelfth-century Auxerre. Despite certain shortcomings, therefore, *Spirituality and Administration* is a good book to have. It brings the unusual wealth of information for twelfth-century Auxerre to general attention and should stimulate further research.

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THOMAS F. GLICK. *Islamic and Christian Spain in the Early Middle Ages*. Princeton: Princeton University Press. 1979. Pp. xi, 376. \$22.50.

The interaction and interpenetration of Islamic and Christian societies in the Iberian peninsula during the three hundred years following the initial Muslim invasion is the subject of this book. Thomas F. Glick, whose study of irrigation in medieval Valencia is well known, has directed his inquiry to various aspects of social and cultural contact, arguing against a much older notion that medieval Spain was divided into watertight compartments that did not permit Muslims or Christians to affect one another’s lifestyles other than in a superficial manner. In addition, he takes up the challenge laid down by Américo Castro and Claudio Sánchez Albornoz and gives a perceptive critique of the methods followed by each of them. He has advanced the argument much beyond the point where they left it and makes it clear that the comparatively simplistic view of medieval Spain advanced by them, each from his own position, is no longer tenable.

After an introductory chapter in which he discusses the Castro–Sánchez Albornoz polemic and the movement of ideas and techniques over political frontiers and other barriers, he divides his book into two parts. The first treats the social and economic

development of early medieval Spain, and the second concentrates on the diffusion of techniques and ideas. Among the many topics studied, all from a comparative point of view, are agricultural production, irrigation practices, the movement away from local herding of sheep to transhumance, the use of forest land, the growth of towns and markets, trade and investment, bonds of kinship, social classes, the intermingling of ethnic groups, the process of translation, and the transmission of technological and scientific knowledge from East to West.

In every instance, Glick subjects old assumptions to an intense criticism. So, for example, he questions the assumption that al-Andalus was particularly prey to political instability, indicating that Islamic society was tribal and segmentary and that a highly centralized state was not the norm to be expected in Islamic Spain. Stability was provided in fact by the law, which transcended political boundaries. He notes also that no satisfactory explanation has been given for the fall of the caliphate of Córdoba but suggests that it was a model that no longer suited the needs of society, especially in view of the intense Berber immigration that preceded it. Similarly, he argues that the model of towns in Christian Spain was Islamic, rather than the Germanic *burgum*, whose establishment along the pilgrimage route to Santiago de Compostela has been studied by Valdeavellano. There are, however, some gratuitous assumptions that are accepted without further inquiry, as for example, “the borrowing by Christians of the Islamic *ribā*, or fighting ‘monastery’ and its re-invention as the crusading military order.” The history of the *ribā* in Spain and the Holy Land and its relationship to the military orders, if any, has yet to be studied in depth.

As an Islamist, Glick tells us that his view of Spain may well differ from that of others, especially medieval Hispanists. Indeed this is true, but there is no doubt that all of us are greatly indebted to him for holding the peninsula up to a new light and showing us new facets of this many-faceted jewel.

The apparatus includes several figures and tables and a map illustrating such things as the ratio of Christian and Muslim scientists. There are also a bibliographical essay and footnotes.

JOSEPH F. O’CALLAGHAN
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JIRÍ SPĚVÁČEK. *Karl IV: Sein Leben und seine staatsmännische Leistung*. Prague: Academia, and Hermann Böhlau Nachf., Vienna. 1978. Pp. 208. DM 80.

The six-hundredth anniversary in 1978 of the death of Charles IV, Emperor of the Romans and King of Bohemia, prompted several important historical exhibits and a number of scholarly publications. Most of this activity took place in West Germany and ex-

pressed a view of Charles's work in which the Bohemian or indeed Czech element was only one among others. The book under review is a useful statement of the Czech position (it has been translated from a Czech original) by a scholar who has come to history by way of diplomacy and whose work is correspondingly precise, solidly based on documentary sources, and on the whole rather austere. It does not attempt to set Charles in the richly textured world of late-Gothic Europe, nor does it offer a systematic narrative of public action; it is instead a tightly packed essay designed to present us with a Charles IV who was independent, original, stubborn, creative, and inspired above all by the Czech and Slavonic traditions that were his by right of his mother.

In his drive to become ruler of Bohemia, Charles had to overcome the indifference or hostility of his father; in his acquisition of the German crown ("King of the Romans") and consolidation of his position in the Empire, he had to work against the Wittelsbachs and later against the wishes of the Avignon papacy; in his struggle to create an effective royal government in Bohemia, in line with the Přemyslid tradition, he had to fight the barons of the land. His overall aim was to create the forms that would give permanence to what he himself had achieved: an empire held together by an emperor who was also King of Bohemia, with both the imperial and Bohemian titles hereditary in the Luxemburg family, and with "Bohemia" expanded into the massive territorial conglomerate that had been put together by the Přemyslids, Charles's father John, and Charles himself: it included—along with Bohemia and Moravia—Silesia, Lusatia, Brandenburg, and a number of other territories.

Such non-national dynastic conglomerates were common late medieval phenomena, but they cannot be grasped, conceptually, by categories drawn from the post-medieval world of national states. Charles's apt concept, "The Lands of the Bohemian Crown," was a politico-legal invention that synthesized "Western" ideas of the crown and Slavonic notions of a historical tradition going back to the Great Moravian Empire. All of Charles's work to exalt Bohemia and Prague, religiously, culturally, and politically, was undertaken in the service of this program. In the end he himself began to lose his grip as a result of intractable realities and his own physical decline; his eldest son and successor, Wenceslas IV, turned out badly, and the more competent Sigismund failed to produce a son. (He did however have a daughter, who carried the Luxemburg legacy to the Habsburgs, who made it work until 1918.)

All in all a stimulating, extremely useful book—but with no index, no bibliography, and no map.

HOWARD KAMINSKY

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THOMAS LINDKVIST. *Landborna i Norden under äldre medeltid* [The *Landbor* (Tenants) in the Nordic Countries in the Early Middle Ages]. (Studia Historica Upsaliensia, number 110.) Uppsala: Uppsala University; distributed by Almqvist and Wiksell International, Stockholm. 1979. Pp. 179. 80 KR.

When Marx analyzed the precapitalistic economy in the medieval period, he postulated a feudal mode of production closely tied to the political and juridical control that the privileged classes exercised over the peasants in the form of serfdom. Only in this generation have Marxist scholars argued that economic and not political-juridical criteria should be the prime determinants in a materialistic conception of history. Historians such as Perry Anderson, Witold Kula, and E. A. Kosminskij have pointed out that the phenomenon of large estates was essential for the feudal mode of production. At the same time, sociologists like Barry Hindess and Paul Hirst have provided theoretical models for a rigorous conceptualization of Marxist theory. The crucial problem for these authors is to explain how the nonproducers were able to appropriate the surplus from the producers, since the latter were in control of both the means of production, the land, and the production process.

The present study, a doctoral dissertation from the University of Uppsala, fits this framework. Thomas Lindkvist compares the tenant farmers in the Scandinavian countries during the early Middle Ages, here defined as the period from the early twelfth to the early fourteenth century. Since both serfdom and large estates were, with some exceptions, absent, this area offers an opportunity to seek the salient feature of the feudal mode of production. The author finds this in the feudal rent that took the form of labor, produce, or money. In two central chapters, Lindkvist provides a structural analysis of the patterns of landholding among the tenant farmers and of their juridical position. His evidence comes from letters, charters, land surveys, and, mainly, laws that he sees as normative. In spite of chronological and regional differences, he points to the common features that distinguished the Scandinavian tenant farmers from their European contemporaries. They were personally free and, in most respects, legally equal to the landowners, but they disposed of their land only for short periods of time. This did not necessarily mean that they were forced to move regularly, but that the contract with the landowners had to be renewed periodically or, in many places, annually. In other words, because of the peasants' legal position and the tenuous nature of their ties with the land, the feudal mode of production in Scandinavia did not bind them to the soil, but had the opposite effect. By increasing the obligations for each renewal of

the contract, the landowners were able to reap the benefit of a swelling agricultural productivity. In a more speculative and less successful final chapter based mainly on literary evidence, Lindkvist argues for a double origin of tenant farmers, by ascent from the sizable slave population found in the Viking period and by descent from the independent freeholders, pauperized by the simultaneous appearance of church and state. Tenant farmers have been studied in individual Nordic countries, mainly by Russian and East European scholars. Lindkvist's work is therefore useful as the first comprehensive and comparative study of this subject. The book has a good English summary.

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DONALD M. NICOL. *Church and Society in the Last Centuries of Byzantium: The Birkbeck Lectures, 1977*. New York: Cambridge University Press. 1979. Pp. 162. \$19.95.

The impressive development of Byzantine studies in the past decades makes increasing specialization and greater technicality of most publications inevitable and, indeed, desirable. But it is also good that Byzantinists of Donald M. Nicol's stature occasionally produce a discussion of major issues involving not only the Byzantine Empire of the Palaeologi, but also Eastern Christendom as a whole, its survival in the Middle East, in the Balkans, and in Russia after the fall of Byzantium, and its relations with the West. Until recently, if Byzantium received any credit from historians of the Middle Ages and the Renaissance, it was for its reputation as an upholder of Greek antiquity and its preservation of the writings and ideas of ancient authors for transmission to posterity. What Nicol shows is that Byzantine society, on the eve of its demise, was not so much concerned with preserving Hellenism, as with "its spiritual identity that was nourished by an irrational belief in the interdependence of time and eternity, a sense of belonging to a theocratic society" (p. 130).

This does not mean, however, that the Byzantine society of the fourteenth and the fifteenth centuries lacked intellectuals who read Plato and Aristotle, or that Byzantine copyists did not actually preserve ancient texts, but that these concerns belonged only to a few and were generally considered by the Church and the state as a lower priority when compared with theological orthodoxy and a religion emphasizing personal experience and spiritual continuity. The controversy that opposed the hesychast monks to representatives of intellectual humanism and that ended in 1347-1351 with the victory of the monks stands at the center of Nicol's discussion of Byzantine society, and his judgment on its results is

both categorical and balanced: the "outer wisdom" of Greek antiquity was not forbidden or suppressed (it still found eminent adherents like Gemisthos Pletho and Bessarion), but "the growing influence and authority of the Orthodox Church as the guardian of the true Byzantine conscience meant that Hellenic learning was relegated to a secondary place" (p. 65).

Does this mean that old Gibbon was right in characterizing Eastern Rome as nothing but a despicable hotbed of "barbarism" and "religion"? This, clearly, is a question of taste and conviction, which Nicol does not want to answer directly. Actually, he remains sympathetic to Byzantine Christian civilization for what it really was and avoids foreign criteria of judgment.

One of the best living specialists in late Byzantium, Nicol is both brilliant in his presentation and accurate in his references. His book can be used as the most reliable and complete source on late Byzantine intellectual history. It also contains useful lists of Byzantine emperors and patriarchs of Constantinople between 1261 and 1453, with exact chronological data for each, and an excellent bibliography. Not only Byzantinists but also Western medievalists and historians of Eastern Europe will find this book to be an accurate and intellectually stimulating presentation of Byzantium's "last will" to posterity.

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MODERN EUROPE

CATHARINA LIS and HUGO SOLY. *Poverty and Capitalism in Pre-Industrial Europe*. Translated by JAMES COONAN. (Pre-Industrial Europe, 1350-1850, number 1.) Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. xvi, 267. \$26.25.

Catharina Lis and Hugo Soly argue that poverty was a structural feature throughout the *ancien régime*: the extent and character of poverty in economic backwaters seems similar to that in areas of economic boom, in periods of population stagnation to that of periods of growth. Without completely denying the altruism of the rich and powerful, they also argue that the primary aim of preindustrial poor relief was the control of public order and of the labor supply. Both of these premises are, by now, accepted historiographic canon. The real contribution of this syncretical survey is to show their constant and interdependent relationship.

Mediterranean and East European phenomena are discussed when appropriate, but much of the book is, in fact, an extended comparison of French

and English developments with a healthy grounding of examples drawn from the authors' native lowlands. The book consists of five chapters, ranging chronologically from "Feudalism, Poverty, and Charity, c. 1000–c. 1350" to "Economic Growth, Pauperization, and the Regulation of the Labour Market, c. 1750–c. 1850." The "long sixteenth century," animated by the triumph of commercial capitalism, and the period 1750–1850, by the growth of industrial capitalism, receive special attention. Each chapter follows a three-part structure: the causes of socioeconomic inequality among peasants and urban craftsmen; the living and working conditions of the poor; and finally the responses of political and economic elites to these threatening poor.

The authors persuasively argue that "‘polarization’ perhaps better than any other word characterizes the social evolution of early modern Europe" (p. 71). This polarization is reflected in the physical development of cities, the separation of high and low culture, and, in the mid-sixteenth century, a poor relief renaissance in the policies of some sixty European towns and cities. They also provide an excellent survey of the "Great Confinement" of the seventeenth century, seeing it not only as a French phenomenon but generalized throughout the rest of Europe as well.

The authors conclude that the "trinity of charity-control-labour regulation" (p. 222) is the inescapable reality of private preindustrial poor relief. As the effects of industrial capitalism increased, so did a whole series of paternalistic programs—private and public—further heighten the polarization characteristic of the earlier period.

This well-written work includes helpful charts and maps, complete notes, and critical bibliographies for each chapter. It offers a synthesis that, unlike previous excellent research on poor relief and charity (Gutton, Mollat, and others), also thoroughly relates poverty to the total economic development of preindustrial Europe. As the first in this "Pre-Industrial Europe" series, it sets a standard for those that follow.

HOWARD M. SOLOMON
Tufts University

JEAN-LOUIS FLANDRIN. *Families in Former Times: Kinship, Household and Sexuality*. Translated by RICHARD SOUTHERN. (Themes in the Social Sciences.) New York: Cambridge University Press. 1979. Pp. xi, 265. Cloth \$32.50, paper \$8.95.

The nearly simultaneous publication of Jean-Louis Flandrin's opus and those of Stone, Shorter, Pillorget, Donzelot, and Foucault established family structure, familial sentiment, and sexuality as major themes of eighteenth-century historiography. In the

first of two loosely linked parts, Flandrin lays important groundwork for the field by disentangling the multiple definitions of family, the several systems of reckoning kinship, and the regional varieties of household structure and inheritance in early modern France. Drawing widely on personal papers, customary codes, and secondary case studies, Flandrin rescues the profusion of extended family types, especially characteristic of southern France, from the leveling tendencies of Laslett's median of means and from the myopia of his own colleagues in the Paris Basin.

The second part is more idiosyncratic. From confessional literature Flandrin concludes that the values and behavior of the modern family issued from a complex dialectic set in motion by an internal contradiction in the post-Tridentine Church: the simultaneous valorization of family life and commitment to a celibate clergy. The Church's internal contradiction, according to Flandrin, had murderous consequences: sexual repression, the greatest infant mortality in Europe, and the inability of the French to adjust population size to available resources as the English did. Eventually French elites, first in Europe to adopt the sentimental family and birth control because they were the most acutely repressed, acted to realize half the Church's message—the ideal of family life—by rejecting its limitations on sexuality.

The book is as testy as it is versatile. It juxtaposes French family reconstitution to English census demography only to probe the weaknesses of each. It subtly champions southern communal families over those infected by the "neurotic individualism" of the north. Flandrin contends that the Sin of Onan was proliferating, first in extramarital and then in marital relations. He makes the case that women initiated contraception, able to do so because the Church's ethic of marital reciprocity increased female autonomy.

Flandrin's brand of flamboyant conjecture makes family history something like Montgolfier's balloon: propelled by its own dynamism, the course of its flight is little affected by the contours of the landscape on which others plod, and where on that terrain it might eventually touch down is anyone's guess. But, after all, the landing—the anchoring of family history to political and institutional history—is crucial to the flight's success. Although Flandrin's preface justifies the historian's concern with the private past on the grounds that domestic life was (and is) intertwined with public conflict, he fails to relate his narrative to public developments "in former times": to tax policies, legal changes, power shifts, economic changes, all of which contributed to the transformation of families that is his theme.

Nor is the book the synthesis of recent studies for

the general reader it claims to be. Highly technical, it is rendered nearly unintelligible by a careless presentation. This makes the book something like the Sin of Onan itself: a pleasant stimulation and potentially fertile but so scattershot that much of its spirit falls fallow.

CAROLYN C. LOUGEE
Stanford University

ANDRÉ CORVISIER. *Armies and Societies in Europe, 1494-1789*. Translated by ABIGAIL T. SIDDALL. Bloomington: Indiana University Press. 1979. Pp. ix, 209. \$15.00.

The thesis of André Corvisier's masterly book is that the "military mentality" was more pervasive in early modern Europe than many twentieth-century historians have assumed. He examines militarization under three headings: the army's relations to the nation, the army as an arm of an increasingly centralized state, and the army as a society with its own system of recruiting, promotion, ethics, and so forth. Much of Corvisier's previous work was based on his statistical examination of existing military records. In this one he uses his mastery of those records to examine the army's changing place in a changing society.

This book has already been recognized in France as a modern military-historical classic. There is hardly a topic upon which it does not have something interesting to say. It is least clear on "militarization" or the "military permeation of society" as part of the process of modernization. This is not surprising. Until "feudal" and "modern" and active and passive citizens' attendant military rights and duties have been more clearly defined, it will be hard to assess the novelty of the army's "new" position in society.

Corvisier's sections on the growth of military administration and military nobility in the service of the state are particularly interesting because of his comparative approach. He accepts the traditional Western view of Prussia as the exemplar of the military state and sees East European societies becoming more military as they followed the lead of the West's new monarchies. By 1789 Western Europe remained an area of "military social groups within but distinct from society as a whole." This view carries over to the very dubious conclusion that the militarization of society "set off by the French Revolution" was interrupted in the early nineteenth century and that the "nearly omnipresent militarization . . . of 1914 came about only with the adoption . . . of universal military service by societies that were no longer military in nature." The peacetime nations in arms of 1914 were thus militarized but not military, although they were soon to devote all

of their efforts to a Napoleonic goal of total military victory that left most of them in ruins in 1945.

This caveat suggests that contemporary historians may still be trying to define military, militarism, and militarization. It should not keep anyone from reading and pondering this splendid, well-translated work. As the title indicates, the naval side of military power is not dealt with, even for Great Britain, the United Provinces, and Portugal, which is not considered at all.

THEODORE ROPP
Duke University

MICHAEL BALFOUR. *Propaganda in War, 1939-1945: Organisations, Policies and Publics in Britain and Germany*. Boston: Routledge and Kegan Paul. 1979. Pp. xvii, 520. \$37.50.

The standard German nationalist line in the 1920s was that Allied, especially British, propaganda had been in large part responsible for the outcome of the Great War. Hence a book comparing British and German propaganda in the Second World War immediately whets the appetite, particularly one from the pen of a distinguished British civil servant and historian of Germany who spent the years between 1939 and 1945 involved in the "war of words." In fact no one could be more qualified for this venture than Michael Balfour.

The subject matter is fraught with pitfalls. On the one hand there are the musty volumes of the *Völkische Beobachter* from whose odors many a well-intentioned researcher has been known to expire. On the other hand there are sufficient succulent morsels to tempt the weak-willed to try and join the ranks of the best sellers. Balfour has come through the dangers splendidly. His sources are a cross section of government materials, intelligence reports, and published documents, and his choice of topics for discussion is always eminently sober. A ripe tale of heroism like the raid on St. Nazaire in March 1942 is dispensed with in a chapter of three pages; and a titillating rumor such as the scare about the Germans dropping rattlesnakes on Britain from airplanes receives less than a sentence.

In the first third of the book Balfour outlines the organization of propaganda in the two states, demonstrating particularly effectively the degree of confusion and the intensity of rivalries in the Nazi propaganda apparatus. In the second part, the bulk of the book, he presents the responses of the British and German governments to certain specific events and issues during the war. The German side receives a fuller treatment, ostensibly because of the availability of primary source material. Brendan Bracken did not keep a diary; Joseph Goebbels did. As the reverses set in after 1942 Goebbels finally

achieved greater control of the propaganda effort and, much impressed by the attitude of Churchill after Dunkirk, urged a "propaganda of the truth." This did not, however, in the end alter the general impression of rashness and inconsistency left by German propaganda, in contrast to the restraint and basic continuity of British pronouncements.

How did propaganda affect the course of the war? The answer Balfour gives is, of necessity, ambiguous. He recognizes that propaganda is bound to reflect the nature of a society and its government, and hence he is inclined to say that propaganda did not affect the actual military outcome of the war. Nevertheless he has assembled plenty of evidence that it was not altogether ineffective either. The problem is that one cannot with any kind of accuracy assess the effect: the sources for such assessment are slim and not very reliable. In sum, this book is an excellent history of the organization and implementation of propaganda in the Second World War, and for the rest Sisyphus would be proud, because the next time the stone should roll much more easily.

MODRIS EKSTEINS
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PENRY WILLIAMS. *The Tudor Regime*. New York: Clarendon Press of Oxford University Press. 1979. Pp. xii, 486. \$43.00.

In this book Penry Williams, a fellow of New College, Oxford, and specialist in the history of Wales in the sixteenth century, has set out to describe the ways in which the Tudor government actually worked. This is constitutional history but written with an emphasis on men rather than institutions. Williams is especially concerned to relate the central government to the localities, to discuss the actual operation of policy and legislation, and to evaluate the effectiveness of Tudor administration.

These are worthwhile goals. Most traditional studies of Tudor government have concentrated on the monarchy, Parliament, and the institutions of central government without paying much attention to the question of local enforcement. More recently, as local history has gained in academic respectability, a number of studies of specific cities, counties, and regions have appeared, but there has been little attempt to integrate the parts and the whole. The matter is especially important because many historians have argued that the Tudor regime was feeble and ineffective and that Elizabeth in particular failed to deal with urgent issues, thus bequeathing a welter of unsolved problems to the Stuarts. Williams acknowledges that he shared these views when he began to write, but he now believes the adverse judgment to be mistaken. The Tudor govern-

ment, he argues, had limited objectives, and it succeeded quite decently in achieving stability, controlling violence, preventing famine, and encouraging trade. All of this was accomplished with a remarkably small administrative force: there was one royal officer for each four thousand inhabitants, while the bureaucrat-ridden French had one for each four hundred.

Williams's work is marked by its tone of moderation and common sense. In the ongoing debate about Tudor despotism, for instance, he occupies a middle ground between Hurstfield's gloom and Elton's whitewash. A short section on Marian religion is valuable in suggesting the positive activities of an administration that is usually remembered only for its exiles and executions, and a brief chapter on rebellions stimulates thought by classifying rebellions into types, showing that protests cannot be related directly to bad harvests and arguing that insurrections actually strengthened the central government because they provided grounds for the punishment of government enemies.

If the book has a problem, it lies in the question of its intended audience. Most of the material will be familiar to specialists, since this work is based on published studies rather than original archival research, yet Williams presupposes too much knowledge and too serious interest for the volume to suit novices. Specific errors are few. The statute of wills did not actually repeal the statute of uses (p. 349), although it did modify its operation significantly. It is not really "surprising" (p. 426) that the Marquis of Winchester seldom attended the Elizabethan Privy Council, since he was a septuagenarian and no longer robust by the time of the queen's accession. The author of Sir Thomas Smith's biography is Mary, not Margaret, Dewar (p. 297). There is no bibliography, but footnotes are particularly full and demonstrate an extraordinarily complete use of recent monographs, articles, and dissertations. In sum, the book provides a useful, thought-provoking synthesis.

STANFORD E. LEHMBERG
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RALPH HOULBROOKE. *Church Courts and the People During the English Reformation, 1520-1570*. (Oxford Historical Monographs.) New York: Oxford University Press. 1979. Pp. viii, 304. \$29.00.

In recent years the sixteenth-century ecclesiastical courts have received well-deserved attention: proceedings have been edited (E. M. Elvey, *The Courts of the Archdeaconry of Buckingham* [1975] and M. Bowker, *An Episcopal Court Book for the Diocese of Lincoln* [1967]); the courts' part in maintaining the faith

and correcting the failings of the laity and clergy has been examined (R. A. Marchant, *The Puritans and the Church Courts* [1960] and *The Church under the Law* [1969]); and some progress has been made in assessing their popularity and utility (S. Lander, "The Church Courts and the Reformation" in *Continuity and Change*, ed. R. O'Day and F. Heal [1976] and M. Bowker, "The Commons' Supplication," in *Transactions of the Royal Historical Society*, 21 [1971]). Many questions remain, not the least of which is how the very institutions, which were so vigorously attacked at the outset of the Henrician Reformation in the *Commons' Supplication Against the Ordinaries*, survived the century with the same powers (if not the same influence) and with the same law, although the study of that law had been prohibited in 1535.

It is against this background that Ralph Houlbrooke's book must be welcomed. He has studied two dioceses, Norwich and Winchester. He shows how both instance and correction procedure were conducted (as did B. L. Woodcock in *Medieval Ecclesiastical Courts* [1952]), and he indicates a decline in both types of business, at least in Norwich. But his conclusion that "lasting damage" was done to the courts in the 1530s (p. 38) is questionable in view of his statistics on the increase in cases before them. What changed was not the use but the status of the courts within society: ecclesiastical censure ceased to carry such opprobrium, and alternatives had to be found—and frequently were found—to the pre-Reformation censures of excommunication and suspension *ab ingressu*.

Houlbrooke's analysis of matrimonial causes adds little to R. H. Helmholz's magisterial work, *Marriage Litigation in Medieval England* (1974), beyond showing that the "church's pressure" in favor of the public solemnization of matrimony succeeded in curtailing the number of matrimonial cases before the courts. Similarly, the increasing use of the courts for tithe cases is well attested as is their comparative efficiency in testamentary matters. In matters of clerical discipline Houlbrooke can add little to the work of others, particularly in determining whether the courts faced a majority or minority protest against reform (pp. 256–60).

Houlbrooke's statistical tables are invaluable and will provide useful material for comparison. Much of his narrative follows familiar paths, and it is regrettable that he quotes so little from his sources. Repetitive though much of the ecclesiastical court material undoubtedly is, it is punctuated by vivid depositions that bring village life in Tudor England right into the twentieth century. This he suggests in the story of the midwives who were determined to find out the father of an illegitimate child and "examyned (the mother) in moste labor and travaylle" (p. 77); childbirth was hazardous enough anyway, but in the presence of fourteen allegedly honest and

inquisitorial women, it made the rack a positive bed of ease!

This is a workmanlike book that brings together much material. It has come out too late to be pioneering, and one suspects that in his efforts to cut it, Houlbrooke lost much of the distinctive flavor of his evidence.

MARGARET BOWKER
University of Lancaster

ROBERT ASHTON. *The City and the Court, 1603–1643*. New York: Cambridge University Press. 1979. Pp. xiv, 247. \$24.50.

"The City," like "Wall Street," may be used to indicate a way of life, rather than a geographical location, and it is so used in this book. This book is not in the first instance a study of urban government, nor of the majority of the population of the City of London: it is a study of the world Robert Ashton has now made his own, that of business and politics under the early Stuarts. Its most significant contributions are on the relations of the crown with prominent members of the business community, many of whom also happened to be aldermen of the City of London.

For nearly twenty years, our view of the City of London, and of its leading citizens, has been dominated by the work of Valerie Pearl, whose conclusions are now significantly modified but not superseded. She had stressed the community of interest between the crown and leading members of the business community, those who could be regarded as a "concessionary interest." In terms of the debates conducted in 1961, this was a discovery significant enough to deserve the stress it received. Yet a "concessionary interest," by its very definition, can be fragmented by the making of too many divergent concessions. Ashton has now changed the emphasis by stressing the extent to which, under Charles I but not under James I, the crown dissipated this fund of support, thereby incidentally lessening the tension between privileged companies and the House of Commons. The Commons' vote of thanks to the Merchant Adventurers in December 1641 may serve for an epitome of this change.

Why did this change take place? Ashton is inclined to blame Charles I for his "prodigality" in dissipating business support. Yet this is not a sufficient explanation. Charles I was doubtless a fool, but if it were unnecessary to understand fools, we would all write very short books. Were there any pressures leading Charles to behave as he did? The need for money, and the related need for patronage, led to many difficulties, particularly with the East India Company. Charles did not create these needs, however he may have mishandled them. An ex-

planation of the troubles in the French trade in 1626 should mention Richelieu, as well as the duke of Buckingham. The use of newly incorporated companies to enforce standards of manufacturing, in the cases of the soapboilers and of the bricklayers and tilers, owes something to the lack of a satisfactory method of enforcement, as well as to the folly of the crown. Did the crown possess a satisfactory method for the government of commerce, or was "the concessionary interest" always likely to break up into concessionary interests? This is a valuable book, and it would be nice to see Ashton pursue some of its themes a little further.

CONRAD RUSSELL
Yale University

STEPHEN D. WHITE. *Sir Edward Coke and "The Grievances of the Commonwealth," 1621-1628*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1979. Pp. xv, 327. \$20.00.

Sir Edward Coke's fame rests not only on his monumental contribution to the development of the common law but also on the important role he played in English parliamentary history, especially in the 1620s. Because of his extensive experience in government, his broad knowledge, and his skill as a speaker, Coke assumed a prominent and influential position in the parliaments of 1621, 1624, 1625, and 1628. Stephen D. White's book explores in great detail this later phase of Coke's parliamentary career. Exploiting the large number of diaries and other parliamentary accounts that have survived from this period, White presents a generally sympathetic treatment of Coke that emphasizes his commitment to reform and denies that he acted as a spokesman for any particular region, interest group, or social class. Even more important, he demonstrates that Coke's political outlook changed significantly during the decade.

The first section of this persuasively argued study deals with Coke's activities in the parliaments of 1621 and 1624. In these assemblies Coke sought mainly to remedy specific grievances, which White treats under the general headings of law, trade, and the privileges and powers of Parliament. The discussion of law is especially valuable in determining exactly what legal changes Coke, who is often depicted as an inflexible legal conservative, desired to bring about. A chapter on economic grievances analyzes Coke's support for the principle of "free trade," which for him was by no means incompatible with governmental regulation of the economy. As far as Parliament itself was concerned, Coke helped to revive the judicial powers of that institution without causing a confrontation with the king. A clash did occur over the issue of free speech late in 1621, when Coke not only supported but

also influenced the wording of the Protestation. In 1624, however, Coke gave ample evidence that he still regarded Parliament as a means of eliminating particular abuses, not as "a forum for the debate of constitutional issues" (p. 179).

In the second section White argues that Coke's concern for remedying specific grievances was transformed during the parliaments of 1625 and 1628 into a more general criticism of the royal court. He traces the emergence of Coke's increasingly ideological approach to political problems, showing how his changed perception of the sources of the country's maladies led him to formulate a more abstract critique of royal power and to insist upon a recognition of fundamental law in the Petition of Right. This shift in Coke's views, the reasons for which the book does not fully explore, resulted in the virtual abandonment of his earlier efforts at reform but made possible the creation of the "rhetorical and constitutional framework" for later attacks on royal government.

White's views regarding the development and the significance of constitutional conflict in the parliaments of the 1620s set him at odds with those revisionists who have stressed the persistence of consensus politics before the Civil War and have minimized the connections between the parliaments of the 1620s and the events of the 1640s. This carefully reasoned study does not by any means, however, simply endorse the conventional interpretation that the revisionists have rejected. White refuses, for example, to identify Coke as the leader of some sort of "opposition party" in the last two parliaments of James I's reign, and at no point does he try to force Coke into a radical mold that clearly he will not fit. But he does give him credit for articulating a set of powerful and durable ideas that eventually served revolutionary purposes. By studying the formulation of these ideas within their proper political context, White has not only shed much valuable light on the operation of Coke's mind but has also deepened our understanding of the long-term causes of the English revolution.

BRIAN P. LEVACK
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MARK A. KISHLANSKY. *The Rise of the New Model Army*. New York: Cambridge University Press. 1979. Pp. xv, 377. \$17.95.

The thrust of Mark A. Kishlansky's argument is unmistakably revisionist. He denies the traditional beliefs that the New Model Army was radical both from and in its conception and that the House of Commons was divided early in the Civil War into conflicting ideological groups or parties. Polariza-

tion took place much later than we have thought: in the Commons, only with the end of the Civil War; in the army, only in the late spring of 1647. Until then, unanimity and the traditional forms were the chief goals of most men.

Kishlansky's estimate of the concern for the preservation of unity and due forms is an important corrective to the tendency of historians to think loosely of opposition and conflict as an integral part of politics. Instead, he shows that divisions and votes were seen as improper. This insight leads to a major modification to accounts of the political career of the New Model and to the suggestion that the crucial turning point was the Commons' attack in March 1647 on the army's right to petition, at a time when the tactics of the Denzil Holles clique in the Derby House Committee were themselves violations of parliamentary norms. The denial of this right added a dangerous new element to the discontents of the soldiery, which until then had been purely professional.

Predictably, analysis of procedure does not always make easy reading, and matters are not helped by occasionally poor proofreading, dangling clauses, an annoying use of jargon, and some very Teutonic compound nouns. More frustrating is the arrangement of the work, with chapters divided into sections on events in the Lords, in the Commons, and in the army. The separate focus on the Lords is novel and highly suggestive, but it is unfortunate that the method involves repetition and sometimes even contradiction. Thus, while Kishlansky argues at length that the hard-pressed parliament under the pragmatic Holles strove in the spring of 1647 to meet the army's material grievances, when he comes to discuss the army's position he reverts to an older view of a hostile parliamentary leadership and contends that it quickly voted concessions in mid-May, once disbandment had been decided, "in a parody" of its earlier delays (p. 218).

As that lapse suggests, Kishlansky is not always able to sustain his revisionism. On page 190 he maintains that the abortive soldiers' petition of March, so critical to his argument, was purely professional; yet on page 201 he suggests that in the early stages there may have been demands for equity and justice at law and complaints against malignants in power, which puts it a little closer to the famous March petition of the Levellers. There are further contradictions, as on the origins of the Manchester-Cromwell quarrel, the significance of frequent divisions, or the extraparlimentary contacts of the "presbyterian" leadership in early 1646. Equally damaging is the basing of his account of "normal" prewar parliamentary practice on pious procedural guides, which results in an alarmingly skewed comparison.

Perhaps a more serious obstacle to persuasion is the lack of adequate reference to the work of other historians. His central claim that the New Model Army was not at first bent on seizing power for radical ends should in justice have contained more reference to the recent work of John Morrill and Murray Tolmie. Those who have identified war, peace, and middle groups, or presbyterians and independents, in parliament cannot be refuted with the mere assertion that the concern for consensus prevailed. They need to be confronted. Consequently, we may wonder, in view of the very striking pattern of resignations of "moderate and presbyterian officers" in the summer of 1647—one field commander per regiment, which Kishlansky finds hard to explain (pp. 182, 219–21)—whether late-1644 politics was partisan after all. For what was the squabble in Manchester's command about? Kishlansky's argument that the perversion of parliamentary procedure undermined institutional legitimacy is extremely important, as is his suggestion about the consistency of the army's later politics. Yet too often he sees procedure as the first cause in political change, rather than as a means. It may be true that Holles's methods were more objectionable than his goals, but when his ally William Strode was reported to have declared early in 1647, "We will destroy them all for Sir Thomas Fairfax will be deceived," then perhaps the army was right to smell a conspiracy.

DEREK HIRST
Washington University

AMOS C. MILLER. *Sir Richard Grenville of the Civil War*. London: Phillimore, with Rowman and Littlefield, Totowa, N.J. 1979. Pp. 215. \$15.00.

The malevolence of Sir Richard Grenville is a recurring theme in Clarendon's *History of the Rebellion*, and there can be little doubt that the differences between the two that emerged during the Civil War induced a prejudice that colored Clarendon's account. Grenville was fiercely but unconvincingly defended by Lord Lansdowne (1732), but a second kinsman, Roger Granville, wrote a more balanced account (1908). The present biography, which makes good use of manuscript sources, goes far beyond either of these and is undoubtedly definitive.

Grenville came to public attention as a result of his marital troubles. Quarrels over his wife's property led to complex litigation and indirectly to Grenville's slandering her protector, the Earl of Suffolk, with the result that he incurred a harsh sentence in the Star Chamber. Grenville's actions during this phase of his life clearly exemplified both avarice and vindictiveness. Miller's handling of the subject is useful not only in relation to Grenville but

also as illustrating the perils of property litigation at the time.

Grenville fought under Ormonde in Ireland in 1642-43, where his reputation for ferocity and cruelty against both rebels and civilians was noteworthy even in that bloodthirsty arena. Four chapters treat Grenville's part in the western campaigns in 1644-45, culminating in his imprisonment by Prince Charles for refusing to serve under Hopton. This part of his career was most critically related by Clarendon. Miller's account is first-rate. His descriptions of military operations are clear and his analysis and interpretation very convincing. Grenville is shown to have had qualities of leadership, and his soldiers, especially the Cornish, were devoted to him. At the same time he was guilty of the utmost tyranny over the civilian populace, especially the local authorities, and he was very insubordinate and occasionally negligent of orders. Miller justly observes that part of the trouble between Grenville and other commanders arose from faulty royalist administration and from the issuance of conflicting commissions. Here and elsewhere he provides a corrective to some of Clarendon's statements. On the whole, however, Clarendon's account stands up very well, and his general verdict on Grenville still seems valid.

In exile from 1646 until his death in 1659, Grenville nursed his grudge against his enemies at court and in 1653 informed the king that Hyde (the future Clarendon) was Cromwell's spy. In his account of these absurd charges and in his concluding summary, Miller presents a very balanced judgment on Grenville. This book constitutes a welcome addition to the literature of the period.

P. H. HARDACRE
Vanderbilt University

WILLIAM M. LAMONT. *Richard Baxter and the Millennium: Protestant Imperialism and the English Revolution*. Totowa, N.J.: Rowman and Littlefield or Croom Helm, London. 1979. Pp. 342. \$28.50.

This volume continues the studies of seventeenth-century politics and religion that William M. Lamont began with *Marginal Prynne* (1953) and *Godly Rule* (1969). In the latter book, Lamont wrote that Baxter was never much interested in millennialism and that millennial thought died out after 1660. He has now changed his mind about that: he reads Baxter's *Holy Commonwealth* as a "millennial document" (p. 24) and claims that Baxter based his ideas of Church and state on his interpretation of the Apocalypse. Following John Foxe, Baxter thought the millennium described in *Revelation* was the happy period of the Christian empire under Constantine and his successors. From this, several important conclusions followed: the papacy of this

period could not be the antichrist, a statement that shocked and confused Baxter's contemporaries; Commonwealth radicals who looked for the millennium in the future were dangerously deluded; once the papacy became totally corrupt, the correct policy for faithful Christians was to support reformed national churches under Christian rulers like Queen Elizabeth. Charles I was such a ruler until he became the dupe and pawn of popery. Richard Cromwell could have become such a ruler but was brought down by radicals who were probably the pawns of Jesuits. Baxter hoped for a time that Charles II might be such a king. James was a problem for which Baxter earnestly sought the solution by rereading *Revelation*. Then William and Mary gave promise that the modest New Jerusalem that Baxter found in the Apocalypse might appear once more on earth.

Lamont draws startling and original conclusions from his study of Baxter's books and particularly of the hitherto unused manuscripts that record Baxter's study of *Revelation* during his imprisonment in 1685-86. The enemy for Baxter was Catholicism, whose spies, undercover agents, and fellow travelers he discovered everywhere—he thought Harrington's *Oceana* was a popish treatise. Constitutional issues, as Conrad Russell has maintained, were of small importance: Baxter, "like many Protestant Englishmen . . . was more concerned about Popery than the Petition of Right" (p. 25). Pinning his hopes on national churches under Christian rulers, Baxter was bound to oppose separatism in religion and disobedience in politics. He was a conservative who, far from heralding the "spirit of capitalism," wanted powerful rulers who would enforce social justice. "Baxter was nearer to the Welfare State than he was to the acquisitive society" (p. 316).

Lamont's study has the great virtue of putting Baxter into context and making intelligible his paranoia about popery, his interest in witchcraft, and his shifting opinions in religion and politics. It nonetheless raises a few questions. Lamont stresses, for example, that the Apocalypse was the source of Baxter's beliefs, but he does not cite chapter and verse. Where in *Revelation* did Baxter find anything about national churches under Christian rulers? Is the Apocalypse the source of Baxter's ideas or is it the source of the metaphors and symbols in which he clothed his hopes and proposals for reform? How could it be that Winstanley, for example, drew such contrary conclusions from reading the same Apocalypse?

RICHARD SCHLATTER
Rutgers University

LIONEL K. J. GLASSEY. *Politics and the Appointment of Justices of the Peace, 1675-1720*. (Oxford Historical

Monographs.) New York: Oxford University Press. 1979. Pp. viii, 334. \$29.00.

Lionel K. J. Glassey has chosen to investigate the political use of the royal prerogative to appoint and dismiss justices of the peace. He begins his story with the earl of Danby, who for partisan reasons purged three county benches. Upon Danby's fall, Shaftesbury and his friends sought to build up a country interest in the counties, but Charles II personally blocked their efforts and finally dismissed them from the council. As a result it was the court that remodeled the county benches between 1680 and 1685. By 1685 the Tories controlled the local magistracy, but these Tory squires refused to serve James II's purposes. In 1687 James therefore dismissed 245 justices and named 569 new ones, most of them Catholic. In 1688 he brought Dissenters as well as Catholics into the commissions of the peace. By 1688 he had purged three-quarters of the justices sitting in 1685. As Glassey rightly observes, these massive changes played a significant role in provoking the Glorious Revolution.

In the years following the revolution William III's trimming policies caused the nomination of justices of the peace to fall back into the hands of lords-lieutenants, *custodes rotulorum*, and local magnates. But the turn toward the Whigs from 1693 to 1696 saw political partisanship once again shape the commissions of the peace. Lord Somers, tentatively at first, then more systematically, added Whigs and removed the more extreme Tories. When the Tories gained control of the Great Seal in 1700, they reversed these changes. By 1704 they had created a Tory ascendancy in the countryside, an ascendancy that the Whigs undermined between 1705 and 1710. During these years Lord Cowper removed 195 justices and named 1,044 new ones, creating a Whig preponderance on the county benches. This preponderance was short-lived. Between 1710 and 1714 the Tories removed 405 justices and named 1,725 new ones, mostly Tories. The accession of George I caused the pendulum to swing back to the Whigs, who by 1720 had removed a high proportion of those Tory squires added between 1710 and 1714 and had added enough new justices to give the Whigs a majority at Quarter Sessions.

This bald summary of Glassey's argument gives little idea of its richness, complexity, subtlety, and caution nor of the vast and careful research that supports it. Glassey sees, for example, that most chancellors sought a preponderance, not a monopoly, for their party on the county benches and that they were often dependent on local advice for candidates. He also sees that manipulating the commissions of the peace, despite what many historians have written, had little effect on the general elections fought during these years. Nor did the fre-

quent oscillations in the commissions of the peace have much effect on the actual administration of the counties, since many of the new justices never acted. What the remodeling of the county benches did accomplish was to bind each party together by offering the spoils of office to its local adherents.

Glassey has not written an exciting work—the prose lacks boldness and the argument occasionally gets lost in a welter of detail. But he has written a solid work, whose scholarship is unassailable and whose conclusions are original, perceptive, judicious, and correct.

CLAYTON ROBERTS
Ohio State University

HERMANN WELLENREUTHER. *Repräsentation und Grundbesitz in England, 1730–1770*. Stuttgart: Klett-Cotta. 1979. Pp. 417. DM 98.

With this detailed work, fated to be controversial, Hermann Wellenreuther presents a revisionist model for understanding the electoral sociology of Britain in the decades before Wilkes and the American Revolution. The author examines the electoral activities of the Duke of Bedford in Tavistock, Bedford borough, Okehampton, Newport, Launceston, Camelford, and Bedfordshire, and the similar activities of Earl Gower in Newcastle-under-Lyme and Lichfield. He challenges the conclusions of J. H. Plumb, John Cannon, and W. A. Speck, contending that these scholars have fundamentally misunderstood the core meaning of the act of voting. He focuses attention on several practices of the era that have in general been neglected by historians: the manipulation of leases to increase the number of voters (for he suggests that the electorate was increasing not decreasing in size!) and the recourse to long-term rather than short-term credit arrangements as a means of fostering enduring ties between landlords and tenants. He argues that scholarly inattention to the power structures in the constituencies and to such local institutions as vestries and corporations—institutions usually empowered to create electorates—has badly skewed our understanding of the era. But behind all these new interests and his iconoclasm Wellenreuther holds fast to an old-fashioned idea as the central truth of Georgian politics: that the relationships between landed proprietors and tenants, far from being exploitative, were reciprocally beneficial—and *seen to be so*.

Wellenreuther applies his conclusions only to the nonurban constituencies. Indeed, one of the major implications of his work is to suggest that the difference between the large urban constituencies and the various smaller or rural constituencies was far greater than we have appreciated. It lay not merely

in the greater complexity of the former but also—even primarily—in the divergent consciousnesses that underlay the choosing of M.P.s in the two types of constituencies. In the large urban ones voting was a straightforward political act, controlled by the relatively sophisticated political perceptions of the electorate, heavily influenced by partisan identifications, and with debate about the candidates turning (at least ostensibly) upon public issues. (Nicholas Rogers, "Aristocratic Clientage, Trade and Independency: Popular Politics in Pre-Radical Westminster," *Past and Present*, 61 [1975]: 70–106, analyzes such a constituency for the 1740s.) But in the rural constituencies—and they constituted a large majority of the total—the choosing of M.P.s was not at all what we of the twentieth century usually take it to be. It was an action occurring in a quasi-feudal matrix, by which electors—tellingly called *Pächter/Wähler* by the author—were in fact choosing not M.P.s but patrons. Indeed, elections were not in the last analysis political acts at all. Instead they were occasions for patrons and electors to confirm their interdependence. The identity of the candidate who happened briefly to stand at the center of this occasion on polling day was of surprisingly little moment. If the patron were adequately meeting the needs of his dependents, the latter would vote for whomever he wished to present. Such considerations lead Wellenreuther to a striking conclusion: a contested election in such a constituency, rather than being a sign of political health, was actually perceived as an indication that the system was functioning imperfectly.

The case is of course unproven. Wellenreuther has damaged his own credibility by making irrelevantly erroneous statements. He has, moreover, examined the electoral activities of only one man (for the comparisons with Gower are too fragmentary to be significant), and in the absence of similar studies of others we cannot know how representative Bedford was nor how persuasively Wellenreuther's interpretive framework will give order to data from other constituencies. But the status of "not proved" should be an incentive to research, not grounds for rejecting the novel conclusions. And such research, the author reminds us, must rely upon the use of largely unquarried sources. Because agents and stewards were the key operatives for any electorally minded landed proprietor, the most important documents for understanding the politics of the era are not the widely used collections of political correspondence but the relatively neglected estate papers and records.

Bubb Dodington once remarked of Georgian politics that "service is obligation, obligation implies return." Wellenreuther argues for the validity of Dodington's judgment. But he does much more. What he gives us is no less than what he believes to

be the true structure of politics at the accession of George III. That familiar claim alone should indicate that, however severe the reservations one may harbor about Wellenreuther's views, this is a work that no student of the era can responsibly ignore.

REED BROWNING
Kenyon College

CHARLES DANIEL SMITH. *The Early Career of Lord North the Prime Minister*. Foreword by the EARL OF GUILFORD. Rutherford, N.J.: Fairleigh Dickinson University Press. 1979. Pp. 335. \$18.00.

Charles Daniel Smith has written a book that is difficult to define. It is not so much a biographical treatment of the early career of Lord North as it is an anecdotal and meandering tour of mid-eighteenth-century politics, somewhat and irregularly limited by the fortunes of its central figure. The first chapter purports to offer evidence that might bolster the old rumor that George III and North had a common paternity, but I find no evidence presented that is both pertinent and new, or at all convincing; and the chapter is permitted to wander off aimlessly into the sexual comportment of the Prince of Wales in a manner that is quite irrelevant, establishing a reputation for Frederick that is hardly in dispute.

There is disappointingly little in the remaining chapters on North's rhetorical style, which is surprising in a work by a professor of speech; and very little indeed on his management of the House of Commons, despite the claims of the preface. The book is clearly a labor of love and has occupied the attention of Smith for over twenty years. Unfortunately, the more important judgments are already to be found in recently published studies by John Cannon, John Brooke, and Peter Thomas. The shorter but fuller and more judicious biography by Thomas provides especially difficult competition, and it is questionable whether anything purely biographical needs to be done on North other than the full-dress biography that Valentine, because of his errors, failed to provide satisfactorily.

DONALD E. GINTER
Concordia University

TODD M. ENDELMAN. *The Jews of Georgian England, 1714–1830: Tradition and Change in a Liberal Society*. Philadelphia: Jewish Publication Society of America. 1979. Pp. xiv, 370. \$14.50.

It is not antisemitic to observe that Jews not infrequently overegg the pudding, and that in the laudable attempt to write their own history they have certainly done so. Todd M. Endelman's book,

however, faces little competition, so shamefully have English writers neglected its subject; a third of the space is given up to Gentiles, so that he contributes to English as well as Jewish history; and he writes modestly and well. Every reader will learn something new about the statistics, setting, or mores of Jewish life in the eighteenth century and, as the author attempts, however briefly, to compare the Jewish experience in England with that abroad, especially in Germany, and with that of other religious minorities in England, there is a context to his discussion.

This is certainly a good book, and if it leaves some sense of disappointment, the reasons are twofold. Though Endelman brandishes some very large and unusable concepts—desacralization, secularization, modernization—his theme is at bottom a simple one, that of the assimilation of the Jewish minority and the consequent threat to its religion and cohesion. This was very obvious in the Jewish upper crust, operating in a world excessively but not exclusively devoted to the worship of mammon. Sampson Gideons were indispensable to English governments, but genteel English society was loathe to accept the sons of Sampson Gideons, however wealthy, without baptism. But the problem affected every social layer of Jewish life. The author gives the impression that English society was exclusive (though depending little on anti-Jewish legislation) and that all that tempted Jews into it (he will not have any Herbergian generational hypothesis) was the desire to make more money or to receive the social adulation usually accorded to wealth. Some more precise analysis than this is now required by historians very conscious of how unassimilable the Irish minority has been (especially when compared in Scotland, for example, with the almost total assimilability of the English minority) and by social scientists lamenting the failure of English capitalism to wean the working class from its independent mores.

English society seems, relatively speaking, not to have done badly by the Jews; certainly Jewish emancipation was not followed as it was in Germany, and as Catholic Emancipation was in England, by mob violence designed to keep its beneficiaries in a position of second-class subjects. On the other hand, reasonable Jews, like reasonable Protestant dissenters, could not but be attracted by the cultural advantages that assimilation offered. What did Judaism do to enable its core concepts and practices to be harmonized with those Gentile cultural achievements that were in themselves estimable? Endelman does not say, insisting simply that there was no English Moses Mendelssohn and that Anglo-Jewry was interested in little except for making money in unintellectual ways. He gives no hint of the devices that ultimately enabled English

Jews to achieve high thinking without plain living. Jewish capacity for assimilation is as much a part of English history as English capability to assimilate is a part of Jewish history.

W. R. WARD
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F. B. SMITH. *The People's Health, 1830-1910*. (Croom Helm Social History Series.) London: Croom Helm. 1979. Pp. 436. £14.95.

This is an ambitious attempt to describe the whole course of the major diseases and their treatment in Britain over eight crucial decades. It is not a traditional history of the great medical discoveries but of the actual application of medicine to society, with all the complications produced by class assumptions and poverty. The "people" here are chiefly the working classes; provision for the upper classes is mentioned occasionally but mainly for comparison.

The arrangement of the book will cause some irritation. F. B. Smith has decided to take his patients from the cradle to the grave and to relate each disease to the age group it most affected. He begins with childbirth, but the ailments of humanity thereafter refuse to be allocated to particular ages. By the last chapter the format has broken down, and "old age" includes arbitrary sections on influenza, heart disease, quack medicines, and the Poor Law. In fact, there is little organization, for subjects like "hospitals," "eyesight," and "school health" could be arranged in any order to equal effect. As in many works of social history, a distinct chronology is also lacking; in covering such a wide range of subjects Smith leaps back and forth between decades, and it is not always easy to locate him without reference to the footnotes. Morbidity statistics impose chronology in the earlier sections, but general subjects like patent medicines are much more rambling.

Smith has another problem common to social historians. He has used mainly printed sources, and the bulk of his material is from the medical journals, especially the *Lancet*. He reproduces many local and regional statistics from these sources, but it is difficult to build up a coherent picture from the diffuse figures for measles in Coventry, smallpox in Birmingham, and so forth. It would be wrong to cavil much at this. The statistics are often from periods when national surveys were erratic and diagnostic methods variable. Smith's information is rich and interesting, but "Britain" appears neither as a unit nor as a series of distinct localities. Readers will wish to trace the numerous references to specific places, and the book demands a place index; for such a substantial work the three-page general index is grossly inadequate.

Smith does not seriously question the value of the medical journals as a source of information on treatment. Their anecdotal pattern is reflected in the book. A section on the *Lancet's* editorial policies would have helped to define the problem; in the early days of the medical journals there was little editorial discrimination between serious research and the lunatic fringe. How then can a particular method of treatment be seen as "typical?"

These defects do not detract from the interest of this book; it is a valuable contribution to a new subject and is fascinating if lugubrious reading. No student of medical history will be able to dispense with it. We trust that Smith has escaped from eighty years of medical literature without a major attack of hypochondria.

M. A. CROWTHER
University of Glasgow

EDWARD W. ELLSWORTH. *Liberators of the Female Mind: The Shirreff Sisters, Educational Reform, and the Women's Movement*. (Contributions in Women's Studies, number 7.) Westport, Conn.: Greenwood Press. 1979. Pp. xii, 345. \$25.00.

Edward W. Ellsworth's study of the Shirreff sisters is a welcome contribution to the history of education. Emily Shirreff (1814-97) and her younger sister Maria Shirreff Grey (1816-1906) helped systematize women's education in nineteenth-century England. Emily is best known for her writings. Her book *Intellectual Education and its Influence on the Character and Happiness of Women* (1858) gave an impetus to the development of liberal arts curricula in girls' schools, and her pamphlets of the 1870s helped popularize Froebel's method of early childhood education. Maria, more outgoing than her sister, gave public speeches on the need for systematic schooling and used her social contacts to gain support for the Women's Education Union, the Girls' Public Day School Company, and a training and registration society to provide qualified teachers for the company's schools, all of which she organized. Both Shirreff sisters were at the center of discussions of women's issues, as preservers, not revolutionaries. Daughters of a family with relatives among the aristocracy, they reformed middle-class women's education to meet the needs of Victorian society.

Ellsworth discusses the Shirreffs' ideas only in relation to women's education, yet they fitted a broader context. In common with reformers of men's education, the Shirreffs believed that education should take the form of rational schooling (defined by systematic learning, regular attendance, and a sequenced curriculum) and have for its content the liberal arts (including the sciences and modern languages). The Shirreffs' contribution was to apply

these ideas in theory and in practice to women's education. Until the time that they did so, education for upper- and middle-class girls was unsystematic and vocational. Their families considered that every girl's vocation was, first, to obtain a husband and then to run the household and care for her family. Hence girls were taught sufficient knowledge to be pleasant company for men and to be efficient housewives. By 1880, largely because of the work of Emily Shirreff and Maria Grey, this kind of education had been superseded by rational schooling in the liberal arts on the model of boys' schools.

Ellsworth's study gives a rich description of the Shirreffs' work, placing it within the context of the nineteenth-century women's movement and explaining the influence of their ideas. Despite its strengths, however, the book is at times frustrating. Ellsworth has a disconcerting habit of shifting perspective, like a photographer taking random close-ups of a crowd. His first reference to a woman rarely tells the reader her importance for the topic; rather he describes her relationship to the "significant" men in her life. Lastly, Ellsworth's is a whig interpretation of women's history in which, during the march toward equality, the Shirreff sisters were the liberators of the female mind.

I do not share Ellsworth's view. I suggest that the legacy of the Shirreffs was a mixed blessing to women. Emily, in her writings on early childhood, helped persuade the public that mothers were indispensable companions to their young children, thus encouraging social action against the employment of married women. Similarly, the Shirreffs' emphasis on education in the liberal arts and the need for women to enjoy learning for its own sake was used by some people to counter demands that women be trained for careers. In retrospect, the Shirreffs did as much to make women of the middle classes content with their lot as to break the bounds of existing gender roles.

JOAN N. BURSTYN
Douglass College,
Rutgers University

HOWARD L. MALCHOW. *Population Pressures: Emigration and Government in Late Nineteenth-Century Britain*. Palo Alto, Calif.: Society for the Promotion of Science and Scholarship. 1979. Pp. xi, 323. \$18.00.

This work is not a study of emigration per se but a study of the politics of state-assisted emigration during the second half of the nineteenth century in Britain. It traces the origins, development, and demise of the National Association for State Colonization, a pressure group that unsuccessfully lobbied to secure state-assisted emigration. The author also de-

scribes the association's relationship to other diverse groups that favored either state or private aid to emigrants from the 1850s, a period of class cooperation, to the 1890s. The movement reached its zenith during the economically troubled mid-eighties, as part of that decade's rejection of laissez faire in favor of collectivism, but characteristically withered away with the return of prosperity.

Howard L. Malchow admirably succeeds in identifying the reasons advanced by those favoring state-assisted emigration, that is, providing markets abroad, avoiding industrial unrest, bolstering the empire, and reducing the supply of labor, while also presenting the arguments marshaled by its opponents, that is, fear of excess labor by colonial trade unions, fear of it serving as an example of "socialist" legislation, resentment by minorities viewing state emigration as a euphemism for transportation, and wariness by union leaders and capitalists who had second thoughts about losing trade union members and skilled labor. For most of the period, both labor leaders and manufacturers, blinded by the fatuities of Cobdenism, regarded emigration as a panacea; they ignored the fact that mates and operatives expelled through the safety valve might serve not only as a market for the workshop of the world but also as competition that might deepen future trade depressions.

Both the author and his publishers have produced a fine book. Writing or reading "loser's history" can on occasion be dispiriting, and the plates from the *Illustrated London News* do much to add life to the subject. Malchow should be complimented, not only for his thorough use of a wide variety of sources but also for his excellent grasp of peripheral issues. On the debit side of the ledger, although the book sheds light on many new characters on the Victorian scene, it contains few surprises. Two exceptions are the fact that Joe Chamberlain, imperialist and friend of the workingman, opposed state-aided emigration and that at times (1878, 1885-87) the annual attrition from emigration was comparable, in numbers at least, to that of British deaths in World War I. Although Malchow does a fine job discussing the "push" of hard times, he generally slights the implications of the "pull" factor on emigration, which operated quite well in the period prior to 1869 and in the two decades preceeding World War I. This was obviously an important factor that contributed to the failure of the movement, which, in combining the characteristics of both an "interest" and "cause" lobby, theoretically should have been strong enough to achieve its aims.

NORBERT C. SOLDON
West Chester State College

ery and Functions of Government in Britain since 1780. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1979. Pp. x, 295. \$20.00.

Of the numerous contributions to the study of modern British administrative history that have appeared during the last two decades, Geoffrey K. Fry's *Growth of Government* cannot be ranked among the more distinguished. The author claims to examine ideas concerning the role of government in Britain between 1780 and the present and to consider the development of governmental functions and machinery over the same period. What is given in the volume, however, does not answer these purposes.

The central difficulty of Fry's work lies not in its conclusions—indeed a number of these make perfectly good sense. He is doubtless right to assert, for example, that Liberals, Green and Keynes in particular, rather than conservatives or socialists, have been the moving force in shaping the intellectual context within which the expansion of government has occurred. His conclusion that the growth of the state in nineteenth-century Britain did not constitute a "revolution in government" certainly merits attention. The problem is that Fry's conclusions rest on no solid analytical structure and consequently lack the force necessary to carry authority and to command respect.

The first part of Fry's book consists largely of a string of quotations from the classical economists, Green, the Fabians, Marshall, and Keynes. Admittedly, the book is a survey, and one has no right to expect detailed analysis, yet the space available could have been put to better use. Unfortunate gaps in Fry's research are also apparent. In the section on J. S. Mill, for example, no reference is made to Mill's substantial article "Centralization," the contents of which are certainly pertinent to Fry's discussion. The work of Alan MacBriar and Willard Wolfe on the Fabians seems to have been neglected in favor of a number of dated treatments (though MacBriar is listed in the bibliography). Nor does Fry always have his facts right. To cite one instance, he states that Mill served in the India Office, "where a Secretary of State and a Council had replaced the Board and the Company" (p. 120). Mill was an employee of the East India Company, and he retired in 1858 when its operation as an agent of government was terminated.

The second part, which focuses on the machinery of government, is no less disappointing. Attempting to say something about the traditional functions of the state as well as the new responsibilities for economic regulation and social provision, this section of the book is almost exclusively descriptive. The connection between the individual thinkers discussed in part 1 and the machinery described in part 2 is never established. As Fry fails to give a

sense of the historical environment inhabited by his Liberal intellectuals, so he fails to locate the institutional developments of the nineteenth and twentieth centuries within their appropriate political and economic contexts, abstracting them from the circumstances that gave them their significance. The result, one regrets to conclude, is a book of very limited value.

BRUCE L. KINZER
J. S. Mill Project,
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ADOLF M. BIRKE. *Pluralismus und Gewerkschaftsautonomie in England: Entstehungsgeschichte einer politischen Theorie*. Stuttgart: Klett-Cotta. 1978. Pp. 258. DM 58.

An excellent book, though slow to start! Adolf M. Birke's aim, far distant, is to show that the rarefied debates on pluralism and state sovereignty—from Maitland to Laski—were prompted by intensely practical, and very puzzling, developments in English law. Thus it is at ground level, in the trade unions' struggles for status, that he begins. From there, through Scots churches' battles for property, and on through a tangle of decisions judicial and legislative, he works upward. But it is a long climb, through dense thickets; by page 124 (the Osborne Case) one has half-forgotten the scholarly peaks that glisten beyond.

After that judgment in 1909, attention shifts to the theorists. But already the basic questions have emerged: can free associations (unions, friendly societies, or churches) find protection in law without losing essential liberties? Is there a middle ground between nonrecognition (with its advantages of voluntarism and autonomy) and full legal incorporation (with risk of state control)? Trade union experience had, in one century, ranged from sheer illegality, through ambiguous toleration, to near immunity. To trade unionists, the ideal remedy was a kind of no man's land where, without recognized incorporate status, statutory safeguards made them practically unanswerable before the law.

But safety for their interest—or any one group's interest—was not the only issue. There was also the issue of protecting the individual against the group: otherwise, privilege or immunity for the “free” association might imperil rights of the members within it. Thus, freedom clashed with freedom, and group freedom with public policy. Group power, in turn, might clash with state power and with ultimate state responsibility. For all these issues, the pluralist theorists cared, but most especially they cared for another value: the integrity and consistency of the law. Birke further shows how these conflicts raised the question of criteria. Should all things be decided by conformity to past law? What was the place of logic? Must both logic and law yield where

a long record of irrational growth and adaptation—irreconcilable with judges' dogma, but functional—dictated other choices? Churchill, the Webbs, Harold Laski (with Holmes intoning wisdom) tossed the questions about, in a result fascinating if inconclusive. Birke links it all to debates among pluralists in today's Germany. For compelling reasons they seek alternatives to the all-competent state.

PAUL BARTON JOHNSON
Roosevelt University

COLIN HOLMES. *Anti-Semitism in British Society, 1876–1939*. New York: Holmes and Meier. 1979. Pp. viii, 328. \$42.50.

There has never really been a “Jewish problem” in Britain, and antisemitism has never been a major force in British society or politics. Nevertheless, the immigration of Jewish refugees from Russia between 1881 and 1914 and from Germany in the 1930s stimulated a significant undercurrent of hostility to Jews. Colin Holmes provides both a microscopic examination of the extremist fringe of antisemitic groups in Britain in this period and an assessment of the wider impact of antisemitic ideas in British society. He begins with a definition of antisemitism as “hostility towards Jews as such,” but he properly notes that antisemitism very often sheltered behind euphemistic “anti-alien” agitation (“aliens” was in the pre-1914 period a code word for Jews much as “immigrants” in Britain today is understood as denoting not the large numbers of white newcomers but black and brown people—many of them now British born). Nevertheless, while clearly conscious of the hypocrisy of the supposed distinction between “anti-alien” and “anti-Jew” feeling, Holmes is perhaps a shade too kind to Balfour's government in concluding that “in its form, it is more appropriate to categorize the 1905 legislation [restricting immigration to Britain] as anti-alien rather than anti-semitic.”

Antisemitism in Britain, as the author rightly stresses, transcended class and party. There was the working-class antisemitism that produced riots in South Wales in 1911 and in east London and Leeds in 1917; there was also the more genteel clubland prejudice against the cosmopolitan friends of Edward VII and the literate scurrility of “Chesterbelloc” and Eliot. Holmes justifiably concentrates on the history of the right-wing antisemitic propagandists, a bizarre collection of cranks, fanatics, and heroes (all three at once in cases such as Sir Richard Burton). But he does not spare the left, pointing, for example, to the anti-alien immigration motions passed by the Trades Union Congress in the 1890s. Only occasionally does a defensive note intrude in the treatment of left-wing attitudes. Commenting on attacks by Communist front organizations in the

1930s on Jewish landlords, Holmes writes: "Throughout the Fascist offensive the CP never forgot that the wider interests of the working class took precedence over those of ethnic interests." Might one not more plausibly argue that the shameless gyrations of West European Communist parties in this period demonstrated rather the CP's total subordination of the interests of the working class to those of the socialist motherland?

One of the most useful passages in this book deals with the antisemitism of Sir Oswald Mosley and the British Union of Fascists. Only rarely does the author seem a little too ready to accept at face value Mosley's subsequent attempts to exculpate himself from the charge of antisemitism—as when we are informed that "Mosley was concerned to engage in reasoned argument against Jewry." The general treatment of Mosley's antisemitism is, however, notably less indulgent than that of Mosley's recent biographer, Robert Skidelsky. Holmes concludes of Mosley that "his attitudes revealed a hostility towards Jews which was expressed within an ethnocentric and conspiratorial framework." Overall, Holmes argues that there was between 1876 and 1939 a continuous tradition of antisemitism in Britain, and he proves his case effectively in this valuable study of the racist underworld of a declining empire.

BERNARD WASSERSTEIN
University of Sheffield

BERNARD WASSERSTEIN. *Britain and the Jews of Europe, 1939–1945*. New York: Clarendon Press of Oxford University Press, for the Institute of Jewish Affairs, London. 1979. Pp. viii, 389. \$17.95.

Britain occupies a special place among those nations that bore mute witness to the destruction of European Jewry. It administered the mandate in Palestine, the most logical haven for the Jewish victims of Germany's extrusion policy, and the governing principles it represented were so antithetical to those of the New Order in Germany that a humanitarian response was in order. Britain's unwillingness to make such a response by altering its wartime priorities to save precious lives and the obtuseness of its governing class reflected in the failure to recognize the special character of the Final Solution is the subject of this well-researched, superbly crafted account. It serves as a sequel to A. J. Sherman's *Island Refuge* (1973), which took the story to 1939.

What emerges from this study, based on newly available foreign and colonial office documents, is that, with the notable exception of Winston Churchill, British decision makers sought to thwart rescue possibilities, a policy that in effect made Britain an adjunct to the destruction process. The Brit-

ish acted as though Berlin's secret weapon was the strategic "dumping" of masses of Jews. The first shots fired by British combatants in the war were not aimed at German soldiers but Jewish "illegals" trying to enter Palestine. British antirefugee policy did not limit itself to a hard-nosed enforcement of the 1939 White Paper. Virtually every opportunity for mass rescue was thwarted either at the top of the decision-making process or at some middle level. A separate Jewish fighting force was ultimately so pared down that it lost all symbolic significance. Statements of retribution that might have helped in breaking through the curtain of silence surrounding the genocide were not made because it was believed that atrocity propaganda would be counterproductive. Retributive bombing of German cities, twice suggested by the Polish government-in-exile as well as the bombing of the gas chambers and crematoria were rejected as were specific offers by Rumania and Hungary to release thousands of Jews. Even the idea of sending food packages to certain camps was opposed because it flaunted the blockade on which British strategy traditionally placed high hopes. Yet throughout the war occupied Greece was fed by Britain. Taken together these policies made it appear as if British officialdom preferred the Nazis to liquidate the Jews rather than have to assume the burden that rescue entailed. That indeed was what Joseph Goebbels believed.

Bernard Wasserstein observes that the Jewish image as a powerful force that led the British to issue the Balfour Declaration in 1917 was altered considerably during the interwar period. During World War II Jews were considered one of many groups victimized by Nazi barbarity. Their "wailing" for special attention made them a strategic liability, a threat to the principal priority of winning the war as quickly as possible. The British governing establishment was too far removed from the stench of the death camps to perceive that Nazi harnessing of modern mass production techniques to produce death did indeed differentiate the Jewish crucible from other atrocities committed during the war. It could not perceive that such an act of social cannibalism required draconic reprisals or at least a recognition of its significance.

The author's conclusion regarding British leadership seems inescapable. It "experienced an imaginative failure to grasp the full meaning of decisions, when the consequences were distant, unseen, and bore no direct relation to the actor" (p. 356). That conclusion will come as no surprise to those who remember Munich or the scandals that wracked the British establishment in the postwar period.

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S. A. WALKLAND, editor. *The House of Commons in the Twentieth Century: Essays by Members of the Study of Parliament Group*. New York: Clarendon Press of Oxford University Press. 1979. Pp. vi, 649. \$55.00.

The House of Commons in the Twentieth Century, edited by S. A. Walkland, is a series of essays by members of the Study of Parliament Group. The genesis (or rationalization) for this book is a three-volume work, published in 1908, by Josef Redlich. Redlich's study of the procedure of the House of Commons is a remarkable work. The Austrian scholar realized, in a way that few English writers or, indeed, perhaps only one other foreigner, the American Lowell, did, that the British clearly had a constitution, albeit not a written one. Redlich was also important for his realization that the real power in the House of Commons, and its contribution to the constitution, was procedure—hence, the importance of Redlich's work and this effort to honor it by bringing out a series of essays that cover many of the topics covered in the earlier study.

There is much in these essays that is both interesting and important. At the same time, one can understand why it was Lowell and Redlich who were the perceptive ones. Just as de Toqueville and Bryce saw American life in some ways more perceptively than their American contemporaries, so too there is something slightly chauvinistic about this new book of essays, excellent as they sometimes are.

The essays, for instance, show all the advantages and disadvantages of the state of political science in England. The English approach to political science is still more historical than the American, although not necessarily the worse for that. The absence of theory, however, and some hesitation about the use of quantitative studies, does at times frustrate the intellect even while the absence of jargon may sooth the nerves. So too, the English are much more willing than Americans, in books of essays such as this, to rely on those who are practitioners of the art rather than on trained political scientists, or even, as they would put it, professors of government. The effect of this is to underline the slightly formalistic approach to political science. Although English political scientists have moved beyond the state of assuming that civil servants are concerned only with technical issues and never with policy issues, and judges only with law and never with policy, one at times wonders how far from that intellectual position some of the contributors to this book in fact have strayed.

There are, however, some excellent essays, combining the best of the elegant English style and mastery of history with the subtle use of the theory and quantitative approaches that dominate American political science. In that category one would have to

put Michael Rush's essay on the "Members of Parliament" and Walkland's own essay on "Government Legislation in the House of Commons." Rush produces a subtle blend of quantitative expertise and the historical tradition in his study of members of Parliament, while Walkland analyzes the specialist committees and their role in legislation in a smooth and elegant way.

Even these outstanding essays, however, underline another weakness in the book. Writing about the House of Commons and basically about procedure in the house, is at times like studying Hamlet without the prince. The current moves in the Labour Party to keep members of Parliament on a very short leash and far more subject both to local party officials and to the national executive are an integral part of comprehending the style of the typical member of Parliament. The projected changes in the specialist committees and the role of the civil service in overtly and covertly opposing their extension are essential for understanding the process of legislation in the house. Yet, the inevitable context of these issues is covered at best, glancingly. Hence, in spite of the elegance and erudition, one, at times, has a strong sense of frustration.

Perhaps the book is at its very best in Marshall's essay on privilege. Geoffrey Marshall is among the outstanding political scientists in England today, although not as well known in this country as he should be. His essay on "The House of Commons and its Privileges" is not only both comparative and perceptive but also opens up many wider notions. The House of Commons has at times been somewhat ludicrous in protecting its so-called privileges; and although there is now a more relaxed approach to parliamentary privilege, Marshall's trenchant criticisms have to be read by anyone concerned with the operation of modern government in England today.

Peter Richards adds a useful short essay on the private members bill. Nevil Johnson has a thoughtful essay on select committees, suggesting the waning of such committees as well as their relationship to royal commissions. Johnson's essay is particularly important on the statutory instrument and the Public Accounts Committee, although it is difficult not to contrast his conclusions with those of Richard Crossman in his *Diaries* (1977). Meanwhile, Philip Norton's essay on the parliamentary parties, with heavy emphasis on the work of the Whips and their offices is an interesting excursion into an almost totally neglected area. The essay, however, would be better still if there were more emphasis on the change over the last decade resulting from a more full-time membership in the House of Commons. Perhaps better integration among these various essays would also have been useful.

As suggested earlier, perhaps the weakest essays

from a scholarly point of view are those by persons who are more practitioners than scholars. Ryle's essay on "Supply and Other Financial Procedures" is exhaustive, while Borthwick's essay on questions and debates falls into the same category, as does Philip Landy's excursus on the speaker and his office. All are encyclopedic but lacking in theory and principle and sadly, on occasions, both in structure and rigorous editing.

It would be wrong, however, to dismiss this book easily. It is essential reading for anyone concerned with the House of Commons and the modern British Constitution. Its defects are perhaps those of books of essays rather than this particular volume. Each essay adds something of substance and must help us to comprehend better the procedure of the house and thus of the British Constitution.

ROBERT STEVENS
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ROY CHURCH. *Herbert Austin: The British Motor Car Industry to 1941*. (Europa Library of Business Biography.) London: Europa Publications. 1979. Pp. lii, 233. £12.00.

This important and well-researched book traces the origins and evolution of one of the U.K.'s main car makers, the influence of its founder Herbert Austin, and the effects of the company on the U.K. car industry. Although Austin's interwar activities gave little hint as to the hectic nature of the industry's postwar history, a book like this can explain much of the car industry's present structure, behavior, and performance.

Despite Herbert Austin's early experiences with Wolseley, which encouraged him to seek the ownership of a business as well as its management, this did not prevent him from falling into the common British trap of designing cars for a limited market. The results were disastrous, and the crisis of 1920-22 was surmounted only by a drastic reorganization and reassessment of company policy. Even then, the financial effects of the crisis were to last until the mid-1930s. Nevertheless, Austin's position recovered, not least because, for a time, head-on competition with Morris was avoided. Roy Church neatly summarizes these issues.

Although concentrating on model policy, Church's limited discussion on pricing policy is useful. For instance, it shows that an attempt at price rigging occurred (p. 116), but this did not prevent the appearance of a cut-price Austin (p. 128) or competitive responses (pp. 128-30). An aspect of model policy seen for its commercial acumen and rapid gestation (pp. 116-17) could also describe a company hesitating until shown the way by a competitor.

What could be a commentary on today's problems is the analysis of the car makers' criticisms of the U.K. steel industry (p. 122) and their attitude to consumer preference (p. 128). References are made to Austin's lagging styles and designs and Morris's superiority (p. 127), but matters should not be overstated: the Austin 10 h.p. engine of 1934 was an advanced design, and time was to show the greater efficiency of Austin's component production.

Notwithstanding its subtitle, this volume is not a completely rounded history of the car industry. For instance, before the mass production of steel bodies the main production technology of the major car firms was machining and metal removing, not sheet metal forming. This technology was supplied by independent bodybuilders, and to omit their role is to miss the economic significance of, say, Rootes buying about 85 percent of a car's ex-works value outside the company: any analysis of this, as on pages 135-36, is disappointingly brief. Again, as Austin made no purpose-designed commercial vehicles between 1925 and 1939, Church does not analyze this sector of the industry, although most of the major car firms were engaged in making commercial vehicles. Indeed, Adderley Park *was* closely integrated within the Morris empire (p. 106), but from 1929 and as part of Morris Commercial.

In a book of such scholarship, lapses such as wrongly cited tables (Table 8, p. 130), a poor matching of text and tables (p. 133), confusing (p. 117, no. 23) or doubtful (p. 186, no. 5) footnotes, oddly titled or omitted references may be irritating, but Church's excellent volume must be regarded as a valuable addition to the literature on the economics of the car industry.

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WALTER MAKEY. *The Church of the Covenant, 1637-1651: Revolution and Social Change in Scotland*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. vii, 216. \$27.75.

The history of Scotland in the seventeenth century has received its fair share of attention over the past three centuries. Many of the earlier works on the Covenanting movements have produced more heat than light, which is usually the case when religion flies in the face of politics. David Stevenson and Walter Makey, however, have produced scholarly works on the period that have redressed this particular historical deficiency. The former has written two books and numerous articles on the subject, and the latter has produced this interesting book that emphasizes the social change in Scotland before, during, and after the Covenanting movement.

This social perspective is the obvious strength of the work. Where it differs exactly from Stevenson's previous work is more of a mystery.

The book is divided into two parts with a brief introduction called the "Silent Revolution." The "Story of a Revolution" makes up the first and weaker of the two parts. The nonspecialist will have difficulty here because an enormous amount of the history of the period is assumed. The second part, "Anatomy of a Revolution," is well researched and contains many interesting ideas. Makey has been careful not to stretch his research material too far, and he cautiously warns the reader about the strengths and weaknesses of his statistical data. These data, the "stuff" of social historical analysis, provide the necessary material for the study of the origins of the ministers and elders.

There seems to be some confusion about a few important concepts, such as conventicles, privy kirks, and presbytery. Certainly conventicles were similar to privy kirks, but there were essential differences: privy kirks opposed an established church and elected their ministers, while conventicles recognized the established church and did not elect church officials. The presbytery was both a church court and a geographical area; it is unlikely, however, that the church was in the tax-collecting business.

During the "Silent Revolution," according to Makey, the ministers disagreed "about almost everything that was fundamental" (p. 184). On the last page he seems to contradict this: "The differences between the two parties were real enough but they were tactical rather than fundamental" (p. 185). In any case, they seem to have agreed that feudalism in Scotland was definitely on the decline by the end of the seventeenth century.

Makey's publisher is to be commended for making so much recent scholarly work available to the interested scholar and student of Scottish studies. Nevertheless, care must be taken that dissertations are carefully rewritten and that serious printing errors are eliminated. Although Makey revised his thesis for publication, expanded part of it, and divided his "A State Opposite to a State," much of it is unchanged. Nonetheless, the book will add much to our social awareness of the Covenanting movement of the seventeenth century.

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IAN WHYTE. *Agriculture and Society in Seventeenth-Century Scotland*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. ix, 301. \$35.00.

Ian Whyte has given us a valuable account of Scottish agriculture in the seventeenth century. He

questions the traditional view that agriculture in Scotland remained basically unchanged from the middle of the sixteenth century until the agricultural "revolution" of the eighteenth century. He argues instead that a considerable degree of change took place during the seventeenth century—at least in the Lowlands—with the result that crop yields improved and tenants gained increased security through longer, written leases. This modest agricultural improvement—and Whyte recognizes its slow pace—was not without setbacks, such as that brought by the political turmoil of the 1640s and 1650s and the terrible famine of the later 1690s. But on the whole, Whyte convincingly shows, the century saw moderate agricultural progress, marked by the increased use of fertilizer and lime, better crop rotations, and, in the eastern Lowlands, a shift away from subsistence to commercial agriculture.

This progress, however, did not free Scotland from a terrible subsistence crisis in the 1690s. Bad weather in 1695, 1696, and 1698 drastically reduced the harvest in many parts of the kingdom, and the ensuing famine cruelly cut back the number of small tenants and laborers, particularly along the Borders and in the Highlands. Despite the advances the century had witnessed, Scotland still remained agriculturally primitive, unable to adjust to severe climatic shocks. One wonders what the loss of life in the 1690s would have been had no agricultural improvement taken place earlier.

Throughout the seventeenth century, the smaller tenants and the agricultural laborers subsisted—when they subsisted—on a monotonous diet of oats and a hardy form of barley known as "bere." Notwithstanding the pastoral nature of the agriculture, meat or dairy products seem to have been little eaten by the poorer sorts. But Whyte points out the interesting fact that even the poorest Scots often had kale yards adjoining their cottages. Kale is rich in vitamins A and C, and it provided a certain balance to a diet otherwise too heavily weighted toward cereals.

Although Whyte's knowledge of Scottish agriculture is impressive and the book is obviously the result of painstaking research in a wide variety of estate papers and other primary sources, the sections on demography are rather superficial. Perhaps *Scottish Population History from the 17th century to the 1930s* (1977), edited by Michael Flinn—the standard work on Scottish historical demography—was not yet available when this book went to press, although Whyte cites other works published the same year. Nor does Whyte seem well grounded in studies of subsistence crises or agricultural practices in other climatically marginal areas of Europe, such as Scandinavia, Switzerland, or northwest England. Wider reading would have enabled him to place his Scottish evidence within a broader context. These

minor faults, however, in no way detract from the importance of the material he has gathered on Scottish agriculture.

The book is nicely designed but unfortunately very carelessly printed.

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BRENDAN BRADSHAW. *The Irish Constitutional Revolution of the Sixteenth Century*. New York: Cambridge University Press. 1979. Pp. 303. \$34.50.

In this major reinterpretation of Irish history during the middle Tudor period, Brendan Bradshaw finds reformation and revolution where conventional historians have seen little more than rebellions and reaction.

Bradshaw devotes nearly a third of his book to providing necessary background. He examines the medieval Irish lordship to show the origins of the problems of government that were its legacy to the sixteenth century. He also discusses the failures of Henry VII via Poynings's expedition and Henry VIII via Surrey's expedition to deal with those problems effectually. And he traces the development of a movement for political reform among the Anglo-Irish of the Pale through an examination of their political treatises.

After setting the stage, Bradshaw turns to the middle Tudor period, beginning with the reform of the Irish lordship in the era of Thomas Cromwell. The Cromwellian reform was restricted to the Anglo-Irish areas. There Cromwell ended the long-standing rule by feudal magnates and revived crown government. Crown government, however, did not mean Irish government. In accordance with Cromwell's concept of unitary sovereignty, the real government of Ireland in both church and state resided in England. As for the "disobedient" Irish, Cromwell established a permanent English garrison to restrain them in their state of insecurity while awaiting an indefinitely postponed conquest.

Cromwell's fall was followed by what Bradshaw calls "the liberal revolution." Its principal architects were the English lord deputy, Sir Anthony St. Leger, and his Anglo-Irish cohort, Sir Thomas Cusack. The liberals had two objectives. One was to assimilate the Irish by consent instead of conquest. This was to be accomplished via the process of surrender and regrant whereby the "unconstitutional" Gaelic lords were granted feudal titles and security of tenure in exchange for submission to the jurisdiction of the crown. The other objective was to create an Irish nation. In this interest the liberals induced a reluctant Henry VIII to accept the act changing his title from lord of Ireland to king of Ireland. Ireland became, like England, a realm of the king, a sov-

eign kingdom. Ireland's new status enabled St. Leger to restore Dublin as its center of administration, and he made a small start in transforming the Irish Parliament into a national legislature by including some Gaelic lords. St. Leger saw royal sovereignty as including the royal ecclesiastical supremacy, which he expected all Irishmen to accept, but he allowed the introduction of religious reforms to await persuasion and education. The death of the supportive Henry VIII brought the liberal experiment to an end.

Bradshaw's version of Irish history during the middle Tudor period differs substantially from previous versions. Such a revision is bound to produce controversy, but those who enter the lists will have a formidable array of evidence and argument to overcome. Irish history during the middle Tudor period should be a lively subject for some time to come.

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OLIVER MACDONAGH. *Ireland: The Union and its Aftermath*. Rev. ed. London: George Allen and Unwin. 1977. Pp. 176.

Many students of Irish history recognize that one of the most incisive minds working in the field is that of Oliver MacDonagh of the Australian National University. Part of the reason for this influence is a short book, entitled simply *Ireland*, that appeared as part of the Modern Nations in Historical Perspective series in 1968. Although *Ireland* has had a significant impact on Irish historians, its publication was virtually unheralded in North America during the late 1960s. With interest in Ireland growing in colleges and universities, it is gratifying to welcome the revised and enlarged second edition of *Ireland: The Union and its Aftermath*. MacDonagh's book is not a history of Ireland since 1800 but rather a discussion and analysis of that history. It is not a book for the reader who does not have some grounding in modern Irish history. But the person who wants a better understanding of Ireland during the last two centuries will find *Ireland* packed with compelling observations, ideas, and, of course, ironies.

The original work examined six principal themes in the development of modern Ireland. These chapters appear with only minor modifications in the second edition, along with a new section of "historical speculation" on the evolution and possible direction of the Northern Ireland crisis. MacDonagh begins with a detailed discussion of the nature and meaning of the Anglo-Irish legislative union of 1801 to establish the setting for the development of his themes. The Union "fenced in the range of the politically possible" (p. 13) for more than a century

and was crucial in molding people and events. But MacDonagh, a student of the emergence of the modern machinery of government, demonstrates that the Union brought not only more direct British control but also the early fruits of the modern and impartial state. In the second chapter we see that, despite Irish resistance to absorption into a truly united kingdom, Ireland, if not purposely a "social laboratory," was, in fact, a proving ground for structures and policies that were tried only later, sometimes generations later, in Britain. Ireland had a paid, professional magistracy, a coherent, national police force, a national system of health dispensaries, and a national school system before Britain. True, the need was more plain and pressing in Ireland, but the government response was positive, which bequeathed to twentieth-century Ireland a seasoned administrative structure and relevant experience in state involvement in social and economic programing.

The subtle modernization of Ireland under the Union is a theme that is also apparent in MacDonagh's discussion of the development of Irish nationalism and the independent Irish state in the years following 1922. Even though the evolution of the Irish national idea was a direct response to British rule and often—particularly in Young Ireland, the Gaelic League, and the Gaelic Athletic Association—expressed itself in terms of a traditional Gaelic and agrarian ideal, the long, intermittent struggle against the Union bore the hallmarks of profound modernity. O'Connell's mass movements, which mobilized hundreds of thousands of Irishmen in support of national political objectives by utilizing the national organization furnished by the Catholic Church, established a durable pattern for future constitutional movements that instilled a consciousness and tactical skill that made Ireland one of the most politically modern countries of the nineteenth century. The successful establishment of the new state in 1922 contains an irony in that the Irish Free State pursued a traditionally British policy with the aid of a substantially British-trained civil service and a very able executive that had learned well the lessons of a century of administrative development. Although the first Free State government forced a determined and armed republican opposition into constitutional politics, MacDonagh includes a good discussion of how the Anglo-Irish settlement was steadily undone not only by De Valera during the 1930s and 1940s but also by the first independent government.

Ireland concludes with a valuable new section that traces the Irish "tradition of alternative government" (p. 144) that has contributed to the Northern Ireland crisis. The "Whiteboy" origins of opposition politics in Ireland, which refer to local agrarian groups of the late eighteenth century that organized

to oppose changes perceived as being in any way adverse, are seen in contemporary Ulster politics, as both communities try to establish alternative structures to preserve their respective heritages and interests. The range of our knowledge and understanding of Irish history has increased tremendously since 1968, but *Ireland* holds up well and has a freshness that marks MacDonagh as a hard-thinking and forward-looking scholar. *Ireland* is thoroughly stimulating and enjoyable reading, and it is an excellent selection for graduate students who want to understand better Britain's Irish problems.

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EMMET LARKIN. *The Roman Catholic Church and the Plan of Campaign in Ireland, 1886-1888*. Cork: Cork University Press. 1978. Pp. xv, 334. £9.00.

EMMET LARKIN. *The Roman Catholic Church in Ireland and the Fall of Parnell, 1888-1891*. Chapel Hill: University of North Carolina Press. 1979. Pp. xxi, 316. \$19.00.

These two volumes are serial to Emmet Larkin's 1975 study, *The Roman Catholic Church and the Creation of the Modern Irish State, 1878-1886*. The first develops the theme of the earlier volume for the years 1886-88, the second continues the story for the 1888-91 period, which witnessed the fall of Parnell. Like the earlier work, these two books present, in exhaustive detail, the correspondence of members of the Irish Catholic hierarchy with each other, with Rome, and with members of the government who supported the clerical-nationalist alliance of the Parnell years. Larkin has severely restricted his sampling of letters to those that throw light on the maneuverings of a peculiarly political generation of bishops. Reading rather like extended articles from a learned journal, the chapters of the books are composed of a clever linking together of letters to form a narrative account of the evidence that Larkin fits into a historical model to explain the development of Irish Roman Catholicism at this time. Each volume has a prologue in which the thesis of the work is set out and an epilogue to indicate how the evidence of the work fits into the model.

The two books represent, with Larkin's earlier volume, a remarkable tour de force, with other volumes promised. Historians of Irish affairs will long appreciate Larkin's shrewd assessments of the often devious political maneuverings of calculating prelates, like the nationalist archbishops, William Walsh of Dublin and Thomas Croke of Cashel. Larkin delights in exploring their complex motives of behavior, almost on a day-to-day basis; most of his judgments have a ring of truth about them. His his-

torical empathy allows him to share the antipathy of most nationalist bishops of the "Plan of Campaign" era toward a difficult prelate like Edward Thomas O'Dwyer of Limerick, who refused to march to the Parnell drum. On the other hand, he also notes the thought of priests like Arthur Griffin of Kerry who represented the tradition of the Cardinal Cullen and Bishop Moriarty era by protesting murder and other agrarian outrage.

Scholars, of course, will question the inevitable "sampling error" that arises from Larkin's historical technique, which he labels "mosaic." He selects and arranges manuscript evidence to form "a representation of what was true," as he says in the prologue of both his books. His hope is that by the sheer volume of the evidence he presents, including whole letters, the reader will be persuaded to accept the "reality" of his "portrait." He admits, however, that the evidence is fitted into the "mind's eye" pattern of history held by the author and it will naturally not be appreciated by everyone.

When he speaks, for example, of the "making and consolidating of the *de facto* Irish state" (vol. 1, p. 318), is he not, in his "mind's eye," referring to a southern Irish Catholic confessional state, like that of the present republic, rather than to an "Irish state"? Against his use of a kind of Whig version of history, some historians would argue that there is a not inconsiderable portion of the Irish population, who have never welcomed the kind of "state" that, Larkin argues, Parnell had already brought into existence. His "pattern" seems to suggest that Larkin has identified himself with those nationalist historians who have tried to simplify Irish history by pretending that Ulster and its intransigent population simply do not exist. Quite apart from the Protestant population, which generally had no use for Home Rule, Ulster had some moderate Catholic prelates who hardly fitted easily into the southern Catholic-Nationalist alliance. If Daniel McGettigan of Armagh, for example, had belonged to the tradition of Walsh and Croke, his funeral would not have been graced by the presence of the Protestant primate and droves of Protestant clergy and laity.

Apart from this common error of many Irish historians, denying Ulster its existence by ignoring its history, we are all greatly in debt to Larkin for these two volumes, filled with so much hitherto unpublished material. When historians of different persuasion from Emmet Larkin look at his evidence, some will question his "portrait" of the age, for all "mosaics" seem at times to be lacking in dimension. It is doubtful, however, if any historian will improve much upon his sad picture of how secular and power-hungry the Irish "priest in politics" can become.

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PATRICK BUCKLAND. *The Factory of Grievances: Devolved Government in Northern Ireland, 1921-39*. New York: Barnes and Noble, and Gill and Macmillan, Dublin. 1979. Pp. xiii, 364. \$27.50.

In the first work to make extensive use of recently opened cabinet and departmental archives, Patrick Buckland has explored in detail the workings of the Northern Ireland government in the interwar years. The first of three major sections describes and analyzes the overall framework of the provincial regime and its relationship to local authorities and to Westminster. The author then deals with those functions (finance, trade and industry, agriculture, social services) that have not usually been the main focus of Catholic and nationalist criticism of the regime. Finally, he turns to the governmental powers that usually figure most prominently in discussions of Northern Ireland because of allegations of discrimination against the Catholic minority in their exercise: law and order, justice, and the control of electoral and educational arrangements.

Buckland has striven mightily to make interesting reading of such topics as the Joint Exchequer Board and the 1924 Eggs Marketing Act. Readers should resist the temptation to skip over such topics to get to the material that seems to bear more directly on contemporary sectarian conflict. The book's great contribution is to demonstrate, by examining routine government functions, that the central problem of the Northern Ireland regime was not its draconian power but its tragic inefficacy.

Although he favors partition "as the most practical way of reconciling the apprehensions of Ulster Unionism with the aspirations of Irish nationalism" (p. ix), Buckland finds little to praise in the workings of the northern government. Even in its exercise of those functions largely irrelevant to the minority problem, he finds only limited areas—for example, agricultural policy—in which the devolved administration managed to make a positive contribution to its citizens' welfare. He argues that the existence of sectarian polarization in Northern Ireland only exacerbated the fundamental unworkability of devolution within the United Kingdom. The book is therefore a contribution to contemporary discussion of devolved government for Scotland and Wales, as well as to Irish history. "There really is no half-way house," Buckland concludes, "between union and complete separation" (p. 280).

The author's commitment to deal only with topics on which departmental archives were available to him sometimes seems unfortunate. For example, he breaks off his account of "law and order" issues in 1922 because constabulary records are still closed after that date. Although he obviously could not treat that issue definitively for 1923-39, it would have been useful to have at least a brief sketch of what can be known from published sources. Never-

theless, within the limits he sets for himself, Buckland's research is thorough, his analysis keen, and his judgment sound.

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JEAN-PIERRE GUTTON. *La sociabilité villageoise dans l'ancienne France: Solidarités et voisinages du XVI^e au XVIII^e siècle.* (Le Temps et les Hommes.) Paris: Hachette. 1979. Pp. 294. 65 fr.

For some years there has been a need for a good synthesis of recent important work on village life in Old Regime France. Jean-Pierre Gutton has provided us with that synthesis. His modest-sized book will give the nonspecialist some excellent insights into the *sociabilité* of French peasants.

The book's coverage is more modest than its title at first indicates. It is in fact a summation of village *sociabilité* during the seventeenth and eighteenth centuries with occasional forays into the fifteenth and sixteenth and very rare references to the middle ages. But even that is a staggering undertaking, for what the French mean by *sociabilité* is nothing less than the entire anthropology of village life.

Gutton's book will not provide many new insights for the specialist in Old Regime social history—nor is it so intended. His main thesis is that peasant life had for centuries been centered on village communities that provided a nearly complete universe for their members. In the seventeenth and eighteenth centuries, however, most of these communities were weakened under the dual attack of statism and the Counter Reformation.

Gutton summarizes the social structure of peasant communities and discusses various forms of families to be found (a reality far more complex than the "extended-nuclear" dichotomy so beloved by sociologists), internal polity, and relations with state and church. I found the discussions of family structure, village government, tax collection, the nature of the *seigneurie*, and the changing role of the church especially well done. There is a great deal of noteworthy information given by means of well-chosen examples. Indeed, a good many lectures on European social history will be rewritten after reading this book. To take only one example, Gutton destroys that cherished belief that the typical parish priests were "of the people," in large part promising young peasants who made it up the social ladder. As it turns out, this was rarely the case. In the dioceses of Tarbes, La Rochelle, and Autun in the seventeenth century and that of Strasbourg in the eighteenth, the parish priesthood was mainly of bourgeois or noble origin. In lower Auvergne in the eighteenth century 10 percent were sons of rich *laboureurs* and the remaining 90 percent sons of magistrates, merchants, or well-to-do artisans. For those

not conversant with the research of the past decade, Gutton's book is filled with such revelations. Above all it conveys a sense of the enormous complexity of village *sociabilité* from region to region.

There are two modest criticisms to be made of the book. First, Gutton is uneven in his treatment of the regions of France. His own monographic research has been concentrated in the Lyonnais, and he naturally seizes upon that region for the greatest number of examples. Some regions rarely get mentioned. Second, very little research by non-Frenchmen is cited. Both of these criticisms are minor because to push them would be to demand a different book rather than critique this one. *La sociabilité villageoise* is not the final summation. It is a modest beginning, but one that deserves to be widely read. Indeed, I hope that some publisher sees fits to translate it, for the book would be even more valuable for advanced undergraduates and others who are not comfortable reading in French.

JOHN B. CAMERON, JR.
University of Southwestern Louisiana

JACQUELINE-LUCIENNE LAFON. *Les Députés du Commerce et l'Ordonnance de Mars 1673: Les juridictions consulaires, principe et compétence.* Preface by JEAN IMBERT. Paris: Editions Cujas. 1979. Pp. ix, 153.

The deputies of commerce represented the interests of trade and industry before the Council of Commerce, created in 1700, and the Bureau of Commerce that supplanted the council and lasted from 1722 until the Revolution. There were twelve deputies, and they were selected from the upper ranks of the merchant aristocracies in the leading commercial cities of eighteenth-century France. Their function was to advise royal *commissaires* in the council or bureau on a variety of economic, financial, and administrative issues affecting French merchants.

Jacqueline-Lucienne Lafon, a student of commercial and public law, examines the role of the deputies in the interpretation and possible reform of Colbert's commercial ordinance of 1673. Her most important primary source is the F¹² series ("commerce et industrie") at the Archives Nationales, which contains a subseries devoted to the Council and Bureau of Commerce. As a first step toward a general study, she has written a short monograph on the relations between the deputies and the "consular" law courts that, though initiated in 1563, looked back to the commercial ordinance as their main charter. The sixty-seven *juridictions consulaires* consisted of part-time judges called "consuls," who were in fact merchants drawn from the most powerful merchant corporations. They had competence over mercantile and business litigation, with the

parlements and the Royal Council sharing appellate jurisdiction.

The main feature of Lafon's study is to show that the deputies actively protected these business courts from the threats of regular tribunals, who wished them abolished, and from the ambitions of lesser merchants in other towns, who hoped that they would proliferate. In these efforts the deputies were successful, and the *juridictions consulaires* of the eighteenth century were essentially those of the late reign of Louis XIV. On the other hand, the deputies failed to extend legal competence beyond the territorial limits of the town or bailliage where the courts sat, and they were rebuked more than once in their efforts to enlarge the definitions of business activity then used for legal purposes.

Although the author's perspectives are rigorously narrow and she is overly generous in her praise of the deputies (who are always diligent, active, well informed, and so on), her legal analyses are most carefully done; and she provides some insights into the actual functioning of the bureaucracy of the Old Regime.

JOHN J. HURT
University of Delaware

ALAIN LOTTIN. *Chavatte, ouvrier lillois: Un contemporain de Louis XIV*. Paris: Flammarion. 1979. Pp. 445.

Pierre Ignace Chavatte (1633-93) was a modest *sayetteur* (a weaver of a local variety of inexpensive woollens) who lived and worked in the poorest quarter of seventeenth-century Lille. His father was a *maître sayetteur* who had been driven by hard times to hire out his services and who had once been convicted and pardoned for murdering a man in a drunken brawl. His mother and his wife were both midwives, and their work gave them access to the more intimate details of family life in the teeming alleys and courtyards of this preindustrial slum. Chavatte himself was also forced to work for others and considered himself an *ouvrier sayetteur* even though, like his father, he had been received as a master. He was thus a rather ordinary representative of the *menu peuple* who lived through the plagues, wars, shortages, and unemployment of the age of the French conquest. What set Chavatte apart was that he was able to compose a chronicle of the life of his city in which he recorded firsthand, in his own colloquial language and phonetic spelling, all the ceremonies, incidents, mishaps, and gossip that interested him from 1657 to 1693. His unusual manuscript thus offers a rare glimpse of the mental universe of artisans in an important center of traditional cloth manufacture.

Alain Lottin's book is an analysis of Chavatte's chronicle and, by extension, a comprehensive study of the *mentalité* of people from his milieu. Lottin has

done an admirable job of remedying the anecdotal nature of this sort of document by checking out every reference, adding material from other memoirs, and extensively researching the matters described by Chavatte. The result is an original piece of scholarship, not just an editing job. Indeed, it is a rare pleasure to find a weaver's descriptions corroborated and filled out with official administrative correspondence instead of the other way around!

This study is therefore an invaluable tool for specialists interested in urban popular culture. I found it disappointing, however, because it confirms and illustrates many well-known generalizations about popular world-views without offering any real surprises. Chavatte's frame of reference was absolutely traditional. He thought in symbolic terms, attributing disasters to the will of God and believing in relics, magic, and witches. He was a devout Catholic who sympathized with the unfortunate. He was also a connoisseur of festive rituals, priestly oratory, and popular amusements.

Two interesting phenomena stand out. One is the way the Lillois retained their traditional loyalties—to the Spanish crown, the pope, the old-fashioned mendicant friars—long after the French annexation of 1668 and how they turned their preferences against Louis XIV, his Gallicanism, his wars, and his Jansenist bishop of Tournai. The other is the extent of Chavatte's economic particularism. He stubbornly defended the superiority of his guild's rather low-grade weaving "style" even against closely allied ones, and he opposed all economic concentration, believing that the size of workshops should be strictly limited in order to spread the work around. Chavatte's voice is worth listening to, and Lottin's presentation helps it to be heard. If the message is rather familiar, this may be only because of the time that has elapsed since 1968 when this study was defended as a thesis.

WILLIAM BEIK
Northern Illinois University

CLAUDE-FRÉDÉRIC LÉVY. *Capitalistes et pouvoir au siècle des lumières*. Volume 2, *La révolution libérale, 1715-1717*. The Hague: Mouton; distributed by Walter de Gruyter, New York. 1979. Pp. 379. DM 72.

This second volume of Claude-Frédéric Lévy's study of French financial affairs, politics, and foreign policy in the early eighteenth century covers the first two years of the regency, a period of significant change in all these spheres. France's financial situation at Louis XIV's death, Lévy shows, was critical; there had been few attempts to reform either the fiscal or credit systems despite growing international awareness of the possibility of improvements in both. One of the reasons for this failure was the corruption of the political system; those

benefiting from the existing systems wielded great influence.

The "liberalism" of the regency consisted in its attempts to shake off these influences and introduce new policies in line with those practiced in Holland and England. Initially, traditional remedies were tried: devaluation of the *livre* in terms of metallic currency, a partial repudiation of the vast quantities of depreciated *billets de banque* circulating, and the establishment of a *Chambre de Justice* to examine the gestion of those involved in the state's financial affairs. These methods having failed, John Law's proposition for the founding of a private, national, note-issuing bank was accepted. Lévy describes in great detail the origins of this bank about which, until now, we have been ill informed. The foreign dimension of the regency's liberalism consisted in a rejection of pro-Catholic policies in favor of a rapprochement with Holland and England, a policy that was crowned with success when the Triple Alliance was signed in 1717. Lévy follows in meticulous detail the complex negotiations that led to this treaty.

The study ends with the foundation of the *Compagnie d'Occident* in 1717. This, too, was a liberal step for the company's charter, modeled on those of English and Dutch companies, represented a rejection of the monopolistic principle apparent in the charters of the French companies founded previously. Displaced during these years were the *frères Crozat*, the most powerful financiers in France in 1715. Lévy's book begins and ends with references to their situation. The unifying theme of the volume is the gradual displacement of the Crozats, and their like, from royal counsels.

Lévy is immensely knowledgeable about this period; he writes as though he had lived through it. He has carried out thorough research in both French and foreign archives and made particularly good use of notarial sources for piecing together the often clandestine activities of financiers. The book is written as an extremely dense narrative. Lévy doubles back when any important new figure is introduced and provides a short biographical sketch. His book, which makes difficult reading, will serve above all as a work of reference. Its utility as such is enhanced by the conscientious footnoting and full bibliography and index.

JAMES THOMSON
University of Sussex

ROBERT DARTON. *The Business of Enlightenment: A Publishing History of the Encyclopédie, 1775-1800*. Cambridge: Harvard University Press. 1979. Pp. xiv, 624. \$20.00.

This massive, ambitious work recounts the publishing history of the *Encyclopédie*, especially some of its

later editions, and attempts to set an example of how to write a history of book publishing. Whereas specialists have tended to concentrate on one facet of any book, Robert Darnton recommends that all aspects of a book be studied, including its origins, manufacture, marketing, diffusion, and influence.

Following these recommendations in his account of the *Encyclopédie*, Darnton combines the new research techniques of the French masters of *histoire du livre* with more traditional scholarship. An independent thinker, he freely criticizes practitioners of any technique. He relies not merely on the standard printed sources but largely on manuscripts. His greatest treasure-trove is the papers of the *Société typographique de Neuchâtel*, which include thousands of documents on the 1777-79 quarto editions of the *Encyclopédie* as well as documents on other editions. He quotes from these documents in detail and in the original French. Some readers might find this irksome; all will admire his skill in deciphering various handwritings and in piecing together a very complicated story while making it understandable and colorful.

Darnton stresses certain facets of the book trade more than others. Fascinated by business history, he devotes hundreds of pages to the publishers' contract disputes, espionage, and trade wars accompanying several editions of the *Encyclopédie*. The result is that his description of the publication of the quarto editions, of the 1778-82 octavo edition, and of the *Encyclopédie méthodique* will now be the standard accounts; they serve as his illustrations of the cutthroat competition of eighteenth-century publishing.

Darnton is also an excellent guide to how the quarto editions were produced. As a bibliophile and social historian he elaborates, for example, on the kinds of paper used, the methods of typography, the money printers earned, and their working conditions.

Moreover, Darnton provides valuable data on the sales figures and distribution of various editions of the *Encyclopédie*. He proves that it was a best seller. Although the first edition was very expensive and was bought mostly by the elite, the later, cheaper editions reached the bourgeoisie of much of France and sold widely elsewhere in Europe.

Other sections of the book discuss the contributors to the first edition of the *Encyclopédie* and the contributors to the *Encyclopédie méthodique*. Darnton compares the two groups with regard to such matters as their social and professional backgrounds and their reactions to the French Revolution. Until much more biographical research is completed on these Encyclopedists, generalizations based on prosopography can be considered only tentative.

The Business of Enlightenment is perhaps least informative about the history of ideas. Darnton says one cannot know what readers thought of the *En-*

cyclopédie. But we do know what some readers thought. For the first edition of the *Encyclopédie*, one can turn to John Lough's *Essays on the "Encyclopédie" of Diderot and d'Alembert* (1968). In over one hundred and fifty pages Lough provides comments about the *Encyclopédie* drawn from eighteenth-century books, pamphlets, and periodicals. There should be some remarks about later editions as well in similar eighteenth-century sources. Also, Darnton's book contains little about the contents of these encyclopedias. He says that five of the editions are versions of "Diderot's basic text" (p. 36). But what exactly is "basic"? The quarto editions, for example, are much less impious and learned than the first edition, partly because many of the most important articles and plates were excised by a hack editor. How much and in what ways was Diderot's great work vitiated in later editions? Darnton only touches on such issues, but they are important in estimating the impact of the *Encyclopédie* and the Enlightenment from 1775 to 1800, one of his central concerns.

For many years to come scholars will turn to Darnton's brilliant work. They will learn a great deal about the publishing history of the *Encyclopédie*, the eighteenth-century book trade, and French history in general. They will also find a model of how to study a book in its totality.

FRANK A. KAFKER
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ALAN WILLIAMS. *The Police of Paris, 1718-1789*. Baton Rouge: Louisiana State University Press. 1979. Pp. xx, 328. \$24.95.

Police, in the Old Regime sense of the term, referred to an agency of administration as well as to the objects (and objectives) of administration. In the first part of his book, Alan Williams deals with the agency, component by component. Most of the material is familiar; not even the evocation of the assemblies of police is as ground breaking as Williams claims. But these chapters have the merit of constituting a more or less exhaustive *organigramme*, featuring not only the protagonists but also the less well-known actors as well (watch and guard, fire service, building inspectors, and so forth). In part two, the author treats the objects and objectives: various modes of social control and relief, sanitation, and others. Williams focuses upon those areas in which the police introduced improvements that made Paris "a better place than it had been." This perspective suggests yet another reason why the Revolution may have been superfluous. But it begs one of the most interesting questions that the author promised to address at the outset: the "impact" of the police on the regime itself.

A major weakness of this study is its abstractness and remoteness. The police remain faceless and elusive, from top to bottom. We learn virtually nothing about the men who shaped the institution decisively (Sartine especially), about the bureau heads (was Puisan a member of the farmer-general family?) and the treasurers (what about Rouillé's connections with the financial milieu?), or about the commissioners, who cry out for prosopographical investigation. Williams's distance from the *quartiers* of the capital leads him to exaggerate the infringement upon and diminution of the responsibilities and powers of the commissioners and to ignore their semi-autonomous corporate existence. He overlooks the crucial question of the relations between the police and the people of the *quartier* and their deeply ambivalent perception of each other. He underestimates the menace of worker insubordination and overestimates the significance of servant criminality. The author rightly construes the placement of guardposts near the markets as a guarantee of order. But because he extrapolates "reality" from forms and regulations, he fails to note that the guards were often a source of disorder in the markets. By a similar process of reification, he presumes—incorrectly—that workers and domestics did not carry weapons because the regulations invested the guard with a monopoly of violence. He cites fines as a form of dissuasion and punishment. Yet he never moves beyond the police sentences to ask whether the fines were ever collected, how they were collected, and what happened if they were not paid. The guild statutes might suggest that *sindics* and *jurés* needed "prompting" to exercise police, but in practice their overzealousness posed a greater problem than their reticence. The guilds were "locked against the intrusion of any newcomers," if one subscribes to liberal propaganda, but not necessarily if one counts corporate admissions. Can one infer policy priorities on the basis of the allocation of men and money? Because more men were needed to police the streets than to provision the markets, does it follow that authorities counted on "fear" more than on bread? Moreover, it is extremely unlikely that the data on police expenditures are complete.

It may be that the only way to put flesh on the police skeleton is to plunge into the "Y" series at the Archives Nationales. Williams dipped into it timidly, but that is understandable, for the Y is a labyrinthine swamp. One way to compensate would have been to draw upon the many *mémoires de maîtrise* based on the Y and other police documents directed by P. Goubert, E.-M. Benabou, and others. It is surprising that Williams did not exploit the large manuscript correspondence in the Bibliothèque Historique between Police Chief Marville and Police Minister Maurepas (part of which

S. Pillorget has just published). The author ought to have justified his heavy dependence on Lenoir's papers by subjecting Lenoir's testimony and ideology to critical scrutiny.

STEVEN LAURENCE KAPLAN
Cornell University

ISSER WOLOCH. *The French Veteran from the Revolution to the Restoration*. Chapel Hill: University of North Carolina Press. 1979. Pp. xix, 392. \$27.00.

Isser Woloch's intention in his study of the French veteran's prospects and fate from the declining years of the Old Regime to the Restoration is to look at some of the larger questions in social history that might otherwise have been buried by excessive study of the local and particular, thus riveting attention upon the unchanging nature of social institutions and policies. What commands his attention instead are the possibilities for change that ensued from two intersecting phenomena. The first was the growth of a substantial force of the male population that was mobilized for war yet survived it. He estimates that there were some 150,000 pensioned veterans from 1792 to 1815. Second were the policies devised to deal with them under the dual forms of welfare and control. He concludes that the promise of change embedded in the egalitarian aspects of the law of June 6, 1793, which aimed at erasing distinctions of rank in the benefits accorded veterans, was short-lived and that the reaffirmation of paternalistic forms of authority was congruent with the political changes initiated by the Directory and carried further under the Consulate and Empire.

The very important information that Woloch assembles and the insights that he offers might have been tied more firmly to his larger ambition of understanding change and resistance to change had he looked beyond the Jacobin failure to some of the prevailing attitudes toward a significant part of the population. He rightly points out that the rationalization of the plight of the veteran was part of a movement to deal with many groups in the population that were increasing in size, and because of disabilities and distress of various kinds, consuming, rather than contributing to the development of, the nation's resources. All or nearly all the images of the poor, their threats to order, their presumed laziness and dissolute life styles, their resistance to disciplined forms of work, were, with one exception, duplicated in the official views of veteran psychology and social behavior.

What distinguished the veteran from the other dependent sections of society was, of course, the nation's debt to him, which demanded a rather different kind of treatment: recognition and decent material benefits. The first was continually being

undermined, however, by the deterioration of the second. The veteran generally did not fare well, or, if he did intermittently, he was continually made aware of his marginal position by the authorities who shunted him from the *Invalides* to other institutions, devised self-help schemes that depended on grants of land and capital, recommended forms of labor deemed appropriate for the physically disabled, or, under Napoleon, administered resettlement projects in "frontier" regions outside France where he was supposed to regain self-respect through tilling the soil and serving as an emergency combat soldier in case of military need.

By arguing that the state's assumption of responsibility for the *mutilés de guerre* reduced poverty levels, Woloch inadvertently brings us face to face with the almost certain knowledge that the volunteers and conscripts were probably already part of the marginal sections of the population and would have swollen the ranks of the poor and the sick had not the war intervened to litter the battlefields with their bodies and their severed and scattered limbs. Continuity is not proved by the revived paternalism under Napoleon. The change, there for us to see, is to be found in this earliest of the modern, large-scale schemes of care, geared to cost analysis, discipline, and the suppression of dissent. This may have been the war's supreme irony.

HARVEY MITCHELL
University of British Columbia

IRENE COLLINS. *Napoleon and his Parliaments, 1800-1815*. New York: St. Martin's Press. 1979. Pp. 193. \$22.50.

Irene Collins's monograph deals with the bodies specifically granted the "legislative power" in the Napoleonic constitutions—how they were formed, how they functioned, and how Napoleon managed them. Thus she concentrates on the Legislative Body (*corps législatif*) and the shorter-lived Tribunal. The Senate and Council-of-State are treated as apart from "parliament"—which is technically correct.

Collins makes no attempt to alter the accepted view that Bonaparte dominated his "parliaments." However, she does offer a convincing rationale for his treatment of them. For example, it is usually assumed that Napoleon abolished the Tribunal in 1807 because its members were troublemakers—despite the fact that he had already unconstitutionally ousted twenty of them in 1802 and cut their numbers from one hundred to fifty in 1804. Not so, says Collins, who holds that he actually eliminated the Tribunal for the reason he stated at the time—it had lost its "utility." In fact, in 1807, the tribunes were straining to cooperate with the emperor, but

the system by which the Tribune debated legislation and then reported it to the Legislative Body (which voted without debating) had not worked. Napoleon thus integrated the remaining tribunes into the *Législatif*, creating a single-house legislature that both debated (by section) and voted.

The emperor still had to endure opposition in the *Législatif*. He preserved it, nevertheless, but not, says Collins, to maintain "a pretense of parliamentary government," as is generally assumed. He saw it as an essential "means of communication between the government and the public." He insisted that every new law be publicly introduced, explained, and justified by his councillors-of-state—and then justified again by legislators from the section that had debated it before the whole *Législatif* voted.

All this makes sense if one remembers that Napoleon was an authoritarian democrat who prided himself on governing for the people. The Legislative Body was a useful forum, therefore, for explaining why his laws were in the general interest (any opposition was expressed only by the raw number of negative votes, registered without comment). The "people" he wanted most to reach, of course, were the *notables* of the departmental electoral colleges and their peers.

Irene Collins's book is not without flaw, but she has made a useful contribution to the political history of the Napoleonic period.

OWEN CONNELLY
University of South Carolina

MARIE FLEMING. *The Anarchist Way to Socialism: Elisée Reclus and Nineteenth-Century European Anarchism*. London: Croom Helm, or Rowman and Littlefield, Totowa, N.J. 1979. Pp. 299. \$26.50.

After many seasons of neglect, the career of Elisée Reclus has received an English-language biographical interpretation for the second time in two years. Marie Fleming's work is a commendable effort by a Canadian-based scholar to condense the varied ideas of the prolific geographer-anarchist into a single small volume.

Fleming's work follows soon after Gary S. Dunbar's *Elisée Reclus: Historian of Nature* (1978) and complements it in many ways. Dunbar offered an analysis of the scientific side of Reclus's career as a popular geographer; Fleming has focused her attention on the evolution of his anarchistic ideas. Both have made extensive use of family archives, socialist and anarchist files, and other primary material scattered across Western Europe.

There is little consideration of Reclus's achievements as a geographer in Fleming's work; this book alone would inspire little appreciation of the French scholar's influence outside the spectrum of anar-

chism. The last chapters are largely a montage of Reclus's ideas in the realm of politics; Fleming's difficulty in organizing her material is obvious. She has tried to fill the gaps in the historical evidence with informed speculation, which is identified as such. Like most biographers, Fleming obviously developed a deep affection for her subject; in the penultimate chapters she accepts the assessment of Reclus's contemporaries who regarded him as "the Anarchist Prophet" and "a saint." On the other hand, in the afterword, she concludes that his eschewal of organized political action was naive, and she accuses him of authoritarianism and insensitivity to human suffering.

Fleming argues that the decisive experience in forming Reclus's philosophy was the Paris Commune; his definitive anarchist position was developed after his arrest and imprisonment for participation in the events of March and April 1871. He became increasingly less tolerant of other socialist positions subsequent to those developments. Yet she argues, with dubious reasoning, that Reclus was more purely anarchistic and more supportive of freedom of thought than his famous anarchist colleague, Prince Peter Kropotkin.

One may quarrel with other generalizations that Fleming makes in passing, such as the assertion that Reclus "was perhaps the most progressive of all prominent nineteenth-century revolutionaries in his views on women nad marriage" (p. 244). But she has made a good argument for the idea that the anarchism of the last century was a remarkably stimulating intellectual leaven, whose force is not spent.

JAMES W. HULSE
University of Nevada,
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ROBERT R. LOCKE. *Les fonderies et forges d'Alais à l'époque des premiers chemins de fer: La création d'une entreprise moderne*. Translated by ELISABETH-ANNE BENOIST-D'AZY. Paris: Editions Marcel Rivière. 1978. Pp. 298.

French industrial history had been enriched by a find of a large collection of the letters of people associated with the *Compagnie des Fonderies et Forges d'Alais* in southern France, a company based on nearby coal mines and iron ore. The letters cover the period of its founding in 1826 until it left the hands of the family in 1874. Robert R. Locke has produced a book based on this collection; it includes excerpts from 333 letters preceded by an 80-plus page explanatory text.

Although one could have wished that the introduction placed the company history a little more securely in the framework of the entire industry—there is, for example, no reference to Jean Vidal's *L'industrialisation de sidérurgie française, 1814-1864*

(1867)—the letters and text do illuminate many aspects of the industry as well as the company, covering technology, finance, recruiting of labor, finding outlets, and the like. There was a long period of teething troubles, and the firm never really made profits until the railway boom picked up in the 1840s and especially after the establishment of the Second Empire at the end of 1851. There is a revisionist view that French industry was thriving in the first half of the century after the Treaty of Vienna, but it is hard to find support for this view in the history of Alais. Technological help was sought by obtaining consultants and workers in England, as well as by trips there by Emile Martin, a consultant, and Benoist d'Azy, the central figure in the company along with the Paris banker Hippolyte Drouillard. Although the company had good connections to financial circles in Paris and Marseilles, its success waited on political help to Paulin Talabot's Paris-Lyons-Marseilles railroad and its antecedents; all hands in the Alais iron works turned out to influence the parliamentary vote on a 6 million francs loan for the railroad.

Of particular interest to this reader are the interruption of letters at the end of 1847 and early 1848 and the subsequent news that workers in Alais had rioted four times and had expelled the English workers, including one Shakespeare, who had been with the company since 1836. (A British railroad chief engineer with the Paris-Orléans railroad had been killed in one such riot.) Attacks on the persons of English technical staff help to explain why the British, so eager to invest in French railroads and in Alais in the 1840s, turned elsewhere after mid-century, compelling the French to create new financial means to develop the country.

A letter from Georges Dufaud of Fourchambault in the Nivernais lists the major iron producers in 1845: Alais is tied with Le Creusot at 12,000 tons of rails capacity per year, behind Decazeville at 15,000, and ahead of Hayange in Lorraine at 10,000. The total capacity in thirteen plants was 88,000.

Nuggets such as these make the book more than a company history, with many glimpses of the coal, coke, and iron industries and the industrialization process in France.

CHARLES P. KINDBEEGER
Massachusetts Institute of Technology

ALLAN MITCHELL. *The German Influence in France after 1870: The Formation of the French Republic*. Chapel Hill: University of North Carolina Press. 1979. Pp. xviii, 279. Cloth \$17.50, paper \$9.00.

Allan Mitchell's previous work has already established him as a leading specialist on Franco-Ger-

man relations in the Bismarckian era. His new book inaugurates his most ambitious project to date: it is the first volume in a planned trilogy that will seek to show that "the German question . . . vitally affected every major aspect of French public life after 1870." German influence in that era, he insists, was not just tangential or temporary but fundamental and enduring. The volume at hand treats the political and economic aspects of that influence, while its successors will focus on military, religious, and social developments in France. For intellectual influences, Mitchell refers us to Claude Digeon's massive monograph.

Robert Paxton has already demonstrated, in his fine book on Vichy, how much can be learned about France by digging through the German archives. The Vichy years, of course, were exceptional, since the Germans were in direct physical control. But Mitchell argues that both during and after the earlier round of German occupation in 1871-73, German pressures and threats weighed heavily on France's destinies. He contends that France became "a certain kind of republic" in considerable part "under the impact of German domination" and that the Third Republic's subsequent history continued to bear the marks of its origin.

Mitchell's case is vigorously argued and is buttressed by a great deal of solid evidence. He shows that Bismarck's pose as a disinterested observer of French domestic affairs was clearly hypocritical; Bismarck remained constantly and almost neurotically vigilant to detect the least sign of either *revanchisme* or political instability across the Rhine, and he resorted time and again to flagrant pressure and outright threats of renewed war in an effort to inspire "a salutary fear" among French leaders. Throughout the 1870s the German statesman intervened repeatedly to stabilize the moderate Thierist republic against its monarchist-clerical rivals; from 1877 onward he switched to support of a prospective Gambettist regime, first against the MacMahon forces and then against what seemed to be a new threat from the left. Although the precise impact of these various actions on French decision makers cannot be measured, Mitchell is convinced that it was considerable and that most historians have underestimated its importance in shaping both the political structure and the economic policies of the Third Republic.

Some readers may suspect that the author has pushed his case a bit too far. Still, the evidence that he presents from both the German and the French archives will not be easily refuted. He demonstrates beyond doubt that German interference and pressures were more often brutal than delicate and that French political leaders, "perpetually apprehensive" of what Berlin might do next, kept trying to reassure the Germans that they only wanted to be good

neighbors. By the end of the 1870s, Mitchell tells us, French leaders "were probably far freer of German control . . . than they realized," but their anxiety still persisted: "apprehension had become a well-conditioned reflex." The mood at Versailles in the 1870s, it would seem, had something in common with that at Vichy after 1940.

GORDON WRIGHT
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INGE SAATMANN. *Parlament, Rüstung und Armee in Frankreich, 1914-18*. Düsseldorf: Droste Verlag. 1978. Pp. 523. DM 86.

Inge Saatmann's study of the Senate and Chamber army commissions during World War I embodies the first sustained analysis of the French parliamentary committee system since Joseph Barthélemy and R. K. Gooch completed their work in the mid-1930s. Unlike her predecessors, Saatmann had available to her the commission records located at the Archives Nationales and the Palais du Luxembourg. She has examined these records with scrupulous care and integrated her findings with the vast literature on governmental structure under the Third Republic. The assembled material sheds light on the resilience of parliamentary institutions even under the most acute wartime strains, notwithstanding the mounting ineffectiveness of those institutions in coping with economic and social problems thereafter.

Saatmann endorses René Girault's controversial thesis that a dynamic entrepreneurial class existed in France before 1914. In large part she credits the business leadership for the nation's successful economic mobilization between early 1915 and the spring of 1917. But, she contends, oligopolistic industries in the metallurgical, chemical, and energy sectors misused their growing political influence. Their chosen instruments were the Senate and Chamber army commissions. These commissions, in Saatmann's view, came to dominate the political process, assuming the prewar roles of the parties and controlling the plenary secret committees. In the short run, the commissions prodded a laggard bureaucracy at the war ministry to fight a modern industrial war. But in longer perspective they constituted the vehicle through which the economic oligarchies took control of the state, undermined "democratic legitimacy," and set in motion forces leading to the disintegration of the Third Republic. The particular villain in this account is Clemenceau, who first in the Senate army commission and then as premier manipulated the patriotic campaign for resisting the Germans in order to undercut the Socialists at home.

The general argument appears more likely to impress kindred spirits in the Bremen school system,

where Saatmann teaches, than to persuade empiricist historians. The author seems unduly eager to find evidence of class struggle where contemporaries saw mostly desperation, confusion, and improvisation. No doubt German heavy industry did take advantage of wartime opportunity to impose a stranglehold on its society and economy, but it stretches credence to claim that a parallel development took place in predominantly agricultural France. Moreover, Saatmann's preoccupation with theoretical issues leads her to emphasize the constitutional features of debates about legislative oversight of the armies, when the overriding concern of those involved was how to pursue the war most effectively. Finally, her narrow research focus results in occasional errors of judgment or emphasis. She might, for example, have softened passages defending Malvy as interior minister if she had perused War Ministry records that demonstrate his complicity with defeatism.

The great contribution of this work lies in the specific information it provides. There are full accounts here of the workings of the legislature under wartime pressures; the legislature's relations with the executive, the press, and industry; and civil-military relations. Saatmann is notably lucid on the process of military procurement, and she illuminates the unfortunate Salonika expedition, the fall of Lyautey, the disastrous Nivelle offensive, and the fruitless negotiations for a compromise peace. The chapter on the secret committees of 1916-17 is particularly instructive. Students of Third Republic political and institutional history will want to read this book closely. The logorrheic style may strain the patience of nonspecialists, and the digressions on Balzac, Habermas, and Montesquieu will not appeal to those partial to a spare narrative line. The persevering will, however, feel amply rewarded by Saatmann's arduous research on a compelling subject.

STEPHEN A. SCHUKER
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CLEMENS A. WURM. *Die französische Sicherheitspolitik in der Phase der Umorientierung, 1924-1926*. (Europäische Hochschulschriften, series 3, number 115.) Bern: Peter Lang. 1979. Pp. 746.

This monograph by a student of Gerhard A. Ritter, is a well-researched, carefully constructed, and (for the most part) closely argued account of the reorientation of French "security policy" in the Locarno period. Burdened by financial weakness and recurring currency crises, French governments jettisoned their "continental system" based on forceful execution of the Treaty of Versailles and East European alliances in favor of compromise with Germany and cooperation with Britain and America.

Clemens A. Wurm's major contribution is his analysis of the domestic and foreign economic context of French diplomacy. His thesis rests on extensive research in French archival sources, especially ministry files and private papers made available by the Quai d'Orsay since 1972.

Wurm's theoretical introduction defines *Sicherheitspolitik* as an integration of foreign policy, economic policy, military strategy, and such intangibles as misperception of a historical adversary, disjuncture between ends and means in strategic planning, and conflicts of priority between "national security" and the "domestic security" of ruling elites. Complete integration of all these factors would, in the author's view, permit an apotheosized *Totalgeschichte* (pp. 8-10). This is not yet possible, we are told, but the elements noted still serve to clue the reader into the many shortcomings of French security policies in the 1920s. This duty behind him, Wurm then eschews theory for an empirical, common-sense "sorting out" of the domestic and strategic factors shaping French policy.

The *Cartel des Gauches*, we learn, was never an "echte Realität" (p. 133). Rather, war and victory helped fracture the French left by "legitimizing" the republic, eroding the negative consensus around anticlericalism and antireaction, and sharpening social and economic issues on which the Radicals and Socialists parted ways. Thus the thesis that the cartel was laid low by the *mur d'argent* must be refined—divisions within the left and the financial incompetence of the cartel's leadership contributed. This collapse gave conservatives another opportunity to call for "national unity" and seek a foreign solution to avoid sacrifices at home, an "Instrumentalisierung aussenpolitischer Zusammenhänge zu innenpolitischen Zwecken" (p. 176). Although he shys away from a strict "primacy of domestic policy" interpretation, Wurm does assert the centrality of the continuing financial crisis in French policy from the Ruhr to the Dawes Plan to Locarno to Thoiry.

France's mid-decade dilemma arose from its inability to marshal the resources to support sanctions against Germany and play midwife to the new East Europe. By 1925, with the end of the respite after the Morgan loan and the London Conference, the cartel failed to fashion an acceptable fiscal program, and the franc sank anew. Meanwhile, the war debt obligations to the Anglo-Saxon powers still hung over France, industrial interests lobbied for renewal of the "natural" ties of French and Ruhr metallurgy, and domestic pressure rose for another lowering of the military term of service. This conjuncture of forces impelled a policy of detente. The payoff? Wurm argues that Briand formulated his "peace policy" leading to Locarno with the prospect of U.S. financial aid firmly in mind (p. 407). He then moves to a lucid account of the negotiations for

commercialization (public sale) of the German Dawes Plan obligations, which could have yielded considerable sums in reparations and eased the French currency crisis.

Poincaré returned to power in July 1926 and supported commercialization, hoping to open Western (especially U.S.) capital markets to the German bonds. Briand believed this would require German consent, hence revisionist concessions to Berlin. This set the stage for the climax of the French stabilization struggle and of Briand's Locarno policy—the meeting at Thoiry. Briand would seek Stresemann's aid in solving French internal problems, while concurrent negotiations between French and German heavy industry restored the prewar cartel ("das sichtbare Zeichen der Beendigung des 'Krieges nach dem Kriege'" and evidence of the "pazifistischen Schritt der Schwerindustrie" according to industry spokesmen [p. 443]). But Briand's initiative was undone by Poincaré's domestic stabilization package that removed the urgency for a grand reparations-revisions compromise with the Weimar Republic. The Thoiry proposals, which Wurm admits were half-baked (p. 552), met opposition from America, Britain, and the French army in any case, clarifying the international and domestic limits to Franco-German reconciliation in the 1920s. There would be no return to a power policy as advocated by diehards like Louis Marin, but neither would Briand's peace policy suffice to appease Germany. These interpretations are generally persuasive and very much in line with those of the unpublished dissertation of Edward Keeton, "Briand's Locarno Policy: French Economics, Politics, and Diplomacy, 1925-1929" (1975).

Finally, a quixotic complaint. Although the *Europäische Hochschulschriften* series performs a welcome service, it is nevertheless unfortunate that important works such as this come to us in the encyclopedic, undigested form of the dissertation. The 559 pages of typescript are excessive, the customary *Personenregister* is useless as an index, and the backnotes are a verbal maze. There is a point beyond which such aggravations cease to be "cosmetic" and begin to interfere with a publication's utility.

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BARTOLOMÉ BENASSAR. *The Spanish Character: Attitudes and Mentalities from the Sixteenth to the Nineteenth Century*. Translated by BENJAMIN KEEN. Berkeley and Los Angeles: University of California Press. 1979. Pp. xiii, 325. \$14.95.

This essay, originally published as *L'Homme Espagnol* (1976), is the work of a scholar famous for his many contributions to Spain's economic and social history

during the sixteenth century. In this volume, however, Bartolomé Bennassar sets for himself the difficult task of delineating the character of the Spanish people during the whole of the Old Regime.

He begins by presenting us with a series of "models," or portraits, of representative Spaniards that illustrate that this was a people whose ideal was the acquisition of fame and fortune with a minimum of effort. Predictably, the list includes a bullfighter and a conquistador while omitting women, merchants, and Catalans. This is followed by an attempt to define "the spatial-temporal frame in which Spaniards lived for three or more centuries" (p. 5) and a series of chapters that examine what Bennassar calls "the time for living." These include discussion of the Spaniards' special blend of intense religiosity and anticlericalism, their renowned distaste for labor and consequent pursuit of leisure, their pleasures with emphasis on their innumerable festivals, endless *tertulias*, and abundant sex, their "passion for honor," and, finally, their "complacent" attitude toward death. The conclusion is an effort to explain what (from a Gallic point of view?) is described as Spain's "cultural backwardness."

This kind of history, interesting and imaginative as it is, is fraught with danger, and Bennassar, who is aware of this, nevertheless manages to stumble into the same traps to which others who have written about national consciousness in historical perspective have fallen prey. Much of the book's evidence, for example, comes from the accounts of Belgian, English, and French travelers—the selection of which, looking at the notes, is highly arbitrary—whose judgments are frequently accepted uncritically. Similarly, the use of trial records of the Inquisition to describe Spaniards' religious attitudes and sexual proclivities makes for interesting reading, but it should also be remembered that the images offered by these documents are colored by the inquisitors' own prejudices as well as those of the persons who sought to make use of this institution. Bennassar also exaggerates the extent to which these records provide for a complete slice of Spanish life. He writes that these documents provide no evidence of feminine homosexuality (p. 207). And no wonder! Such cases were generally directed toward secular, not inquisitorial, justice and were then treated with a severity not at all in line with what he describes as Spaniards' "complacency" toward "sins of the flesh."

Difficulties in the use and interpretation of sources are compounded by those of focus. Essentially, this book is about Castile; Catalonia and the other peripheral "kingdoms" of Spain are neglected, possibly because many of their attitudes and mentalities were very different from those reflected in the author's Castilian "models." Considerable emphasis is also placed on what is described as

Spain's "unchanging milieu," but this only leads to interpretations that exaggerate continuity and underestimate change, especially with reference to the eighteenth century. Equally questionable is Bennassar's tendency to view Spaniards as a race apart, unique in their manners and mores. Much of what he writes about the Spaniards' love of ostentation, for example, applies equally well to Venetians; their "passion for honor" has its parallels among Englishmen in the sixteenth century and Languedocians in the eighteenth; and their attitudes toward death do not appear too far removed from those that Philippe Ariès has recently outlined for Europe.

Such criticisms, however, are mostly the product of my own attitudes (and prejudices) about Spaniards of this epoch. Bennassar has written a very personal book, and the vision he provides of Spain is that of a scholar with deep admiration and respect for, not to mention knowledge of, his subject. Benjamin Keen's translation, moreover, captures the intense, personal style in which the essay is written, although he probably goes a bit far when he repeats some of the author's original mistakes. Even so, this is a book that adds immeasurably to our knowledge about Spaniards' everyday life. At the same time, however, it reminds us that much more about this subject remains to be discovered.

RICHARD L. KAGAN
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JUAN JOSÉ CASTILLO. *Propietarios muy pobres: Sobre la subordinación política del pequeño campesino en España (La Confederación Nacional Católico-Agraria, 1917-1942)*. (Serie Estudios. Ministerio de Agricultura Secretaría General Técnica.) Madrid: Servicio de Publicaciones Agrarias. 1979. Pp. xxii, 552. 450 ptas.

This work focuses on one of the more problematical social classes in contemporary Spain, the small peasant proprietors of Castile and the North. Juan José Castillo is primarily a sociologist, but he has written a creditable history of the efforts of the Catholic Church in the early twentieth century to organize peasant proprietors into multiclass *sindicatos* designed to save them both from the impersonal forces of capitalism and from radicalization by the left. The instrumentality for this was the National Agrarian—Catholic Confederation (CNCA), and Castillo's book is basically a history of that organization from its founding in Madrid in April 1917 to its integration into Franco's corporativist state after the Civil War.

The author's conceptual framework is only moderately Marxist, but he incorporates into his work, with apparent approval, a great number of quotations taken from writers of rather more pronounced orthodoxy who subscribe to various timeworn ideas not excluding the notion of "fascism" as the "final

stage" of capitalism and also as the essential characteristic of the Franco regime. The central problem for the author would appear to be that Castilian and northern peasants did not behave as Marx (for example, in the *Eighteenth Brumaire of Louis Bonaparte*) suggested they should: they did not accept as their "natural ally and leader" the urban proletariat. To the author's continuing bemusement, the peasant proprietors of these regions perversely insisted, over several decades, on identifying not with the propertyless peasants below them but with the landed classes above. Christian ideas formed a powerful ideological cement between the agrarian poor and the rich, and the peasants let themselves be recruited into "vertical" unions that were effectively subservient to the interests of the large landowners. This recruitment, as the author says, was a triumph for Catholic social action. But the main feat of the CNCA was the pursuit, over many years, of policies that benefited the great proprietors while being carried out with the aid of the masses of small peasants. The ultimate "payoff" was that the small peasants of the Center and North went over almost en masse to the Nationalist cause when the Civil War came.

All of this—although faithfully chronicled by Castillo—appears to violate his belief in the "materialist conception of history," and he is never quite able to reconcile himself to the apparent non-primacy, in this case, of economic and class considerations, which may explain the diffuse and not especially illuminating character of his concluding section. Nevertheless, a great deal of work has gone into this substantial monograph. As the first extended essay on the subject of the CNCA, it is a useful and, within the limits suggested, scholarly contribution to our understanding of Catholic social action in Spain and its bearing on the origins and outcome of the Civil War.

GERALD H. MEAKER
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FRANCIS BALACE. *La Belgique et la Guerre de Sécession, 1861-1865: Étude diplomatique*. In two volumes. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège, number 226.) Paris: Société d'Édition "Les Belles Lettres." 1979. Pp. lxxix, 638. 160 fr.

Study of the foreign policy of Belgium involves close examination of the diplomacy of the small state's kings as well as that of the foreign ministry. Although the two often supplemented each other, they sometimes diverged significantly. Francis Balace demonstrates how great this divergence was during the American Civil War, how dangerous it

was for Belgium and for the Northern states, and how skillfully Leopold I concealed his activities.

Motivated primarily by a desire to protect the tentative Mexican empire of his son-in-law, Maximilian, Leopold worked to persuade Britain and France to grant diplomatic recognition to the Confederacy. Permanent division of the United States would, he believed, weaken the support given to Maximilian's opponents from the North, whether in the name of the Monroe Doctrine or republicanism. When foiled by British reluctance, which had been stimulated by the Confederates' lack of success at Antietam, Leopold schemed to arrange European mediation that, he intended, would benefit the Confederate cause.

The official position of Belgian Foreign Minister Charles Rogier, who was not on the best terms with the king, was one of neutrality. This posture was consonant with Belgium's international status and Rogier's desire for Union assistance in Belgium's capitalization of the Scheldt tolls. It was adhered to despite the cotton and industrial crisis that, Balace suggests, may have been aggravated by overproduction in preceding years. Complications did result from purchases of arms by the contending factions and Union recruitment of mercenaries. Balace describes well how Leopold publicly appeared not to differ with his minister's policy and even received credit for calming the British during the *Trent* affair, when he was actually agitating for Britain to take belligerent action. Equally impressive are the sketches of the Union representative in Belgium (and head of the Union secret service), Henry S. Sanford, and of the even more vain Confederate representative, Ambrose D. Mann.

This thorough account will be definitive unless long-missing sources are discovered. The author's judgment is sound, the writing clear, the narrative well paced. Detailed documentation including extensive quotations is given in multiple footnotes. These provide a fine guide to the literature of the subject and insight into the thoughts and actions of the participants. Special mention must be made of the exhaustive bibliography and of the valuable essay on the location and history of the numerous primary sources consulted by the author on both sides of the Atlantic.

J. E. HELMREICH
Allegheny College

H. VAN RIEL. *Geschiedenis der Provinciale Staten van Zuid-Holland, 1850-1914* [History of the Provincial Estates of South Holland, 1850-1914]. 's-Gravenhage: Provinciaal Bestuur van Zuid-Holland. 1979. Pp. 524.

This volume is divided into three parts, each of which is subdivided into chapters. Part 1 purports

to present the genesis and legal functions of the provincial government of South Holland. *Inter alia*, it tries to discuss the separation of South Holland from the Province of Holland in 1840, the social and economic conditions, and the electoral system of 1850. Part 2 is entitled "Balance" and part 3 "The Political Development of the Provincial Government, 1850-1914." The latter treats, or attempts to treat, such subjects as political events and a variety of other ill-defined problems. There is obviously a need to write a history of the various Netherlands provinces, but this volume will not serve as a prototype. At best it is a *mélange*, if not jumble, of data, anecdotes, quotations, and so forth. Much of the factual material is not without interest or significance, but no effort has been made to give it any degree of cohesion, organization, or structure. Instead, readers have to use their own ingenuity to extract from these pages some meaningful data.

Whatever this volume purports to be, it is most definitely not, as the title might suggest, a history of the provincial estates of South Holland. There is little information on the political events, debates, parties, or factions in the provincial assembly or the role the assembly played in the evolution of the province in the nineteenth century. Perhaps one of the more interesting chapters is the discussion on the creation of the province and the debates on the issue of dividing the once mighty province of Holland into two entities. The author's presentation, however, consists mostly of a string of quotations, and we learn little, if anything, about the reasons behind the decision to create the new province.

Of importance and interest are the data on social and economic conditions; from them we may learn something about sanitary and health conditions, taxes, wages, the system of poor relief, and so forth. But much of the data does not always directly relate to the province but to other parts of the country. Furthermore, we never receive a clear picture of the social and economic development of the province between 1850 and 1914. Nowhere do we find a good survey of the development of the important city of Rotterdam or of various leading industries.

In conclusion, the usefulness of this work is very limited. At best it could serve as some kind of reference work from which we can extract information for further study and writing of a real history of the important province of South Holland.

GERLOF D. HOMAN
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PEKKA SUVANTO. *Die deutsche Politik Oxenstiernas und Wallenstein*. (Studia Historica, number 9.) Helsinki: Suomen Historiallinen Seura. 1979. Pp. 201.

Count Axel Oxenstierna, chancellor of Sweden for forty-two years, was one of his country's greatest

public servants. After the death of King Gustavus Adolphus in 1632 he guided Sweden through the minority of Queen Christina. Surprisingly, no full-length biography of Oxenstierna yet exists; the few studies of various facets of his career that are available (by Nils Ahnlund, Johannes Kretzschmar, Walter Struck, and Wilhelm Tham) are dated. The present volume on Oxenstierna's German policies in the years 1632-34, appearing only a few years after Sigmund Goetze's more general study of the chancellor's imperial politics, is a significant and welcome addition. Pekka Suvanto's aim is "to clarify what Oxenstierna regarded as the main goals of Wallenstein and how these influenced his political and military decisions" (p. 10). For his study this Finnish scholar has relied on Oxenstierna's voluminous published correspondence and on unpublished source materials from Stockholm, Dresden, and Merseburg archives.

The result is a very detailed and careful, albeit rather tedious, analysis of the chancellor's German policies in the period between Gustavus Adolphus's death at Lützen and Wallenstein's assassination at Eger. Suvanto's work will not alter our basic understanding of the period or its main antagonists, but it does fill in some of the gaps. The author reiterates the view, developed at greater length in his earlier *Wallenstein und seine Anhänger am Wiener Hof zur Zeit des zweiten Generalrats, 1631-1634* (1963) that the imperial general was neither an idealist nor a megalomaniac but simply a political pragmatist who wished to safeguard the security of his own house. He portrays Oxenstierna as a cool and rational political analyst, careful and controlled in his actions, yet able to act boldly and decisively when the appropriate opportunity presented itself. Although the chancellor sympathized with his coreligionists in Germany, he remained always first and foremost concerned with protecting Swedish security and interests in the Baltic Sea. He distrusted and became increasingly disenchanted with Wallenstein's peace negotiations in 1633. He was equally suspicious of the north German Protestant electors, in particular John George of Saxony and his general, Hans Georg von Arnim, who were seeking to eliminate Sweden's influence in Germany in order to allow Saxony to play a more important role. In dealing with these antagonists Oxenstierna revealed himself as a masterful diplomat who took charge of the very complicated and confused political situation that followed the untimely death of his king.

Suvanto's assessment of Oxenstierna's German policies is a bit too sanguine. Since the author is primarily concerned with relating the chancellor's actions to Wallenstein, he concludes his analysis somewhat prematurely with the assassination of the imperial general in February 1634, when Sweden's position in Germany seemed secure. In spite of his diplomatic talents, however, Oxenstierna was un-

able to prevent the very rapid deterioration of this position in the succeeding months. The battle of Nördlingen, early in September 1634, was a catastrophe that shattered the Swedish position in Germany, which the chancellor had nurtured so carefully during the preceeding two years.

BODO NISCHAN
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MICHAEL TOCH. *Die Nürnberger Mittelschichten im 15. Jahrhundert*. (Schriftenreihe des Stadtarchivs Nürnberg, Nürnberger Werkstücke zur Stadt- und Landesgeschichte, number 26.) Nuremberg: Stadtarchiv Nürnberg. 1978. Pp. liii, 229.

This dissertation, issued as a typescript in the city archive's monograph series, is an examination of the middle and upper social strata in Nuremberg during the fifteenth century. Completed at Erlangen University, presumably under the guidance of Hermann Kellenbenz, Rudolf Endres, and Wolfgang von Stromer, it is an intelligent attempt to provide a concrete structural analysis of the middling groups—essentially, the craftsmen and merchants—traditionally regarded as the core of German urban society. Since the secretive tax system in Nuremberg left no good series of registers with wealth assessments or even exact payments, Michael Toch has sought reasonably solid statistical evidence for economic and social stratification in two different sources, the salt provisions of the 1440s and an uneven group of extraordinary lists from 1497 to 1504 (the best is the Common Penny tax register). Acutely aware of the deficiencies of these sources, Toch is careful and modest in the generalizations he draws from them. The rather elementary quantitative framework is filled in with numerous non-statistical materials, and the entire study is informed by the perspectives of the newer social history as practiced both in Germany and abroad.

In contrast to the unfocused essay by Werner Schultheiss in *Städtische Mittelschichten*, edited by Erich Maschke and Jürgen Sydow (1972), Toch's dissertation is a systematic, problem-oriented analysis of Nuremberg society. Starting with definitions and generalizations that Maschke and others have proposed for towns with guild constitutions, Toch tests them in this major industrial and commercial center with a strong patriciate and no guild influence. The results bring no great surprises but offer a satisfying specificity on issues of wealth and occupational stratification, social status, the functions of minor officeholding, urban topography, rural land-holding, education, marriage patterns, charitable activities, and asocial behavior.

Among the propertied burghers of mid-fifteenth-century Nuremberg, Toch already sees a fairly clear differentiation between a lower middle class (com-

fortable but generally less wealthy handicraft people) and a rich upper middle group of merchants and more entrepreneurial craftsmen. This distinction was even sharper with the elimination of craft elements from the upper middle class by 1500, a development Toch asserts but does not demonstrate or explain clearly. He becomes increasingly concerned with the relationship between non-patrician merchants and those in the political elite; and, unfortunately, with this thorny problem the issue of distinctions between the middle and upper strata gets lost. This is the fascinating but elusive area where class and noneconomic status confuse one another, as Toch knows, but he has allowed it to blur his real focus on middling groups. As the author points to social instability and elite conflicts before the Reformation, the inadequacy of his evidence (like the unimaginative and sometimes unclear presentation of his tables throughout) is especially frustrating. But I do not wish to seem negative: historians far more experienced than Michael Toch or I have left these issues unresolved, and Toch has given us a piece of historical craftsmanship, examples of which have always made the city of Nuremberg justly proud.

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HELMUT NEUHAUS. *Reichstag und Supplikationsausschuss: Ein Beitrag zur Reichsverfassungsgeschichte der ersten Hälfte des 16. Jahrhunderts*. (Schriften zur Verfassungsgeschichte, number 24.) Berlin: Duncker and Humblot. 1977. Pp. 337. DM 88.

This study is not for the general reader. It grew out of a Ph.D. dissertation for the Philipps University at Marburg and retains something of the narrow focus so often characteristic of doctoral works. But for those scholars interested in institutional history, especially that of the German Imperial Diet, it offers interesting insights into the workings of the Reichstag and, by extension, into the corporatist movement of the early sixteenth century.

Helmuth Neuhaus examines a relatively obscure subcommittee of the Reichstag, the *Supplikationsausschuss*, a kind of petitions board that flourished during the reign of Charles V, 1521–55. The board, both in its membership and in the business that it handled, had its groundings in the early sixteenth-century reform movement begun in the Holy Roman Empire by Berthold von Henneberg. Neuhaus suggests that the fortunes of the board reflected the fortunes of the reform movement and thus serve as an index to corporatist strengths and limitations. The suggestion has considerable merit. Unfortunately, so much of Neuhaus's energies go into a description of the board—into blocking out the time

and place of its meetings, determining its membership, and sorting out its various agenda—that the linkages with the reform currents and the parliamentary life of the imperial estates remain tentative.

Certainly the volume of petitions handled by the board indicates that the contemporary Germans, poor as well as rich, sovereign as well as subject, were turning to both the Reichstag and the emperor for judicial, financial, and administrative assistance. By documenting this activity, Neuhaus sharpens considerably our perception of the term “Kaiser und Reich.” We see it now in concrete example as the reformed definition of imperial sovereignty. But what about the constitutional implications of the board’s decisions on these various petitions? After all, the board was but a subcommittee of the Reichstag. Its membership varied in terms of individual estate, but the number remained generally constant at fourteen: six electors, two spiritual princes, two secular princes, one Reich prelate, one Reich count, and two imperial free cities.

Insofar as individual petitions involved constitutional issues, this small group could not legislate; it could only recommend. And there was always the Kaiser, as well as the Reichstag itself and the other imperial institutions like the supreme court, who had a stake in any such constitutional recommendations. Neuhaus develops only a random few of the cases that came before the board and reports that the board handled the petitions in such a way as not to antagonize these other components of imperial authority. But it is not clear to me just how the board could act on any petition of consequence without colliding with these other loci of power. Now that Neuhaus has rescued the board from historical oblivion, we need someone to follow through the various cases and to trace out the actual implementations of the board’s rulings in those instances.

Meanwhile, we have Neuhaus to thank for focusing attention on an institution whose activities and membership demonstrate that the German estates could cross curial boundaries within the Reichstag and work together in a heretofore unappreciated fashion on the direction of imperial administration and government.

JAMES ALLEN VANN
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CHRISTOPHER R. FRIEDRICHS. *Urban Society in an Age of War: Nördlingen, 1580–1720*. Princeton: Princeton University Press. 1979. Pp. xvii, 350. \$22.50.

Nördlingen was a middle-sized imperial city of well under two thousand households, centrally placed north of Augsburg on the main trade routes out of

the Upper Danube Valley. Its economy was already stagnating before 1620. The Thirty Years’ War brought heavy inflation, arbitrary taxation, called *Anlagen*, and a reduction by one-half of the total number of citizen households. Recovery was remarkably swift in the two decades after the Peace of Westphalia. From the 1670s, war costs against the French and Turks imposed renewed *Anlagen* paid to the Swabian circle and Habsburg emperors. These burdens were heavier and more damaging than had been in their time the impositions of the Thirty Years’ War. Citizen craftsmen working with wool, leather, and flax were hit by local and regional stagnation of markets and subjected to the new capitalist competition of the putting-out industry, organized by unscrupulous merchants, emerging from the wool-weavers’ trade to flout the rules alike of mercantilist craft and of city council. It led to riots when combined with poor harvest and high cereal prices in the 1690s.

The introduction and chapter 1 locate civic politics and constitution within the town’s region, meager *contado*, and wider economy. Chapter 2 deals with the massive disruption to demographic growth caused by the 1630s, while chapter 3 shows how textiles and long-distance trade were damaged by war, leading to heavier investment in local vicualing industries. Chapter 4 is an excellent archival study of wealth mobility, making skillful use also of well-known price and wage data from Augsburg to arrive at indexes of real values. Full tabulation is provided through appendixes of marriages, baptisms, burials, immigration, occupation, and mean, median, and total wealth, set against the wealth mobility of various fractions of Nördlingen’s productive citizenry and backed by many tables, graphs, and figures in the text. Chapters 5 to 8 deal with the mechanism of social control available to the city council, its treasury, church schools, and hospital, whereby the divine retribution of Lutheran paternalism was seen to hold the town together. Vivid vignettes are provided in the correspondence of a mother of six to her husband while she was waiting to be tortured and burned as a witch in 1590, as well as in the various visitations to the town’s six elementary German schools. Chapter 9 analyzes the Wörner family, whose new business methods led to modernity via strong opposition from a craft-centered council and citizenry.

Christopher R. Friedrichs provides a convincing study of urban development in seventeenth-century western Germany by demonstrating from the facts that the share of wealth held by the top 10 percent of the citizenry, relative to the rest, always increased at times of war and that the period of sustained peace in the 1650s and 1660s was the only occasion when wealth mobility favored poorer citizens. His thoroughly researched book draws Nördlingen into

the seventeenth-century crisis debate, and it moves that "crisis" from the 1640s into the latter half of the century by linking it to the rise of capitalism.

GERHARD BENECKE

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KARSTEN RUPPERT. *Die kaiserliche Politik auf dem Westfälischen Friedenskongress (1643-1648)*. (Schriftenreihe der Vereinigung zur Erforschung der Neueren Geschichte e. V., number 10.) Münster: Aschendorff. 1979. Pp. 438. DM 98.

Since the publication in 1959 of Fritz Dickmann's *Der Westfälische Frieden*, there has been a burst of interest and activity in studying the first great diplomatic congress of modern times. In addition to the impressive, though as yet incomplete, documentary publications of the *Acta Pacis Westphalicae*, there have been a number of monographic studies. Karsten Ruppert's work fits neatly into this scholarship and significantly augments it. His work on the theme arose as part of the project to edit the imperial correspondence for the *Acta*. The book is a revised version of a Bonn dissertation completed under Konrad Repgen. The study traces in close detail the emperor's diplomacy in Germany and at the peace congress from 1643 to 1648. It has all the virtues of classic diplomatic history: it is thorough, painstaking, virtually letter by letter in its progress. It clarifies the formation of Austrian policy, which went into crucial memoranda. It analyzes the political content of particular documents and indicates the byplay between court advisors in Vienna and the imperial envoys at Münster, especially the somewhat independent stance of the chief imperial negotiator, Count Maximilian von Trauttmansdorff. Of necessity, this detailed presentation makes for heavy reading, and the book is of much more value to the specialist in the areas of diplomatic history and Austro-German history of the period than the general history reader.

Ruppert has based his work primarily on the riches of the Vienna Haus- Hof- und Staatsarchiv but, in addition, has used for the first time the private papers of Trauttmansdorff, to which Dickmann was denied access. A close comparison of Dickmann's work with Ruppert's would seem to confirm the latter's conclusion that aside from personal detail and some anecdotal material, the private papers do not add substantially to the diplomatic papers in the state archive. Ruppert has also used materials in Munich, the Hague, Paris, the Vatican, and a few other places.

Particularly interesting are the careful development by Ruppert of the Habsburg policy on the cession of Alsace to France, which breaks new ground, and his conclusion that the last winter of

the negotiations, in 1647-48, was a decisive moment in the emergence of the princes collectively, rather than the emperor, as the chief negotiator for the Reich.

This is an excellent monographic study, which will serve not only as a guide to the forthcoming imperial correspondence in the *Acta* but also will be very useful for students of the Peace of Westphalia.

ROGER WINES

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PETER BEYER. *Leipzig und die Anfänge des deutschen Eisenbahnbaus: Die Strecke nach Magdeburg als zweitälteste deutsche Fernverbindung und das Ringen der Kaufleute um ihr Entstehen, 1829-1840*. (Abhandlungen zur Handels- und Sozialgeschichte, number 17.) Weimar: Hermann Böhlaus Nachfolger. 1978. Pp. 203. 21.00 M.

It is easy for the reviewer to criticize Peter Beyer's book because so much of the work is dominated by the ideological themes of Marxism. The book was written within the framework of this vision and makes continuous note of it as it follows the pronouncements of the collected works of Marx and Engels. Another bow is given regularly to the work of Hans Mottek, whose *Wirtschaftsgeschichte Deutschlands: Ein Grundriss* (1976) appears to have had a seminal influence upon Beyer. In chapter 2 of Beyer's book, for example, Marx and Engels are cited in twenty-eight of seventy-one footnotes, Mottek in eight.

Hardly a paragraph of this fairly interesting book is free of these conceptual bindings, making it difficult for the reader to evaluate the documentary evidence. The introduction and first two chapters are essentially a review of ideology in the framework of railway history. Later chapters examine the development of interest in a Leipzig-Magdeburg route, the influence of Leipzig merchants, the relationship between rail and water transport, and some of the diplomatic problems that arose when independent states, railway companies, and individual entrepreneurs had to deal with one another.

There are a number of worthy points. Friedrich List's influence in Saxony is developed in some detail, especially his connection with the Leipzig bourgeoisie. Leipzig itself appears to have struggled to attain a significant place in early railway development, and the Saxon middle classes, heretofore obscure in the literature available in this country, are revealed.

Of some value for railway historians is the list of archives and documentary collections consulted. This is revealing, in part, of the paucity of material available to scholars who are forced to limit their research to the DDR. As useful as their archives ob-

viously are, there are omissions that could have been corrected partially if the Eisenbahn Archiv in Nuremberg and the Haus- Hof- und Staatsarchiv in Vienna could have been consulted. One or two works published in the West are listed in the bibliography, including ones by Carlo Cipolla and W. O. Henderson, but little is found of the studies done in the Federal Republic of Germany during the 1950s, 60s, and 70s. This is not a work that every German specialist will wish to read. For most it will remain on the reference shelf.

DONALD S. HOFFMAN
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RAINER S. ELKAR. *Junges Deutschland in polemischen Zeitalter: Das schleswig-holsteinische Bildungsbürgertum in der ersten Hälfte des 19. Jahrhunderts; Zur Bildungsrekrutierung und politischen Sozialisation.* Düsseldorf: Droste Verlag. 1979. Pp. x, 597. DM 68.

The political history of Germany in the early nineteenth century owes much of its originality to the disproportionate participation in political movements of the *Bildungsbürgertum*—the teachers and professors, lawyers and officials, and other graduates of the classical *Gelehrtenschulen* and universities of the German states. The political importance of these groups during the Vormärz has long been noted, but historians have paid little attention to the problem of how the *Bildungsbürgertum* became an active political group. Rainer S. Elkar's study contributes to an understanding of this development by exploring the process of political socialization among the educated classes in one region of Germany—Schleswig-Holstein—in the first half of the nineteenth century.

Elkar's contribution to educational history is impressive. His straightforward analysis of quantitative data linking educational trends with broad structural changes produces some suggestive insights. He finds, for example, a lack of correlation between statewide levels of enrollment in higher education and both price series and population growth patterns, suggesting that the relationship between education and economic development was not as direct as was once suspected. He also finds that in this era of stagnating or even declining enrollments in higher educational institutions, social recruitment of the student body narrowed, resulting in a more pronounced "self-recruitment" of the educated professions and a more limited opportunity for social mobility through education. Although these findings are relevant for any discussion of the interests and political leanings of the educated, their implications are left largely unexplored.

Elkar rests his argument more heavily on the use

of sources that depict the inner world of the schools. He analyzes not only the official records pertaining to curriculum, conduct of classes, and discipline but also the extant records on student culture—minutes of student associations, lists of topics chosen for addresses and essays, student albums and memoirs. In fact, the tension between the official world of the school and the world of student culture is in Elkar's view one of the most interesting and dynamic aspects of student life in the Vormärz. Student efforts to create an autonomous intellectual life as a counterpoint to the repressive and stagnant official education produced a certain, if contained, capacity for intellectual innovation and even rebelliousness. The paradox of contained opposition is, of course, viewed as an important political lesson for the students, formative of their later political stance as well.

Throughout the study Elkar points to the peculiarities of the setting for his study. Schleswig-Holstein was economically backward, politically dependent, intellectually spellbound by the Enlightenment, and thoroughly Lutheran. Peculiar, too, was the version of liberalism espoused by the middle classes—more moderate, *Stand*-oriented, fearful of revolution than elsewhere. Similarly, the democracy favored elsewhere found little support in the duchies. Elkar would like to argue that there are connections between the backwardness of the economy of Schleswig-Holstein, the intellectual character of its schools, and the nature of its political life. But the effort to make specific links between educational and political history fails in the end. By nature a comparative argument, it requires analyses of other regions so that what is peculiar to this region can be sorted out from what was generally true. It would be surprising to discover that school discipline, for example, was vastly different in other German states, despite admittedly varying degrees of receptivity to curricular innovation. Yet other political tendencies did emerge in middle-class milieus elsewhere. Comparison would certainly help to make the case more clearly.

Part of the problem lies as well in the failure to deal seriously with influences outside the school. Although Elkar discusses both familial socialization and structural change, he spends little time on the changing interests and strategies of middle-class families as they coped with a changing world. The structural suggestions of the early chapters are never integrated into the intellectual and political arguments made later on. But as it stands, Elkar's study provides an important contribution to the intellectual history of the German middle classes and some insights into the political formation of the *Bildungsbürgertum* of Vormärz Germany.

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TONI OFFERMANN. *Arbeiterbewegung und liberales Bürgertum in Deutschland, 1850-1863*. (Forschungsinstitut der Friedrich-Ebert-Stiftung, Politik- und Gesellschaftsgeschichte, number 5.) Bonn: Verlag Neue Gesellschaft. 1979. Pp. 623. DM 108.

After Prussian troops had dispersed the last German rebels in 1849, there were still workers' clubs about. Some were of democrats, some were printers' and tobacco workers' unions, some only social groups. Toni Offermann has the most precise information on how the dozens of German states used the laws on the books to destroy these tag ends of revolution.

Politics resumed slowly in the fifties, the bourgeoisie being first back in the field. Both its Progressive Party and National Association had plans for laboring men. They were to be educated through lectures and courses so that they might be brought to a higher cultural and, with luck, economic level. But they were not to engage in political discussion, which did not concern them, and, above all, they were not to have anything to do with socialism and communism. An anxiety about such doctrines was already present on the German scene. Offermann provides the fullest set of facts on these educational societies with their bourgeois tutors and artisan pupils, including tables on the occupations of the participants.

Those who joined were unlikely to have been independent craftsmen, attached as the latter were to a vision of the past. Nor were they unskilled day laborers, who had little to gain from increased literacy. Rather, the few thousands who filled the ranks were skilled workers, shaken by having crossed the threshold of the factory and thereby having lost their freedom.

The middle-class politicians expected to keep their working-class children in separate cradles. Blue collars were not to discuss economic problems, politics were taboo, and men from different states were not to meet in national convention. This well-documented but patronizing effort was of short success (1859-62). Workers in Hamburg and Leipzig soon tried to start something that would be more distinctly their own and not subject to mentorship. If their efforts were modest and halting, they did persevere. They wrote to the forceful, colorful, and quite erratic Ferdinand Lassalle, and he tried to form a party from these disparate clubs of the politically alert laboring men.

Offermann's finest work lies in the detail on these early proletarian groups. No one will have to go down this road again. But when he gets to 1862, he argues the indispensability of Lassalle and helps his case by not providing a sufficient national or international context for the increasing working-class agitation in the 1860s. The heroic nature of Lassalle is an old Social Democratic fable, echoed by his most recent biographer, Na'amann, who is multi-

tudiously cited here. But on Offermann's own evidence, these political neophyte artisans were making good progress. With Lassalle added to their cause for a few months, they did not make much more.

This is organizational history and of no insignificant moment in German history. For these are the years bourgeois and proletarian functioned together in politics. Once they separated, they were not to come together again, and the split was fateful.

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ADOLF NOLL. *Sozio-ökonomischer Strukturwandel des Handwerks in der zweiten Phase der Industrialisierung, unter besonderer Berücksichtigung der Regierungsbezirke Arnsberg und Münster*. (Studien zum Wandel von Gesellschaft und Bildung im Neunzehnten Jahrhundert, number 10.) Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 386. DM 62.

In 1848, the Communist Manifesto asserted that "the lower strata of the middle class—the small tradespeople, shopkeepers, and retired tradesmen generally, the handicraftsmen and peasants—all these sink gradually into the proletariat, partly because their diminutive capital does not suffice for the scale on which Modern Industry is carried on; . . . partly because their specialized skill is rendered worthless by new methods of production." Known as the *Niedergangsthese*, this postulate has haunted generations of German historians, most of whom have managed to find "incontrovertible" evidence in support of the Marxian theories of capitalistic accumulation and concentration and the corollary decline of the artisans and their crafts.

Now Adolf Noll, a student of Wolfram Fischer, has re-examined the manifesto's proposition within the context of the "second phase of industrialization," that period of profound structural change in Germany's economy and society between 1870 and 1914. Since aggregate data on such economic variables as employment, income, and capital stock are lacking, researchers from Schmoller on have generally relied on demographic analyses to supplement impressionistic studies of the artisans' plight. Noll, however, has devised an alternate approach in which he investigates "the exogenous and endogenous causes of the economic changes that affected small business (as he defines *Handwerk*) and the social, political, and ideological effects of these changes on small business itself" (p. 35). His testing grounds are the districts of Arnsberg and Münster, representative of Germany insofar as the former exemplifies a rapidly industrializing area, while the

latter stands for an agricultural, preindustrial region.

It is a worthwhile departure from the traditional method. Noll not only manages to generate nearly two hundred pages—more than half the book—of quantitative and qualitative evidence; he also arrives at some significant insights into the reaction of small business to industrialization. Thus Noll finds that the *Handwerker* adapted rationally and successfully to the changing economic structure; their response to more capital-intensive competition, for instance, was to increase their productivity and raise their capital stock. But ideologically, small business tended to hang back, continuing to see itself as the loyal intermediary between the crown, the capitalists, and the “*vaterlandslose Gesellen*.” The reason, Noll argues, must be sought in the irrational self-image of the *Handwerker* that rejected the dynamism of an industrial system as alien to their collectivist world.

But, above all, Noll provides a stimulating example of what the “new” economic history can do in a situation that is as hard to quantify as it is easy to forejudge. Prefigured in Rosenberg’s study of the “great depression,” the heuristic use of economic variables to explain noneconomic tendencies and structures in society has been widened here into a *Korrespondenzprinzip* designed to uncover the mutual influences and interactions at work within a social class. It is an approach that deserves to be emulated much more widely in dealing with the convolutions of history as much as the paucity of evidence that we so often have to face.

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WOODRUFF D. SMITH. *The German Colonial Empire*. Chapel Hill: University of North Carolina Press. 1978. Pp. xiii, 274. \$16.00.

Recent research on late nineteenth-century European overseas expansion has begun to bear fruit in the form of general surveys and collections of essays also on German colonial history. Rudolf von Albertini’s pioneering comparative study of European colonial rule overseas from 1880 to 1940 (1976) was immediately followed by three other extensive publications: a symposium on *Germany in the Pacific and Far East, 1870–1914*, edited by Paul M. Kennedy and John A. Moses (1977); Lewis H. Gann and Peter Duignan’s Euro-African study on *The Rulers of German Africa, 1884–1914* (1977); and the East German symposium on Germany’s colonial expansion in Africa, edited by Helmuth Stoecker (1977). Unfortunately none of these volumes appears in Woodruff D. Smith’s bibliography. So this new general

study on German colonial history, while praiseworthy, exhibits some shortcomings.

It is hazardous, moreover, to write such a study without first evaluating the records of the Imperial Colonial Office. These records are presently located in the central archive of the German Democratic Republic at Potsdam and have been used by many Western historians. Unfortunately Smith was compelled to rely on an incomplete microfilm collection of those documents found at the Hoover Institute in Palo Alto. Otherwise, Smith’s interpretation rests upon several microfilms available in the Library of Congress and the National Archives, a few documents published by the German imperial government, a number of publications (too few unfortunately) from the contemporary discussion of colonialism in imperial Germany, and, most extensively of all, the numerous works on German colonial history that have appeared in the last fifteen years as a result of intensive research by other scholars in the colonial office records. This is not to say that Smith’s work should be dismissed as merely secondhand historiography. Once the body of research has reached a point where the nonspecialist is in danger of losing his general view of the subject matter, comprehensive studies with independent lines of interpretation become necessary. The new research on German colonial history offers a superb basis for such a general history despite the fact that many questions still remain to be answered. This basis would have been more secure, however, if Smith’s bibliography, which lags approximately four years behind the current level of discussion, did not contain many gaps.

Nevertheless, such shortcomings reduce only marginally the value of this important and necessary study. Although it omits some central aspects, this is the first survey of Germany’s colonial empire since that by M. E. Townsend, published in 1930. During the last decade and a half, research on German colonial history has had two main concerns: (1) the domestic, socioeconomic, and ideological origins and problems of German imperialism and (2) the economic and social history of the colonies themselves. The focal point of Smith’s study is clearly the first of these concerns. It is more a history of German colonial politics than of the German colonial empire overseas. Smith deals with this task admirably by providing us with useful guidelines for orientation as well as fruitful new challenges. His book does not, however, satisfy the need for a comprehensive study of imperial Germany’s colonial history based on all available archival records and encompassing all of the economic, social, political, and ideological aspects of the problem both in Germany and overseas.

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E. ROSENBAUM and A. J. SHERMAN. *M. M. Warburg & Co., 1798-1938: Merchant Bankers of Hamburg*. Translated by A. J. SHERMAN. New York: Holmes and Meier. 1979. Pp. xiii, 190. \$24.50.

M. M. Warburg and Company opened its doors in Hamburg on the first day of 1798, and almost two hundred years later it still flourishes. If the proverbial caution of generations of family proprietors ensured that the bank would never become a leading financial institution in Germany, it also enabled their concern to take in stride the competition of larger banks and a succession of crises in the marketplace. For its first hundred years, the firm was a modest concern, with the partners living over the bank, and as late in 1898 it had only thirty-six employees. But M. M. Warburg and Company enjoyed a solid reputation and served as the Hamburg agent for houses as powerful as the Rothschilds. Like the Habsburgs, the Warburgs often married strategically, and their nuptial tentacles embraced the Henriques clan in Copenhagen, the Frankfurt Oppenheims, the Gunzburgs in St. Petersburg, and the Kuhns and Loeb in New York. Until 1900, the firm's principal business lay in currency transactions and bill brokerage, but thereafter it organized or participated in large international issues. Max Warburg, the guiding spirit in the firm from 1893 until 1938, and his partners began to assume director's seats in corporations to which the bank rendered financial services. M. M. Warburg and Company's sound investment policy enabled it to survive the crippling inflation and speculative adventures of the 1920s. By his own admission, Max Warburg failed to appreciate the gravity of Hitler's accession to power in 1933. Almost at once, Warburg representatives were forced off corporate boards and deserted by old clients. In 1938, after valiantly struggling to keep the bank afloat and helping less influential Jews to emigrate, Max Warburg went into exile in the United States, where he died in 1946. The bank reorganized as Brinckmann, Wirtz and Company, returned to Warburg control in 1956, and in 1970 reassumed the family name.

In 1922, to mark the forthcoming one hundred and twenty-fifth anniversary of the firm, Max Warburg commissioned his friend Eduard Rosenbaum to write a history of the bank. The financial crisis put an end to the project, which Rosenbaum resumed fifty years later with the support of Max Warburg's son, Eric. Rosenbaum was able to take the narrative only to 1889 and entrusted the subsequent history of the firm to A. J. Sherman. Rosenbaum's contribution is urbane discursive, while Sherman, to whose portion falls the meat of the story, is tighter but thinly spread. The period from 1900 to 1933, when the bank was a financial institution of some prominence with important political

connections, receives but a scant sixty pages, much of it suggestive, all of it undeveloped. This abbreviated account cannot accomplish more than to whet the appetite for a more thorough treatment of the bank's long history. At the least, the publication of the complete version of Max Warburg's memoirs, from which his *Aus meinen Aufzeichnungen* (1952) was culled, would be very welcome. A professional study of M. M. Warburg and Company, with a full depiction of its business transactions, corporate connections, and political ties would be the appropriate monument to a dynasty remarkable both for durability as well as accomplishment.

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ALFRED D. LOW. *Jews in the Eyes of the Germans: From the Enlightenment to Imperial Germany*. Philadelphia: Institute for the Study of Human Issues. 1979. Pp. x, 509. \$17.50.

This book has its beginnings in a manuscript that Alfred D. Low wrote in Vienna during the late thirties. Its purpose at that time was to take issue with the claim of Nazi writers that most great Germans had been enemies of the Jews. Before the work could be published, however, the Nazis annexed Austria, and the author fled with the manuscript to America. Twice revised and rewritten in English, it has now appeared after more than forty years.

In certain respects the book still reflects its origins. The residue of a defensive, sometimes polemical, tone remains in some of the chapters—as if it were still necessary to rescue Goethe, Schiller, or Nietzsche from their Nazi perverters. At some points the author seems as intent on making excuses for the antisemitic aberrations of leading figures as on explaining their views. But in general, and especially in the foreword and the concluding chapter, a more sober assessment prevails, the apparent result of a very different perspective gained by living after the Holocaust and in a pluralistic society.

Taking in both Germany and Austria and spanning a century and a half from the Enlightenment to the year 1890, Low deals with scores of prominent and not so prominent individuals representing the political, intellectual, and literary elites. Scarcely a figure of the first rank is missing as virtually everyone had something to say about Judaism and the Jews. Their assessments, of course, varied widely. Some liked Jews but not Judaism, some only particular Jews but not Jews in general, some ancient Jews but not modern ones, some only orthodox Jews or only those who resisted conversion. Others had the opposite preferences. It was thus im-

possible for German Jews to find favor in everyone's eyes no matter what they did. Unfortunately, Low does not tell us a great deal about how Jews responded to the judgments that were passed on them, although the other side of the equation—Germans in the eyes of the Jews—is obviously integral to the larger picture of the German-Jewish relationship.

In assembling affirmations of Jewishness, Low might have been more careful to distinguish between those figures who genuinely appreciated Jews as Jews and others whose regard was limited to Jews who transcended their origins. A large proportion of the personal relationships that he discusses involved Jews who were either converts to Christianity or at least wholly alienated from any form of positive Jewish expression.

Despite these shortcomings, Low's book is a valuable compendium of views on an issue that was by no means peripheral to German thought and creativity. Judgments passed on Jews and Judaism were symptomatic of more general thought patterns and political ideals. Drawing upon an impressive array of primary sources and using the large literature of studies on the relationship of individual figures to the Jews, Low documents a remarkable range of attitudes, feelings, and literary images. Rarely did the Germans he examines reach the genuine humanitarianism of a Lessing, but neither did any but a very few even approximate the consistent hatred of the Nazis. Low succeeds in showing that, for all of their prejudices with regard to Jews, Judaism, or both, men such as Frederick the Great, Metternich, and even Fichte, cannot be seen as foreshadowing the ideology of the Third Reich. The principal antecedents of unambiguous, unqualified antisemitism, one must conclude, lie elsewhere than in the thinking and actions of the German and Austrian elites.

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HEINRICH POTTHOFF. *Gewerkschaften und Politik zwischen Revolution und Inflation*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 66.) Düsseldorf: Droste Verlag. 1979. Pp. 504. DM 84.

This important study argues that Germany's socialist unions failed to develop into forceful allies of the Weimar Republic because of political behavior patterns established between 1918 and 1923. Less encompassing than the title suggests, it focuses on the General Federation of German Trade Unions (ADGB) and seeks to explain why Germany's largest trade union federation missed the opportunity to become a decisive bulwark of democracy. War and

revolution, Heinrich Potthoff notes, opened new fields of political activity for organized labor. But Carl Legien and the Social Democratic leadership of the ADGB, fearful of losing social and economic advances made during the war and overburdened with organization duties, concentrated on preserving the status quo. This defensive posture prevented the ADGB from recognizing and exploiting the dimensions of political activity created by the Weimar Republic. As a result, union leaders silenced local demands for sweeping social and political change by centralizing authority in their own hands and drastically curtailing internal union democracy.

In foreign affairs, a field of responsibility for which the unions were ill equipped, the ADGB loyally implemented government policies without seriously considering the consequences. Domestic politics offered a more familiar battleground, but here, too, the ADGB did not capitalize on the opportunities within the Social Democratic Party, parliament, and state bureaucracy. Sharp political divisions within the unions, traditional aversions to politics, and mounting public opposition to trade union influence discouraged political initiatives. At times of crisis, such as the Kapp Putsch, the socialist unions did intervene for the republic. Yet no coherent political strategy developed out of these emergency actions. By the time a program had begun to take shape, mounting inflation undercut union confidence and led ADGB leaders to rely on the government and the SPD for political guidance. The pattern of inactivity and indecision visible by 1923 continued throughout the 1920s and seriously handicapped socialist labor's defense of democracy at the end of the Weimar Republic.

Potthoff's skillful analysis illuminates the organizational bottlenecks, missed opportunities, and short-sighted pragmatic orientation that produced a pattern of passivity. It is organized topically and based on a thorough knowledge of current trade union scholarship, new archival sources, and extensive use of ADGB publications. Important new information on the scope of ADGB participation in foreign affairs is introduced as well as carefully drawn profiles of the 1918–19 membership, the radical opposition within the unions, and trade union representation within the SPD's parliamentary delegations. Closer attention could have been given to the political role of nonsocialist unions since their influence often weighed far heavier than membership figures suggest. The author's emphasis on World War I as the formative experience for early Weimar political attitudes also underestimates the importance of labor's pre-1914 traditions. Nevertheless, within the limits set by the author, this thorough, well-written study provides valuable new insights into the political behavior of Germany's

socialist unions during the early years of the Weimar Republic.

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WILHELM DEIST *et al.* *Ursachen und Voraussetzungen der deutschen Kriegspolitik.* (Das Deutsche Reich und der Zweite Weltkrieg, number 1.) Stuttgart: Deutsche Verlags-Anstalt. 1979. Pp. 764. DM 32.

Popular and scholarly works on Nazi Germany and the Second World War continue to pour forth in dismaying profusion. The book under review, 764 pages in length, is the first of a new ten-volume compendium, *The German Reich and the Second World War*, which is being produced under the auspices of the Office for the Research of Military History in Freiburg-im-Breisgau under the direction of Manfred Messerschmidt. According to the producers of this series, their work will be far more than a military history and will include a full discussion of political and economic developments in Germany and German-occupied Europe, as well as detailed analyses of how the societies of all European countries were affected by the war. The producers promise further that, because their authors have drawn on an immense range of sources, many of them heretofore unused or neglected, their series will contain information and details that will be new to specialists as well as to the general reader. The titles of forthcoming volumes indicate that this series will provide a straightforward chronological account of the course of the war and concomitant problems. An exception to this chronological approach will be volume 5, which will focus on the Nazi war economy and theories of territorial expansion (*Grossraumvorstellungen*).

This first volume of the series deals with the origins of the war and the measures taken by the Nazi government to prepare for it. Part 1 by Wolfram Wette examines militarist and pacifist ideologies in the final years of the Weimar Republic, the propagandistic mobilization for war, and the Nazi reorganization of domestic institutions for war. Part 2 by Hans-Erich Volkmann analyzes Nazi economic preparations for war; part 3 by Wilhelm Deist covers the armament and build-up of the Wehrmacht; while part 4 by Manfred Messerschmidt discusses Nazi foreign policy in the context of preparation for war.

The scholarship of all four authors is impressive, their coverage detailed and comprehensive. They agree that the Nazi government was preparing systematically for war and do not presume to offer radically different or original interpretations of Nazi policy. Neither specialists nor general readers will find nuggets of new information in this book that

are likely to alter their own interpretations of the Nazi era, but they will find brief and authoritative discussions of such problems as the attitude of the German churches, political parties, and labor unions toward war; emigration and resistance; the effectiveness of the Four Year Plan; and the timing of the decision to go to war. To judge from this first volume, this series promises to be a solid comprehensive history, a readable analytical chronicle for insatiable students of the Third Reich, and an encyclopedia of information for others compelled to deal with the subject.

NORMAN RICH
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FRIEDRICH JERCHOW. *Deutschland in der Weltwirtschaft, 1944-1947: Alliierte Deutschland- und Reparationspolitik und die Anfänge der westdeutschen Ausseiwirtschaft.* Düsseldorf: Droste Verlag. 1978. Pp. 512. DM 68.

Several scholars have scrutinized Germany's post-World War II economic recovery as an issue in the Cold War. Friedrich Jerchow's richly documented study presents a refreshing and persuasive interpretation of the same events from a German perspective. Adding massive evidence from the records of German commercial organizations and administrative organs to the usual diplomatic sources, Jerchow analyzes in depth the interplay of forces leading to the revival of West German foreign trade. Since that revival depended on resolution of the reparations question, which in turn depended on the ability of the occupying powers to treat Germany as an economic unit, the analysis touches every major issue of the period.

Because the war left the occupying powers in control of Germany, Jerchow's analysis begins with a definition of their war aims. Using familiar sources, he reaches familiar conclusions: the Soviet Union sought security and economic recovery, Britain a stable European order, and the United States a liberal democratic world. The only novelty is the strength of the determination to regain great power status he attributes to France.

The originality and significance of the work stem from the conflicts among the aims of the occupiers and with actual conditions in Germany explored in the book's second part, aptly labeled "the confrontation with reality." Jerchow synthesizes previous studies of the occupation, the memoirs and papers of occupation officials, and his own extensive research in the records of German trade associations. The results are extremely instructive. The German material reminds us that the occupied had definite ideas about their fate and that these ideas had some influence on the occupiers. In fact, Jerchow contends that the leadership assumed by vari-

ous municipal and economic associations paved the way for the eventual development of a pluralistic society in West Germany. Second, Jerchow's detailed description of the administration of foreign trade in the British, American, and French zones reveals vast differences in both aims and methods. The British dilemma resulting from the need to revive an economy whose exports competed directly with British goods led to early efforts at cooperation between occupier and occupied. The American military government, fearful of lingering Nazi influence, postponed such steps and imposed extensive administrative machinery instead. In spite of the differences, the heavy burden borne by both Britain and the United States supplying food to their zones led to fusion as part of the effort to promote exports to balance the necessary imports. The French zone, on the other hand, proved so profitable to the occupier as to make unification of the western zones economically undesirable even when Soviet conduct made it a virtual political necessity.

Given the interplay of these forces, the eventual emergence of a separate West German state dependent on world trade should occasion little surprise. Jerchow recognizes the arrogance and omnipotence of American influence as well as the intolerable Soviet behavior that left Western Europe no alternative to American dominance. The subtlety of his interpretation, however, which is far more comprehensive than previous studies of these events, shows that all problems are not invented, and certainly not resolved, in either Washington or Moscow.

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ROLF STEININGER. *Deutschland und die Sozialistische Internationale nach dem Zweiten Weltkrieg: Die deutsche Frage, die Internationale und das Problem der Wiederaufnahme der SPD auf den internationalen sozialistischen Konferenzen bis 1951, unter besonderer Berücksichtigung der Labour Party.* (Archiv für Sozialgeschichte, Beihefte, number 7.) Bonn: Verlag Neue Gesellschaft. 1979. Pp. ix, 433. DM 78.

Rolf Steininger begins his excellent monograph on Germany and the Socialist International after the Second World War by examining the roots of that problem in the interwar years. At the Paris Congress of the Labor and Socialist International (LSI) in August 1933, the German socialists were accused of suffering from a paralysis of will at those decisive moments when revolutionary methods were called for. Steininger correctly concludes that the LSI should have pronounced that verdict on itself. But internal splits and a growing retreat from inter-

nationalism made even a serious appraisal of LSI powerlessness impossible.

Following the first spontaneous (not Moscow-inspired) popular front negotiations between French socialists and Communists in 1934, a strong right-wing conspiracy developed in the LSI under the leadership of the British Labour Party. Its aim was to reconstruct the International in order to turn it into a powerless information bureau. Despite heroic warnings by the LSI Secretary, Friedrich Adler, that a second August 1914 capitulation was imminent, the triumvirate of British, Dutch, and Danish socialists had their way. The international working class, bureaucratically excluded from decisions taken at the top, was once again set adrift, disunited and helpless in a world at war.

The fate of socialist refugees in Britain after 1939 was pitiful. Those who were not won over to Labour Party self-aggrandizement, and still strove to give working-class internationalism some meaning, were treated as lunatics and outcasts. William Gillies, secretary of the Labour Party's international department, worked deftly to increase anti-German sentiments in his own party. In the climate of rabid German hatred he helped to create, the Nazi hierarchy and the German working class came to be viewed as the same evil.

Germanophobia loomed large when discussions about the reconstruction of the International began as the war was drawing to a close. Steininger traces the tremendous potency of that particular brand of poison during the postwar years, when the International was being reconstructed. It took a half-dozen years to arrive at an agreement on two difficult problems: how to integrate the German socialist party, whose reputation was tarnished by its past, and how to give a new face to international socialism. The German question was solved when Ollenhauer replaced Schumacher as party leader and when the party earned its anti-Communist spurs in the first years of the Cold War. At the founding congress of the Socialist International in Frankfurt in 1951, the Labour Party's historical preference for pragmatism over theory and suspicion of substantive internationalism played a decisive role. Steininger ends his introduction with the thought: "The history of the LSI is a history of its decline, which began soon after it entered the world-political arena" (p. 10). One might well ask whether the same judgment might not be made about international socialism's third incarnation.

This is a carefully researched and judiciously reasoned monograph, qualities one has come to expect of Friedrich-Ebert-Stiftung publications. Some two hundred pages of pertinent documents enhance the usefulness of the volume.

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KLAUS GÜNTHER. *Sozialdemokratie und Demokratie, 1946-1966: Die SPD und das Problem der Verschränkung innerparteilicher und bundesrepublikanischer Demokratie*. (Forschungsinstitut der Friedrich-Ebert-Stiftung, Politik- und Gesellschaftsgeschichte, number 6.) Bonn: Verlag Neue Gesellschaft. 1979. Pp. 300. DM 46.

German Social Democracy throughout its venerable history has been the focus of innumerable studies ranging from autobiographies by its leaders to analyses of its programs and goals. Undoubtedly, books on this important party, presently the senior governing one in the federal republic, will continue to attract the attention of scholars in the future. Such interest is enhanced as a result of the sponsorship of a number of studies by the Research Institute of the Friedrich Ebert Foundation (in Bonn), closely linked to the Social Democratic Party.

The present volume, falling under these auspices, deals with one crucial time segment, 1946-66, when the organization emerged from the ashes of the Nazi regime and became the chief opposition in the Reichstag to the Christian Democrats. Within this time span, the emphasis is on an analysis of the degree of intraparty democracy within the broader framework of a democratic political system.

In the introductory theoretical section, Klaus Günther distinguishes between the policy-making role played by party leaders (called the representatives) and the passive role of members (the represented) in the numerous groups of the highly structured organization. He seeks to determine the extent of intraparty democracy by examining chiefly the nature and fate of resolutions introduced by lower functionaries at the periodically held conventions. But he also seeks to assess the role of the SPD as an opposition party in the Bundestag to measure further the democratic nature of the German body politic.

In the substantive sections of the book, the author (presently teaching at the University of Bonn) is critical of the leaders' failure to promote discussion and allow for criticism of policies. "Stimulative" initiatives are tolerated, but serious dissent is stifled as leaders constantly seek to maintain unity in order to gain more voter support in state and national elections. Günther pleads for a broadening of a democratic "corridor" in which the rank and file has an opportunity to participate to a greater extent in the formulation of policy and in which the leaders consult the members more frequently. Surprisingly, in the light of his extensive treatment of intraparty democracy (or the lack of it), Robert Michels's iron law of oligarchy is alluded to only once.

As to the relationship of SPD to the German democratic polity, Günther focuses on various phases of the party's role in opposition: from con-

flictive (1946 to 1955), to cooperative (1955 to 1959-60), to adaptive (1959-60 to 1966). From this perspective, he examines the party's position on selected foreign, economic, and social policies and its "fixation" on elections, the role of parliament, and the rule of law as instruments to promote democracy. He concludes that any dissenting voices within the SPD who challenged their leaders' position on these issues had little impact, proving once again that the democratic ethos needed reinforcement.

Although the study is based on a wealth of documents and other archival material, the reviewer wishes that they had been supplanted by interviews with elite and rank-and-file members to provide for a more varied texture and a less jargon-filled text. The book enriches the literature on the SPD but leaves unanswered the question whether the author would stick to his basic thesis of a lack of intraparty democracy if he were to analyze the party from 1966 to the present when major factional and ideological schisms between the young Socialists and the old guard erupted periodically to make life for the leaders more difficult than in the first two postwar decades.

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HANS GEORG LEHMANN. *Der Oder-Neisse-Konflikt*. Munich: Verlag C. H. Beck. 1979. Pp. 294. Paper DM 39.

Hans Georg Lehmann's case study of the Oder-Neisse conflict is actually three studies of this important postwar problem: (1) a political/international-relations-oriented narrative of the sequence of events concerning the western Polish border question from 1939 to 1972 (with emphasis on the decisions made between 1944 and 1950); (2) a brief (pp. 177-83) summary periodization of the turning points in the sequence; and (3) an attempt to reformulate the known facts into a mathematical model of a conflict situation, using the tools of computer language (Fortran) and math-oriented political science model systems including such constructs as the interdependence of variables, feedback, the Boole Function mechanism, and the like. He has more success in realizing his goal, "die Verflechtungen internationaler, aussen- und innenpolitischer Interdependenzen zu erhellen," in the first two instances than in the last.

This is the first case study devoted solely to the Oder-Neisse conflict and as such is of value not only as a consideration of the theme but also as an analysis of how the Cold War affected international questions that had at one point or another been



considered essentially settled (at Yalta and Potsdam) but which were reopened and inflamed as the former Allies in the anti-Hitler coalition broke into competing power blocks, dragging their former German enemy with them.

Lehmann has been unavoidably influenced by the work of the revisionist historians of the Cold War but has stayed away from the various forms of economic determination that informs much of their work. Indeed, most of his book is in essence a well-done synthesis of straight political history. Nonetheless, one of Lehmann's most intriguing points will not please most traditional historians of the period, namely, that much of Soviet behavior and decision making was a reaction to Western, especially American, policy making (such as Byrnes and Marshall's revision of the U.S. position on the Oder-Neisse question taken previously at Yalta and Potsdam) and not an expression of aggressive expansionist tendencies (pp. 136, 142, 203).

One of the more interesting aspects of Lehmann's study is his examination of the effects of policy made by the super powers on Poland: the clear evaluation of Poland's domestic and foreign policy actions to strengthen its western border both legally and in terms of public opinion. He shows how the conflict and its ramifications drove the Poles further into the embrace of the Soviet Union often against their own wishes (see especially p. 158). Lehmann also examines in some detail what the various stages of the conflict meant to the different participants and how those governments manipulated and interpreted the circumstances to channel domestic political conflicts into foreign policy questions with regard to the border problem (pp. 128, 175).

In short, the first part of the book (pp. 15-176) is a well-documented (though the available state department and U.S. military government records were not consulted) evaluation of the subject matter, which is of value to the general reader and the specialist. The value of the third part of the book is more problematical. It is essentially a recapitulation of the first, attempting to explain the same events through the use of graphs, flow charts, tables, and computer language in a phased conflict model. While there is supplementary information and interpretation here not available in part 1, it is often hidden behind the veil of Fortran and the obscurantist text of a historian trying to build a mathematical model. This attempt obscures more than it clarifies, at least for this reviewer. Had Lehmann combined the first and third parts of the book without the model building he would have given us an even more comprehensive work of greater value.

There are also a few relatively minor negative points: a working knowledge of Latin phraseology will be helpful in reading this work; American military government was not trapped by its major directive (JCS 1067) but rather ignored it whenever

necessary (pp. 65-66); and it is questionable whether John F. Kennedy ended the Cold War in 1961 (p. 181).

The book is of considerable value for its clear analysis of the conflict in the first part and may be of use to cliometric historians in its third part.

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ALFRED PERRENOUD. *La population de Genève du seizième au début du dix-neuvième siècle: Étude démographique*. Volume 1, *Structures et mouvements*. (Mémoires et Documents, number 47.) Geneva: Editions Société d'histoire et d'archéologie de Genève; distributed by Librairie A. Jullien, Geneva, and Librairie H. Champion, Paris. 1979. Pp. xv, 611. 48 FR.

Many historians will not share Alfred Perrenoud's view that demographic data provide the key to social history. But students of his work will readily acknowledge the care with which he has selected, explicitly assessed, and analyzed his sources for a general description of the sociodemographic base of Genevan history covering the years 1550 to about 1830. Building on Edouard Mallet's *Recherches* (1837), Perrenoud reconstituted a 10 percent sample of 5,000 families between 1625 and 1780. Notarial records provided particularly valuable information on wealth, occupation, and the geographic and social origin of spouses for a representative cross-section.

In this first of the projected two volumes, Perrenoud intends to reveal population dynamics by isolating factors of growth and decline and by describing the role and structure of the migratory sectors, the composition of households, and socioprofessional patterns; thus "permanencies" are to become visible, "less for retaining what remained than for grasping what evolved" (pp. xi-xii). This is achieved, first, by a numerical overview, then by an investigation of structures, that is, divisions as to sex, marital status, and age, household composition, and occupational stratification. The third, perhaps most unique section focuses on "immigration . . . foundation and fabric of all urban demography" (p. 231), with a special description of the highly volatile alien work force. The last part concentrates on movement: of prices and income; of marriage, birth, and death rates reflecting seasonal, pestilential, and infant mortality. Graphs, lists, tables, and maps throughout the text and also in an extended appendix provide information at a glance.

Perrenoud concludes that for cities "all or almost all" demographic patterns still await discovery (p. 497) but are clearly linked to social structures. The term social is throughout perhaps too narrowly understood as merely involving income, wealth, and

occupation. Perrenoud's further conclusion that "the shock of events" (p. 498) does not truly alter basic patterns needs re-examination in the light of E. William Monter's discovery of a lasting shift from a "transitional" to a "completed" marriage pattern that was initiated by the famine and war of 1586-87 in the countryside and, after 1590, in Geneva proper (*Journal of Interdisciplinary History*, 9 [1979]: 423-25).

As a longitudinal study of evolutionary demographic dynamics in a middle-sized, north European city, undertaken in the tradition of French historical demography, Perrenoud's work is unique, thorough, and outstanding. As history it remains open to challenge. May the human past really be reduced to quantitative aggregates, given the irreducible core of people acting, at least to some degree, in freedom? Only patient inquiry into individual lives—with Pascal's "esprit de finesse," not just "de géométrie," as Jacques Barzun has stressed (*AHR*, 77 [1972]: 54)—will grant glimpses of that reality, perennially central to Clio's domain but truly beyond the grasp of merely numerical methodologies.

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R. J. W. EVANS. *The Making of the Habsburg Monarchy, 1550-1700: An Interpretation*. New York: Clarendon Press of Oxford University Press. 1979. Pp. xxiii, 531. \$49.50.

Following his excellent study of the bizarre world of Rudolf II (1973), R. J. W. Evans has undertaken the enormous task of explaining the emergence of the Austrian Empire and the forces that gave it some measure of coherence in spite of all the contradictions it embraced. He brings to the work an extraordinary command of the sources, Czech, Magyar, Rumanian, and Polish as well as those in the Western languages.

The outline of his thesis, based largely on intellectual currents, can be broadened to cover the social and political developments in Central Europe as well. There was, he argues, a three-stage development. First, the sixteenth century witnessed the gradual emergence of humanist culture in an open atmosphere of social opportunity, a movement that was suddenly interrupted by the Reformation and the onslaught of Habsburg political consolidation. By 1600 the feudal structure of society and the new political system had collided, overlapped, and to some extent coalesced. There then followed a period of crisis, roughly corresponding to the Thirty Years' War, that saw the triumph of the Counter Reformation and at the same time the emergence of a new class of magnate nobles in the three main re-

gions of the Empire. Confirmed in their position rather than created by the dynasty, this largely native magnate class held a virtual monopoly over provincial and regional affairs. This predominance was made all the more effective by their control of ecclesiastical and monastic offices through ties of family and patronage. Finally, there arose in the late seventeenth century an intellectual synthesis, the Catholic culture of the imperial baroque, that offered a set of attitudes and provided the necessary universal abstractions that gave sense and purpose to the whole enterprise.

Evans regards the political, military, and diplomatic events of the period as "epiphenomena" adequately treated in existing literature, and says very little about them. He concentrates instead on formal culture to show how society sustained the dynasty's universalist ambitions. In his analysis, the role of the Church, more Habsburg than papal, is not merely crucial but determinant: "it was Catholic orthodoxy which created Austria" (p. 191). After describing in great detail the magnates' patronage of the Counter Reformation, Evans attempts to anatomize the cultural synthesis this world produced. In the final section, he draws on themes developed in his work on Rudolf II: occult humanism, "official magic," and the persecution of unofficial sorcery. This offers many provocative insights into the thinking of a world seemingly impervious to the revolutionary ideas beginning to take hold in the West, or at least a world very tardy in taking note of them. His contrast between a "rational" West and an "irrational" culture in Central Europe seems to me rather overdrawn. The "enlightened" West had then, and still has, its full share of successful charlatans, wizards, and the like. Habsburg patronage of such types was hardly original or unique, though Evans does make a forceful case for a special interest in them in Leopoldine Austria. In any event, he has unearthed an imposing array of alchemists, astrologers, and eclectic pseudo-orientalists.

Though it is very well written, this is not a book for beginners. The arguments are tightly drawn, the detailed evidence is sometimes intimidating. It is a major contribution that will lift our discussion of the rise of the Habsburg monarchy to a new level of sophistication.

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JOHANN FRANZL. *Ferdinand II.: Kaiser im Zwiespalt der Zeit*. Graz: Verlag Styria. 1978. Pp. 384. DM 36.

Modern biographers, by and large, have not found the Habsburgs to be especially rewarding subjects. Powerfully but conventionally pious, not overly given to imaginative reflection, and only intermittently self-revealing, the dynasty was rarely as

exciting as the events that it helped shape and direct. The result is that any historian who tries to deal with the Habsburgs as human beings ends up by either concentrating on the historical context that surrounds them or, through authorial commentary, investing them with a kind of dramatic quality that most of them lacked.

Johann Franzl's work, which blends serious history with a touch of *Feuilletonistik* in the way Golo Mann did with Wallenstein several years ago, is decidedly of the latter order. His study of Ferdinand II, the devout but occasionally pragmatic mediocrity who presided over the outbreak of the Thirty Years' War and much of what followed, is an adequate recitation of the major events of the Emperor's life. The re-Catholicizing of his native Styria, the Habsburg *Bruderzwist* and his rather ambiguous part in it, the war and its Wallenstein interludes are all there. Franzl attempts to enliven this narrative with his own moralizing reflections on the brutalities associated with religious bigotry and the wars and torments that attended it, all of which were certainly functional aspects of Ferdinand's policies. Curiously enough, the author ends by urging historians not to proceed judgmentally, advice that he is hard put to follow in his own text. The result of his efforts is a book that is interesting enough to read, repetitious and cliché-ridden though it is. It also tells us little about Ferdinand that we did not know before—namely, that he was the dutiful end-product of an upbringing directed by his grimly Catholic Bavarian mother and the Jesuits.

The author's research appears to have been exclusively in published sources; his listing of secondary materials, extensive though not complete, seems to have played little role in his thinking. This may account for the air of superficiality that pervades the entire book. Characters, such as Gustavus Adolphus of Sweden, occasionally appear on the scene with no explanation of their motives. Even granting the limitations on his primary sources, Franzl might have drawn more from them than he did. For example, Ferdinand's lifelong problems with his younger brother, Archduke Leopold, who resented the secondary political status to which his birth consigned him, tell us much about the problems of seventeenth-century dynastic organization. They are not developed here beyond Leopold's role in the quarrel between Emperors Rudolph and Matthias over the Habsburg throne.

It may, however, be just as well that the book is no more heavily documented than it is. Franzl's system of footnoting is so cumbersome that it borders on the perverse. No footnote numbers are used; rather the reader is apparently supposed to notice catch phrases to which further references are attached in the endnotes. Sometimes quotation marks signal that documentation might be there, but

when these do not appear, one has to follow raw intuition. All of this involves a good deal of silly page-flipping; the practice should be discouraged whenever it appears.

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HANS MAGENSCHAB. *Josef II.: Revolutionär von Gottes Gnaden*. Graz: Verlag Styria. 1979. Pp. 300. DM 39.80.

The author of this study informs us that it is his intention to concentrate on the personality of Joseph II, to produce a psychological *cum* spiritual portrait of that complex ruler. He asserts that his book rests on exhaustive research in contemporary documents and in the literature. What he has produced, however, is an amateurish exercise in pseudo-Freudianism with occasional admixtures of wholly undigested structuralist theory. So far as this reviewer can discover, not a single unpublished source has been used by Hans Magenschab, and the sources he does use are, to put it kindly, exiguous. It would, in fact, be no feat at all to construct a wholly adequate working bibliography of Josephinism from the titles that he does not cite in his list of works consulted. (There are no footnotes.) Suffice it to point out that he does not use Mitrofanov, Arneth's *Maria Theresia*, or Valjavec.

The results of such autodidacticism are, predictably, uneven. Where Magenschab follows standard authorities, he is capable of describing an aspect of Joseph's rule succinctly and cogently, as he does with the attempted tax reform or with the catastrophic results of Joseph's Russian policy. More often, though, Magenschab goes ludicrously wrong. His treatments of Francis Stephen and of Maria Theresa are wholly one-sided. Joseph's father emerges as a bumptious and self-indulgent fool, his mother as a bigoted and reactionary virago. The assertion, made repeatedly, that Joseph was a true child of the Enlightenment who meant not only to enthrone philosophy but also to abolish the old order in favor of a bourgeois egalitarian society is just plain silly: whatever Joseph's innermost thoughts on the matter, it is evident that his reliance on bourgeois officials to carry out his reforms represents a means to an end, not an end in itself. Magenschab assumes that Joseph was a revolutionary spirit who occasionally made use of arbitrary methods because he believed they were necessary to ensure the survival of the Habsburg monarchy. In fact, there is good reason to believe that Joseph followed French and Prussian centralist models that placed humane values second to the proper functioning of the state.

As for the highly touted psychological insights,

the method used throughout is one of making a somewhat dubious assumption, then repeating it as if it were a given, and finally deriving conclusions from it. Thus, the author asserts that the relationship of Joseph's much-loved first wife, Isabella of Parma, to his sister Maria Christina was overtly lesbian and that this turned him into a life-long misogynist; that his ambiguous relationship to his father was deeply oedipal; that Joseph, who frequently quarrelled with his mother over matters of policy, actually wished her dead, thus saddling himself with life-long feelings of guilt; and that he was basically a masochist who was always bringing about the failure of his own projects in order to punish himself. Apart from the shaky logical ground that he occupies, Magenschab in arguing these propositions is continually falling prey to the anachronism that the "superior psychological insights that we have achieved" are necessarily valid for eighteenth-century relationships.

Having said all this, it remains to observe that the book abounds in factual errors, of which the description of Joseph in the last year of his life as "an old man of fifty-nine" (p. 56) is merely the most striking.

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KARL VOCELKA. *Verfassung oder Konkordat?: Der publizistische und politische Kampf der österreichischen Liberalen um die Religionsgesetze des Jahres 1868*. (Studien zur Geschichte der Österreichisch-Ungarischen Monarchie, number 17; Schriften des DDr. Franz Josef Mayer-Gunthof-Fonds, number 12.) Vienna: Verlag der Österreichischen Akademie der Wissenschaften. 1978. Pp. 238. DM 50.

Seminars led by Adam Wandruszka on liberalism and the Catholic Church first inspired the author, a specialist in Austrian cultural history in the early modern period and Ottoman-Austrian relations, to begin work on the subject of this monograph. Using parliamentary reports, newspapers, brochures, and other printed materials, Karl Vocolka discusses the implications of the debates over the Religion Laws of 1868 for the German Liberal Party and constitutional government in the Austrian half of the Empire. These laws, moderate rather than radical reforms, were the result of Liberal efforts to reduce the influence of the Catholic Church. The Concordat of 1855, which had accorded the Church powers and privileges forbidden it since the time of Joseph II, was anathema to the Liberals' conception of a constitutional state. However, in 1868 the Liberals succeeded only in breaking the Church's hold on education, enacting a mild civil marriage law, and declaring the equality of all religions.

The debates over the promulgation and passage of the Religion Laws raged simultaneously in Parliament and in the press. Since the Liberal Party had neither a fixed philosophy nor unified adherents, the press played a vital role in formulating a Liberal position. Liberal failure to unite even on issues like the revocation of the Concordat that, as anticlericals, they generally supported, was a major weakness. Another was their lack of a popular hero. They did win the allegiance of an intrepid and vital press, which supplied enthusiasm for the movement as a whole.

Vocolka devotes the major part of the book to an assessment of the statements of Liberal politicians and journalists. He illustrates his points with copious documentation. Others have written extensively on the 1855 Concordat's impact on Church-state relations and on the unique relationship between the Liberal Party and the press. Vocolka acknowledges this in an extensive review of the literature in the preface. But he is the first to attempt a thorough examination of both the political and journalistic environment of 1868.

Vocolka sees the protracted debates over the passage of the laws as indicative of the Liberals' dwindling political viability. First, the proponent of revocation, Eugen von Mühlfeld, was defeated by the moderates. Second, press exhortations to pass a stricter measure were ignored in Parliament. Third, a miniature *Kulturkampf*, staged primarily by satirical journals, failed to strengthen the Liberal position. In the long run, the Liberals were unable to profit from the whole affair. Vocolka believes this to be a direct result of Liberal refusal to draw on their radical heritage of 1848. Only the school law, which removed primary schools from Church control, proved to be of lasting importance for the Liberal Party and for Austria.

While Vocolka illustrates that the Liberal weakness was already visible in what contemporaries regarded as a Liberal victory, he demonstrates less clearly the impact of the press on public opinion. His chapter on satirical magazines is primarily a selection of persiflages. In general, he more closely analyzes the press expectations of the Liberals than journalistic influence on public opinion.

This monograph provides a readable, tightly organized analysis of a number of complex issues. Vocolka develops his argument chapter by chapter and then thoughtfully summarizes it point for point in the conclusion.

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J. ROBERT WEGS. *Die österreichische Kriegswirtschaft, 1914-1918*. German version by HEINRICH MEJZLIK. Vienna: Verlag A. Schendl. 1979. Pp. 194. DM 46.

The book under review is narrower in scope than suggested by the title, for the treatment of the Austrian war economy is limited to certain industries: metallurgy, fuels, transportation, and armaments. A special chapter is devoted to manpower. Excluded from treatment are consumer goods industries, such as food and textiles, even though J. Robert Wegs points out that lack of food and clothing contributed to the Austrian defeat on the battlefield. In any case, food procurement should not have been omitted, had the aim of the author been to provide a comprehensive account of the war economy.

Be that as it may, the armaments-related industries that stood under direct control of the War Ministry are treated with great thoroughness in the carefully documented text. Actually, several government agencies, including the Commerce Ministry, regulated output and distribution, but their powers, unlike those of the War Ministry, were limited to the respective halves of the Dual Monarchy. An entire chapter provides a good overview of institutions involved.

The performance of war-oriented industries receives better marks than has been hitherto the case. Early Austrian misfortunes on the Russian front and those of 1918 on the Italian front are attributed by the author to poor strategy rather than to a lack of armaments comparable to that of the adversaries. He argues that even though Austria started the war relatively unprepared, it adapted remarkably well to war requirements. In steel and related industries the largest firms were given support, while smaller ones were allowed to close. Since allocation of raw materials to different uses followed a priority scale, many firms in the low-priority sector were forced to close as well. The labor thus released found employment in the priority sector. In addition to generating internal economies, concentration of production provided relief to the strained transportation capacities.

Metals and other commodities were distributed through specially organized "centrals," which were privately owned but stood under governmental control. Copper, which was in critically short supply, was recovered from boilers, electrical wiring, and church bells made out of bronze. Substitutes affected efficiency, for example, in transportation, since steel boilers in locomotives generated less pressure than their copper counterparts. War-related production reached its peak in 1916. From then on, bottlenecks in material inputs and transportation began to affect unfavorably both the quantity and quality of output.

Productivity declined steadily because skilled workers inducted into military service were subsequently replaced by less qualified labor. Lack of food and exhaustion through overtime work caused

additional decreases in productivity. Wages lagged significantly behind price increases. Nominal wages of inducted workers who continued in civilian work (*Landsturmpflichtige*) were lowered to the level of those in military service (*Heeresdienstpflichtige*). Relatively weak unions behaved in a docile manner, but military grievance commissions provided some relief from employers refusing wage adjustments, arguing that this would affect unfavorably their postwar competitiveness.

Investment in armaments industries was subsidized by the War Ministry within the framework of the armaments-oriented Hindenburg Program, to which Austria had acceded in 1916. One of the main beneficiaries was the gun manufacturing Škoda concern, which supplied 30.5 cm mortars even to the German army. Machine gun production was increased from 1,200 units in 1914 to 15,000 units in 1917. On the Italian front, machine guns were allocated to sectors where sharpshooters had been thinned out in combat—a chilling example of the substitution of labor with capital.

All in all, this work by Wegs is a welcome addition to the largely politically and militarily oriented literature on World War I.

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MANFRIED RAUCHENSTEINER. *Der Sonderfall: Die Besatzungszeit in Österreich 1945 bis 1955*. Graz: Verlag Styria. 1979. Pp. 416. DM 68.

Manfried Rauchensteiner, a younger historian of the University of Vienna who already has a work on military history in 1945 to his credit, has written a competent, informative study of the Austrian occupation, 1945–55. He places a heavy emphasis on the years up to 1950 and a somewhat diminishing one on the second half of this decade. The study rests on Austrian sources and British and American archival materials and is focused both on the policies of the occupying powers and the relations of the Vienna government with them. Austrian postwar domestic politics also attract the author's attention. On the whole a well-written and interesting account, it reaches conclusions that, though not startling, are unlikely to be radically altered by future research. The rather obvious fact that Austria paid with the occupation for its deep involvement with the Third Reich, its strong *grossdeutsch* and later Nazi orientation, its early enthusiastic endorsement of the Anschluss, and its role during World War II when Austrian soldiers fought loyally alongside the Germans should perhaps have been spelled out more clearly. This is not to say that Rauchensteiner has not attained a respectable level of objectivity;

but confining attention to the post-1945 period minimizes Austria's own past errors and magnifies the political and tactical mistakes of the occupiers regarding goals and methods.

In the Moscow Declaration of 1945 the Allies asserted that the treatment of Austria would be contingent on the contribution to its own liberation. This is another question that is not met head-on by the author; he rather implies (p. 197) that the annihilation of 65,000 Austrian Jews, carried out by German and Austrian Nazis, should have been calculated as Austria's sacrifices, as a "contribution" to its own liberation. In the immediate postwar era Great Britain assumed that Austria in its entirety would become a sphere of British influence. But since 1944 the Soviet Union had shown increasing interest in Central Europe. It did not, however, include Austria into the pro-Soviet, Communist-dominated belt of states, but pushed toward a quadripartite division of Austria like that established in Germany. Soon American political and military leaders began to look upon Austria as being of the "greatest political and military interest" and were convinced that the United States could not afford to let this key area slip into the Soviet fold, since this would lead to a weakening of America's strategic position in Germany and Italy. Such overriding American interests aside, the United States often played the role of mediator between the USSR and Great Britain, frequently arrayed against each other, while the Austrian government pursued a middle course, despite its pro-Western orientation in regard to domestic politics and ideology.

Soviet policy toward Austria was from the beginning shaped by the desire to provide through the occupation of eastern and northeastern Austria a shield for the protection of the developing Communist regimes in Hungary and Czechoslovakia (p. 63). In Austria, in contrast to the rest of East Central Europe, the Soviets restrained their ambitions and goals. Austria, having escaped the imposition of a Soviet-type government, was, according to the author, a unique case (*Sonderfall*). He does not always make clear the essence of this uniqueness and whether it should be credited to Soviet generosity, Austrian political skill, the occupation by several armies, the numerical weakness of the Austrian Communist Party, or the geographic distance from the USSR. Nor is Austria compared with the Communist states of East Central Europe. Since in the end the Soviet Union, for whatever reason, treated Austria better than most of its immediate neighbors, the author appears overly charitable toward the USSR and indulges in a sort of even-handed critical treatment of the occupying powers, pursuing a policy of "neutrality" similar to one to

which Austria itself, in accordance with the State Treaty of 1955, was committed.

ALFRED D. LOW
Marquette University

WALTER POLLAK. *Sozialismus in Österreich: Von der Donaumonarchie bis zur Ära Kreisky*. Vienna: Econ Verlag. 1979. Pp. 319. DM 36.

Where social democracy failed in the first Austrian republic, it succeeded in the second. From 1945 to 1966 the Socialists participated in a coalition régime with the somewhat conservative Austrian People's Party (ÖVP); since 1970 they have governed alone, under the leadership of Bruno Kreisky, winning an absolute majority again in the elections of 1979. Socialism succeeded after World War II, contends Walter Pollak, because of changes in the social structure of Austria, as well as historic developments within the Socialist Party itself.

Austrian socialism from its beginnings in the 1860s grew in tension between revolution and reform, between radical social criticism and participation in state power. Victor Adler united moderate and radical factions to build a party that led a strong trade-union movement, expanded a network of cultural and welfare institutions, and nurtured the brilliant Austro-Marxist school of social science. When the multinational empire fell apart, Socialists took the main initiative in forming a republic. Averting leftist putsches, they were nudged out of power by the right in 1920. Austrian Socialism rejected both Bolshevik revolutionism and German socialist revisionism, but its radical phraseology isolated it and contributed to an increasing political polarization in the 1920s, with the Christian Socialists moving closer to the right and the emergence of fascist and Nazi groupings. Pollak scathingly castigates the party of Otto Bauer for ideological dogmatism, political illusions, and indecisiveness. The Socialist Party remained in impotent opposition, refusing opportunities to share the responsibilities of power or to organize a workers' uprising in the face of the right-wing threat that forced it underground and into exile in 1934.

Out of the rubble of the Second World War, the Socialists were able to rebuild their party and trade unions, but they acquired additional bases of support as well. Though the population of Vienna had declined from the peak of imperial glory, the smaller provincial cities had expanded, drawing country folk to manufacturing and service industries and into the Socialist Party in their new workplaces. In 1946-47 Austria nationalized much of its heavy industry and banking, not from Socialist pressure but to keep it out of the hand of the Soviet

occupiers. These nationalized industries, as well as the continually expanding institutions of the welfare state, formed a new and powerful social base for the Socialist Party, which is no longer narrowly ideological but considers itself (and is regarded by technocrats, officials, and managers) to be an energetic modernizing force in Austria.

Pollak contrasts Bruno Kreisky's leadership most favorably with that of Otto Bauer. This leadership is challenged by younger leftists who accuse Austrian Socialism of acting as management for modern international corporate capitalism and by environmentalists who succeeded in maneuvering a "no" vote on a nuclear power plant. Pollak insists, however, that socialist reforms within the system will lead to a radical transformation of that system—the position of Victor Adler.

An Austrian Socialist journalist, Pollak died in 1977; his book was edited with an afterword by his son, Michael Pollak. The volume is not a scholarly work and takes little account of recent historical literature. There are few new insights on the period before 1945; the author should have devoted more space to the Kreisky era, to data on the changes in Austrian social structure, and to the international context of Austrian politics. Surprisingly, little attention is given to Karl Renner; and the careers of the revolutionary socialists of the 1930s, such as Joseph Buttinger, are not followed into the postwar period. But the book is refreshingly unsentimental and presents a journalistic narrative, a "modern" history of a "modern" party.

INGRUN LAFLEUR
Grand Valley State Colleges

ANNE DENIS. *Charles VIII et les Italiens: Histoire et mythe*. (Travaux d'Humanisme et Renaissance, number 167.) Geneva: Librairie Droz. 1979. Pp. 185.

The French invasion of Italy in 1494–95 has a different status in the historiography of the two countries involved: the French often date the beginning of their Renaissance from this first great encounter with Italian culture; the Italians look back on it as the beginning of a time of misery caused by foreign intervention. This work is concerned with the Italian side of the story, as it can be reconstructed from the large body of literature the event gave birth to: poems written on the occasion of entries, chronicle reports, and histories. Anne Denis has striven to extract and analyze the mythic elements that were operative in the midst of the historical act.

The chief myth, expressed mostly during Charles's trek south through Florence and Rome to Naples, was that the French king was to be a savior or liberator. The Medici-made equilibrium in Italy

was crumbling, to be sure, but that hardly explains the extravagant heralding of Charles VIII as another Charlemagne. Such rhetoric is best explained as typical humanist invention for the pomp and pageantry that characterized the march south. The principalities resisted only during the trip north; Charles barely escaped back into France with all his "conquests" lost. The French king was "the catalyst of a kind of nationalism" (p. 109) in the form of the Holy League created to harass him during the return in 1495; but, since Spain and the Empire were partners in the league, it is also true that Charles was the catalyst of a foreign interventionism that proved to be more important historically.

The Italians' disenchantment with Charles was rapid and thorough. They were used to the prince who selected himself by dint of *virtù* and so were quite dismayed by the personal mediocrity of Charles VIII—an average product of Salic dynastic roulette. They judged him to have been an instrument of *fortuna*—bad fortune in this case, since his advent marked the beginning of Italy's descent through a ring of woes down to the abysmal sack of Rome in 1527.

No great revelations emerge from this work. Indeed, the exhaustiveness of the research will appear to amateurs of the subject to yield only thematic repetitiousness. Obversely, however, those learned in the subject should savor the nuances of difference that can come only from detailed presentation of evidence. It is worthwhile, in any event, to have illuminated so well the state of mind of the citizenry in the most fervidly political part of the fifteenth-century world.

RALPH E. GIESEY
University of Iowa

MARTIN LOWRY. *The World of Aldus Manutius: Business and Scholarship in Renaissance Venice*. Ithaca, N.Y.: Cornell University Press. 1979. Pp. 350. \$34.50.

As both its title and subtitle indicate, this book explores a range of topics in later fifteenth- and early sixteenth-century Venetian history and the history of printing through the career of Aldus Manutius. Manutius is generally known only as a printer who made widely available relatively cheap and convenient texts of the classics, especially in Greek, and who employed, among others, Erasmus. But, as Martin Lowry demonstrates, he was even more interesting than this might suggest. He was, for example, a respected scholar in his own right, who chose to become a printer only in middle age for reasons not entirely clear; Lowry argues that he did so out of a conviction that literary education improves character and hence that increasing the sup-

ply of the right sort of books would make the world a better place. This work is, then, among other things, a contribution to the history of humanism and its educational ideal.

But, once having entered the risky world of business, Manutius, to survive, had to confront a multitude of problems of a quite different kind; the nature of these problems and his solutions to them occupy some of the most vivid pages of this book. Clearly he had to compromise; to support his program of publishing Greek texts, he needed the profits from the more popular Latin classics and such pious works as the letters of St. Catherine of Siena. Among Lowry's contributions to our knowledge of the Aldine Press is the revelation that Manutius provided only a small fraction of its capital, a circumstance that further limited his publication of the scholarly editions that presumably interested him more than his partners. In fact, scholars were still divided about the desirability of extending learning to a wider audience, and one of the most interesting sections of this study demonstrates the uncertainty of the educated about the value of the printing press. One element in the significance of Manutius was, therefore, that he was in a position to mediate between scholars and printers. And his achievement was substantial, as Lowry reveals in his detailed survey of Manutius's production, year-by-year, from his beginnings as a printer until his death in 1515.

This book brings together every shred of evidence about Manutius, and it has important things to say about various aspects of his period. These two dimensions of Lowry's work are, however, not always very artistically combined, and one sometimes has the impression that, because the strictly biographical evidence was thin, some of the material about the times was added to fill in gaps. The result is a useful but occasionally unfocused study.

WILLIAM J. BOUWSMA
University of California,
Berkeley

ALISON BROWN. *Bartolomeo Scala, 1430-1497, Chancellor of Florence: The Humanist as Bureaucrat*. Princeton: Princeton University Press. 1979. Pp. xi, 366. \$25.00.

In this excellent study of the career, place in society, and writings of Bartolomeo Scala, Florentine chancellor from 1465 to 1497, Alison Brown has mastered the methodological problems of researching an individual who wrote and supervised the writing of hundreds of documents and who stirred antagonisms as a client of the Medici. She solves the first problem by focusing on his reform of the chancery

as well as its instruments and the second by avoiding the temptation to assess his significance in a simple republican and Medicean dichotomy. To explain Scala's search for office and favor, Brown joins the growing number of scholars who view patronage as a customary means of self-advancement in *quattrocento* Florence. Criticisms of Scala by his contemporaries imply, however, violation of standards of obsequiousness; these standards and the degree to which Scala conformed to them have yet to be determined.

The question of Scala's favor mongering should not obscure Brown's more important research on Scala's reform of the Florentine Chancery and integration of humanistic culture with a public life. Under Lorenzo de' Medici the Florentine government, having subordinated medieval corporations, achieved a sovereign power that required a reformed chancery to express its authority. Tele-scoping changes in government and chancery that other scholars have seen as occurring over the previous hundred years, Brown views Scala's introduction of vellum for state letters, his simplified salutations in state letters, and the institution of a group of secretaries who had full responsibility for administering foreign affairs as fundamental changes "from medieval chancery to modern secretariat" (p. 161). These secretaries inscribed documents and letters as servants of the sovereign authority of the Florentine people rather than as notaries whose authority derived from the emperor. The author recognizes that not all of these reforms remained in force but sees Machiavelli's appointment to the chancery without notarial status as an affirmation of Scala's labor.

Scala should be judged as a man of public affairs with a broad knowledge of humanistic culture, Brown contends, and not as a scholar. The author accepts Scala's explanation that his "trivial ability" resulted from a paucity of books and teachers in Vespasiano's Florence and his failure to study logic. The humanists had, however, explicitly eliminated logic from the *studia humanitatis*. Humanism informs his writings, which constitute "important and original reinterpretations of classical ideas with contemporary relevance" (p. 277).

Though eclectic, Scala constructed a coherent philosophy that resembles stoicism in its reliance on human reason, a reason most fully expressed in a single man. Human experience, being too dynamic to be encompassed by laws, requires one judge who comprehends the exigencies of time and circumstance and renders a justice fit for each individual. Brown recognizes that *quattrocento* Florentines understood this as legitimization of Medici rule, but she also views it as an attempt to come to terms with the humanists' enhanced perception of flux in

history. Concerning this and Scala's relationship to even broader interpretations of humanism, Brown will doubtless have more to say in her forthcoming edition of Scala's writings.

JAMES R. BANKER
North Carolina State University

DAVID R. COFFIN. *The Villa in the Life of Renaissance Rome*. (Princeton Monographs in Art and Archaeology, number 34.) Princeton: Princeton University Press, for the Department of Art and Archaeology, Princeton University. 1979. Pp. xx, 385. \$45.00.

Twenty years ago David R. Coffin published a substantial monograph on the Villa d'Este at Tivoli, and since then he and his graduate students at Princeton have carefully re-examined a number of other important late sixteenth-century villas in and around Rome. The book under review, therefore, should represent an authoritative summation of a wealth of original research and—from the title—embody a much-needed synthetic study of architectural and social history.

The volume comprises nine chapters, the first two of which deal with the concept of *villeggiatura* as it was defined by early Renaissance humanists in Florence and as it was subsequently developed and practiced by the papal court at Rome, beginning with Paul II's *giardino segreto* at the Palazzo Venezia and culminating in a series of late *cinquecento* papal and cardinalate villas clustered about the hillside town of Frascati. As a *tour d'horizon*, these chapters are effective; his thorough knowledge of secondary literature and especially his exhaustive reading of unpublished *avvisi* preserved in the Vatican Library enable Coffin to document successfully the restless movements of Church dignitaries in search of rustic tranquility, healthier air, and lavish private entertainments during the months of *vacationes generales*.

Coffin then describes major monuments in three main groups: the early suburban villas, represented by Innocent VIII's Belvedere, Agostino Chigi's Farnesina, and the papal hunting lodge of La Magliana; then the more elaborate "ceremonial" retreats, including the Villa Giulia and the *vigne* of cardinals Caraffa, Grimani, Pio, and d'Este on the Quirinal as well as the Villa Medici and its neighbors on the Pincian Hill; and finally the classicizing examples of the Villa Madama, the Villa Lante, and the Casino of Pius IV. To these familiar works, Coffin adroitly adds a large assortment of relatively obscure *vigne*, *barchi*, and *casini*, so that nearly fifty individual buildings are discussed. In the final chapter the Farnese Palace at Caprarola, the Villa d'Este at Tivoli, and the Villa Gambara-Lante at Bagnaia are offered as the preeminent masterpieces of a golden age.

Unfortunately, the book fails to fulfill the promise of its title. Villa culture is presented as an essentially architectural phenomenon, and Coffin's approach to the subject depends almost exclusively on chronicle and description. Each building is laboriously reviewed as a unique artistic entity whose commission, authorship, building history, siting, design, painted decorations, and functions are dryly discussed in turn. As a result, the text resembles an extended catalogue, rich in new documentation but regrettably barren of comprehensive analysis. Issues of central concern to many architectural historians are never pursued. For example, Coffin believes that the origins of the villa are to be found in vernacular farm building, but he timidly avoids reconstructing a coherent historical evolution. Similarly, non-Roman precedents and developments are ignored, and the history of garden planning—surely of capital importance here—receives scant attention. Ideas about the villa from Renaissance architectural writers such as Francesco di Giorgio, Sebastiano Serlio, and Anton Francesco Doni are not invoked where they might have illuminated aspects of design or function.

To this unusually circumscribed view of villa architecture, Coffin brings an even narrower conception of its human context. The title's "life" is restricted solely to the pursuits of *otium* by the nobility. For this the *avvisi* supply newsy data concerning the whereabouts of important persons but little regarding the activities of tenants and caretakers, villa economy, or the pattern of social behavior inside the country villa. To be sure, these are difficult subjects, but they do require deeper research if the Italian Renaissance villa is to be comprehended beyond its intrinsic esthetic qualities.

This is the first book ever to appear on this topic in English, and it will certainly prove valuable to interested students as well as to specialist historians. Both its contributions and its omissions should encourage future studies in a remarkably attractive field.

RICHARD J. TUTTLE
Tulane University

JOHN W. O'MALLEY. *Praise and Blame in Renaissance Rome: Rhetoric, Doctrine, and Reform in the Sacred Orators of the Papal Court, c. 1450-1521*. (Duke Monographs in Medieval and Renaissance Studies, number 3.) Durham, N.C.: Duke University Press. 1979. Pp. xii, 276. \$17.75.

Given the current interdisciplinary interest in Renaissance language theory and discursive practice, this monograph by John W. O'Malley has precisely the scale and range needed at this stage in the study of Renaissance eloquence. He has admirably ful-

filled his stated purpose: to investigate a "new body of evidence," the sermons preached before the pope from the pontificate of Nicholas V to that of Leo X ("new" because most of the texts were both unpublished and also unread by other scholars), by employing a "new hermeneutical device." He uses the canons and technical principles of the epideictic genre of classical rhetoric (the modes of "praise and blame") to reveal "changes in form, mood, and content" that will in turn illuminate the history of Renaissance preaching, "the theology and religious world view operative at the papal court," and certain issues in the history of Renaissance humanism as well (p. 3).

Of particular use to the historian is O'Malley's elucidation of the potential of genre studies. The study of genre lies at the point of intersection of social and intellectual historical inquiry; the study of "literary" genres is comparable to the study of "semiotic" genres, the codes and messages embedded in festivals, rituals, costume, or urban architecture. All genres are well-demarcated sites of very specific cultural activities. The study of genre evades the invidious choice between "form" and "content," or between social institution and intellectual theme, since a genre represents the institutionalization of a theme or themes and genre rules, governing the forms of address and reception, are particularly responsive to shifts in social needs, motives, and behavior.

O'Malley, to be sure, does not have the command of sophisticated techniques of discursive analysis that we find in French sociolinguists or even Anglo-American literary historians, but his erudition and common sense do not betray him. The chapter "A Renaissance Setting" is an admirable exposition of what the sermons were meant to do and the nature of the audience's expectations, expectations that turn out to be ambivalent, asking both erudition and brevity, elegance and piety. A forceful example of the contribution of genre study to intellectual history is his chapter on the ethical content of the sermons, "The Ideal of Christian Life"; O'Malley usefully undermines the rigorous opposition of traditional historiography between humanist and Scholastic intellectual elites in the Renaissance when he documents their separate reactions to the constraints of the sermon genre as resulting in the development of virtually identical ethical themes.

In sum, O'Malley uses the sermons to give a fresh perspective on Renaissance culture, illuminating such diverse issues as the extent of the classicizing or "pagan" bent of the Roman court, the culturally inclusive nature of the notions of reform within the papal circle, the limits of the pretensions to elegance and refinement of a particular Renaissance elite, and the shifts in didactic aims of the preachers,

which entails a revised view of the needs and motives of the homilectic audience.

NANCY S. STRUEVER

Johns Hopkins University

PAOLO SIMONCELLI. *Evangelismo italiano del Cinquecento: Questione religiosa e nicodemismo politico*. (Italia e Europa.) Rome: Istituto Storico Italiano per l'Eta Moderna e Contemporanea. 1979. Pp. xxxii, 490. L. 10,000.

In this weighty contribution to the much-discussed but still problematical issue of Italian Evangelism—now generally acknowledged to be philo- and almost always crypto-Protestantism rather than an indigenous current flowing into the so-called Catholic Reformation—Paolo Simoncelli applies more broadly the interpretive scheme set forth in his *Il caso Reginald Pole* (1977). To understand what Evangelism was and why its adherents failed to head off the rigors of the Counter Reformation, he insists that we must look at the interaction between its "internal" evolution (what went on in the minds, discussions, and writings of *spirituali* like Pole and Gasparo Contarini) and "external" developments (shifts in papal and imperial policy relating to the Protestant challenge, the exigencies of local political situations such as Cosimo I's effort to establish an absolute state in Tuscany) in the critical period from the mid-1530s to the mid-1550s.

The heart of this book (the introduction, chapters 1, 2, 4, and 5) is a detailed examination of these interrelationships. By about 1537, Simoncelli shows, three major Italian groups had emerged: radicals (including Antonio Brucioli, Marcantonio Flaminio, and Bernardino Ochino) committed to propagating openly in the vernacular the Protestant message of salvation by faith alone and predestination; moderates (Contarini, Gian Matteo Giberti, and others) personally convinced of these tenets but wary of the social consequences of disseminating them; and intransigents (above all Gian Pietro Caraffa, the future Paul IV) determined to crush the northern Protestants and silence their Italian sympathizers. During the 1540s the radicals set forth their position in words (notably in the *Beneficio di Cristo* of 1543) and action (the flight from Italy of Ochino, Pier Martire Vermigli, and Pier Paolo Vergerio), while the intransigents adroitly discredited the moderates by implicating Contarini (sent on a hopeless mission to the Regensburg Colloquy) in illegitimate compromise with Protestants. By the late 1540s the radicals were for the most part dead or departed and the moderates reduced to real or apparent (Nicodemite) conformity. Still, in the drastically reduced space left to them, the *spirituali* cleverly managed to express their con-

victions in veiled forms, such as incorporating close paraphrases of the banned *Beneficio* into their writings and utilizing literary genres (such as collections of vernacular letters) to memorialize their heroes and celebrate past moments of glory.

Simoncelli's main arguments are important and convincing. They would be easier to follow, however, if his sentences and paragraphs were shorter and if, adhering to J. H. Hexter's "maximum impact rule," he avoided excessive quotation, particularly from widely available published source collections. Two sections of this volume are somewhat peripheral. Chapter 3, a brief examination of hitherto neglected works and letters of Vittoria Colonna and others (texts included in appendixes), interrupts the presentation of his central argument: it should have been issued separately. Although the final chapter, dealing with "political Nicodemism" among Tuscan literary figures of various political stripes, helps to illustrate the major themes, it is admittedly a work in progress in which tentative hypotheses are being tested; hence its tone is not consonant with that of the book's core. Despite these shortcomings Simoncelli's work should be read with care by students of mid-cinquecento Italian religion, politics, and culture.

ANNE JACOBSON SCHUTTE
Lawrence University

ROBERT BONFIL. *The Rabbinate in Renaissance Italy*. Text in Hebrew. Jerusalem: Magnes Press. 1979. Pp. 327. \$20.00.

The rabbinate for Jews is the legitimate interpreter of their talmudic-legalist tradition that serves them as an authoritative guide in all areas of life and religion. Little wonder, then, that so many scholars are attracted nowadays to the history of the institution, trying to trace its development and fix its social and religious parameters. For this present study, Robert Bonfil has recourse to communal registers, for the most part in manuscript, to rabbinic *responsa*, legal formularies, homilies, and the like. Then he correlates all these data with information culled from municipal Italian archives. His principal aim is to determine the characteristics of the institution by placing it within the context of the socioeconomic system in which it developed. In this manner, he is able to explain in his first chapter the emergence of the rabbinic and Renaissance university. On pages 227-28, he even goes to the trouble of juxtaposing, column with column, a rabbinic ordination with a Latin one, to demonstrate to us how they adhere to the same literary pattern.

From chapter 2, we learn that some Renaissance rabbis did become public functionaries dependent upon the communal payroll. Mosse Bäsola (1450-

1560), himself a rabbi and an early explorer of Palestine, held all this in little esteem, claiming that such a relationship made the rabbis play into the hands of rich dignitaries (pp. 92-94). Bonfil, in one of the high points of his study, is able to compare the salaries of these appointed rabbis with those of contemporary workmen in the building trade (pp. 103-10). He concludes that the rabbis were poorly, indeed miserably, paid. No wonder that so many of the scholars searched for ways to make a living outside communal structures.

Some of those rabbinical Renaissance scholars, Bonfil suggests, deserve more attention in the future from researchers. Surely, we should have enough data to venture a biography of Jehiel Nissim of Pisa or of Mosse ben Abraham, Provençal (1503-75). This in turn might help us understand even better the way in which the civilization of the Renaissance reflected itself in the personalities of those rabbis (pp. 173-206). From an impressive collection of booklists, Bonfil is able to establish, by means of quantitative content analysis, that there was a clear decline in interest in scholastic philosophy, while the rising interest in mysticism should be understood as a kind of repentance practiced by the Jews who were marked by the trauma caused by the expulsion from Spain in 1492 (pp. 186-87).

The last part of the book consists of some sixty-four documents, many of which are published for the first time. Bonfil treats this part of his work with the same delicacy and care that he displays through the whole book.

JOSEPH SHATZMILLER
University of Toronto

JEAN GEORGIN. *Venise au siècle des lumières*. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques. Civilisations et Sociétés, number 41.) Paris: Mouton. 1978. Pp. 1,225.

The recipe for a popular history of eighteenth-century Venice, as Jean Georgin points out, has been known for a long time. Start with a large portion of Casanova, add a measure of masked ball, *commedia dell'arte*, gambling, and scandal, finish off with Napoleon, and serve. Modern Venetian scholarship, for the most part, has overlooked the eighteenth century and has failed to penetrate very far beyond the superficial symptoms of urban senility. The exceptions are noteworthy: books by Davis and Tabacco on the decline of the nobility and its power to govern; important chapters by Lane and Fanfani on the late revival of trade; and the monographs of Beltrami and Berengo on population, agriculture, and social history. Georgin's ambitious goal is a total history of eighteenth-century Venice, ranging from the traffic of the port and the output of main-

land industry to the political crisis culminating in the end of the republic itself. Cycles in agriculture, demography, prices, administration, crime, and public finance are explored, often by reference to massive statistical tables that unfortunately run the gamut from difficult to impossible to interpret.

Georgelin argues that the decline of the Republic of Venice was less the fate of its people than that of its ruling class and therefore its government. As such, the roots of the crisis extend back into an earlier age, with the formation of a government by exclusive closed caste. By the 1700s, governing power had fallen into the hands of about forty-two families. According to Georgelin, even though there was no great shortage of competent administrators, the oligarchy slowly ceased to rule in a progression of deepening crises.

Georgelin devotes a substantial part of his book to the fortunes of the patricians, their house property, mainland estates, wealth, and offices. Taxation, justice, and the bureaucratic structure of government also receive attention. But the story of how the government functioned, or failed to function, remains in large measure untold; the link between government and society is never fully forged. The reader does not know whether administration was efficient, whether government policy helped or hindered enterprise, and, perhaps most important, whether the crisis in government mattered at all to the livelihood of the people.

Venise au siècle des lumières may soon become the premier source book for eighteenth-century Venice but not its premier history. Georgelin's diligent mining of the archives has brought forth hundreds of tables and charts (the appendixes alone run over two hundred pages), but the narrative is often fragmented and inconclusive in its interpretation of the century's major economic, political, and social trends, almost as if consideration of these trends had been submerged by the massed findings of research on matters of lesser scale. Nevertheless, Georgelin's achievement in exploring the cyclic rhythms of this difficult century will endure as an important contribution to Venetian history.

RICHARD T. RAPP

National Economic Research Associates

LUCIANO GUERCI. *Condillac Storico: Storia e politica nel "Cours d'études pour l'instruction du Prince de Parme."* Milan: Riccardo Ricciardi Editore. 1978. Pp. xi, 477.

It has long been recognized that Enlightenment history often served the interests of Enlightenment polemics. The philosophes regarded the past as a battle waged between the forces of reason and ob-

scurantism. History's role was to elucidate this battle and ensure that the present generation adhered to the right side.

It is therefore not surprising that, upon his appointment as preceptor to the young Duke of Parma in 1758, Étienne Bonnot de Condillac composed a twelve-volume history as part of his charge's curriculum. Divided into the *Histoire ancienne* and the *Histoire moderne*, it is basically a pedagogic manual of interest to modern scholars because it illustrates how political ideals can determine the choice and treatment of historical events and themes.

Luciano Guerri's study is competent and scholarly. In the first part he places Condillac's pedagogic activities within the reforming milieu of mid-eighteenth-century Parma. In the second part he analyzes Condillac's treatment of specific subjects. The philosophe approves of those rulers and states that adhere to a nondespotic form of absolute rule, resist the temporal authority of the papacy, and prevent the pursuit of luxury from undermining their subjects' moral fiber. Those who fail in one or more of these categories are subject to criticism.

Given these criteria, the results are predictable. The balance sheet on Rome—even the Republic—is negative. Pursuit of wealth encouraged dissensions among classes, eventually causing political and social anarchy. Other people and events who fare badly include Gregory VII, the Crusades, and Louis XIV. Among those approved and recommended to the prince are Charlemagne, Louis IX, and Henry IV. Throughout the *Histoire* Condillac emphasized that rulers should moderate the use of their authority and should not allow the papacy to interfere in their states' temporal affairs.

Among this study's more interesting features is the examination in both text and notes of the way in which Condillac used and modified the historical opinions of such contemporaries as Voltaire, Mably, Millot, and Rousseau. In this context, the chapter on the Franks is of particular importance. Here Guerri links Condillac to the eighteenth-century debate concerning the origins of feudalism and the effect of the Frankish conquest on political institutions and social structure. Opposing the aristocratic interpretation of Boulainvilliers and the absolutist position of Mably, Condillac suggests that early Frankish rule was largely democratic and that absolutism developed for the most part after the reign of Clovis.

As is the case with many studies that attempt to relate ideas and environment, the connections between the two here tend to be vague. Parma's milieu may have provided the occasion for writing the *Histoire*, but its actual influence upon Condillac's historical thought is not clearly shown. Generally,

however, this book is a useful addition to the field of Enlightenment historiography.

EDWIN G. EHMKE
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ANTON VAN DE SANDE. *La Curie Romaine au début de la restauration: Le problème de la continuité dans la politique de restauration du Saint-Siège en Italie, 1814-1817*. (Studiën van het Nederlands Instituut te Rome, number 6.) The Hague: Staatsuitgeverij. 1979. Pp. 268.

The key problem that faced the restored papacy in 1814 was the need to adapt to a world transformed by revolution. To discover the extent to which this need was recognized at Rome, Anton van de Sande has analyzed the decision-making process of the curia on three questions: the reform of the Papal State, the ecclesiastical reorganization of Piedmont, and the negotiations with Vienna over Lombardy-Venetia.

Historians have usually depicted the curias as split into two hostile factions: *politicianti*, those prepared to make realistic adaptations to the post-revolutionary world, and *zelanti*, those dedicated to a rigorous defense of principle against opportunism, of tradition against adaptation. Sande finds this contrast too stark. The picture that he presents is of two vague and shifting groups, differing on tactics but in agreement on principles and basic objectives; the policies that emerged from their interaction were characterized by continuity with the eighteenth century.

Sande's interpretation seems valid where religious policy is concerned: here *politicianti* and *zelanti* alike were convinced that the primary objective was the establishment of effective papal supremacy over the church at the expense of bishops and rulers. As in the eighteenth century, therefore, the struggle against Jansenism and Josephinism, not the struggle against the revolution, was seen as crucial. No better example of this attitude—or of the tactical flexibility of the curia—can be found than the curia's determination to base the ecclesiastical settlement of northern Italy upon the Concordat of 1801, which, despite its revolutionary origins, had marked a notable advance for papal authority over the episcopate. In the negotiations with Piedmont, difficulties arose only when Victor Emmanuel wished to abolish this vestige of the French regime; the curia, however, insisted upon preserving its chief provisions and eventually won over the king with financial concessions. It met with more tenacious opposition in Lombardy-Venetia, where Francis I was resolved to introduce the Josephinist state-church, and negotiations ended in failure in 1817.

Sande's case is less impressive when he turns to

the Papal State. He argues for continuity between Consalvi's reform program and that of Pius VI, but this unduly minimizes the novelty of Consalvi's reforms, which went far beyond anything Pius VI had contemplated. Moreover, where reform was concerned, the most striking continuity in the curia lay not in a "tradition of *reformismo*" as Sande feels but rather in the consistent hostility of most of its members, which brought the defeat of every serious attempt at reform during the nineteenth century. Though Sande tries to minimize this hostility, it seems undeniable that here the traditional picture of a curia polarized between a reactionary *zelanti* majority and a *politicianti* minority is essentially correct.

Even the reader who does not fully accept Sande's arguments, however, will appreciate this perceptive work and the extensive archival research on which it is based; it is a welcome addition to the slim roster of recent studies on the Papal Restoration.

ALAN J. REINERMAN
Boston College

MORRIS B. MARGOLIES. *Samuel David Luzzatto: Traditionalist Scholar*. New York: KTAV Publishing House. 1979. Pp. xv, 253. \$15.00.

Samuel David Luzzatto (1800-65), known as Shadal in Jewish history, was an Italian-Jewish scholar of second-order importance. There has been no intellectual biography of him until now. Morris B. Margolies provides this in a useful, though in turn quite middle-brow, fashion.

"Landscape and Influences" sketches the historical background. "Life and Work" concentrates on Luzzatto's tough life and on his many years as teacher at the Collegio Rabbinico in Padua. "Luzzatto's Religious World" outlines his rather simplistic ideology: he was committed to the divinely revealed character of the Pentateuch and to what he interestingly called the contrast between "Abrahamism and Atticism" (that is, ethics versus intellectualism). His scholarly work was concentrated on Biblical exegesis (grammar, history, and so forth). This obviously interests Margolies most, to the point that he occasionally even adds homilies of his own in the chapter "Exegesis." An extraordinarily large proportion of Luzzatto's writings was done in the form of letters, which he exchanged with his Jewish scholarly contemporaries (of whom several are more important than he); these were published either at the time or since. Margolies discusses them in "Luzzatto, Rapoport, and Other Contemporaries," and he adds a few previously unpublished, but also unimportant, letters in an appendix. Next to Biblical studies Luzzatto was chiefly interested in

Hebrew poetry, and he did important work in collecting and editing, especially the medieval R. Yehuda HaLevy's creations. He also published a lot of his own rather mediocre poetry in Hebrew, the language in which he did most of his work. A brief overall evaluation, bibliographies, and indexes conclude the book.

There was not much history in Luzzatto's life. He was a bookish man. He had a short fling of enthusiasm for the Italian revolution of 1848 (pp. 42-48), which one might have liked to see treated more fully. His Jewish "politics" consisted of devotion to the peoplehood of Israel in history and to a very moderate, essentially religious, proto-Zionism.

Jewish modernization occurred in Italy and Germany in the nineteenth century along parallel and, as Luzzatto's life shows, even parallax lines. His politics, as just outlined, coincided with those of his German contemporary, the founder of "neo-Orthodoxy," Samson Raphael Hirsch. The conservative wing of Jewish modernization shared heavily biblical—as contrasted to talmudic—Judaism, Hebraism, theological Zionism, and bourgeois liberalism. Most of Luzzatto's peers (and superiors) were Ashkenazic Jews. Even his "religious empiricism" is, in a simplistic way, a precursor of Franz Rosenzweig's highly sophisticated phenomenologism in the twentieth century.

Margolies is obviously not particularly interested in intellectual history. If he were, he could have made more even of Luzzatto's middle-brow conceptuality. (Some of the previous secondary literature treats him in such a perspective.) His view can be described as religious empiricism, equally opposed to the main Jewish-rationalist tradition and to kabbalistic mysticism. (This explains his strained relations with clearly the most important of his intellectual contemporaries, the philosophical historian, Nachman Krochmal.)

In a preachy foreword the chancellor of the Jewish Theological Seminary of America stakes a claim to Luzzatto in the name of American "Conservative Judaism." There is some ideological justification for this claim. On the other hand, Margolies mentions casually (p. 209) that Luzzatto's grandchildren, like Moses Mendelssohn's in Germany, were Christians.

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PAUL GINSBURG. *Daniele Manin and the Venetian Revolution of 1848-49*. New York: Cambridge University Press. 1979. Pp. xiv, 417.

CLARA M. LOVETT. *Giuseppe Ferrari and the Italian Revolution*. Chapel Hill: University of North Carolina Press. 1979. Pp. xiii, 278. \$15.00.

The Risorgimento, considered by some the triumph of nineteenth-century liberalism and nationalism, is one of the most studied periods of Italian history. Still, most studies of the Risorgimento seem to support the adage that in history, as in life, it is success that counts. Thus, those who provided the inspiration, political leadership, and the military means for the creation of the Unitary Kingdom have been studied and restudied while the role of the republicans and the radicals on the one hand—with the exception of Mazzini—and the figures of the Catholic counter-Risorgimento, on the other, have received far less attention. The volumes under consideration shed considerable light on the activity and thought of two men whose vision for Italy differed substantially from that of the liberal monarchists and moderates who molded the new state in their image.

Neither of these studies is, or pretends to be, a comprehensive life and times, so those planning such an enterprise for Manin or Ferrari may proceed with the assurance that they have not been pre-empted. As the titles indicate, Manin is studied in light of his role during the Venetian revolution of 1848-49, while Ferrari's thought and public life are examined within the context of the broader Italian revolution. Consequently the book by Paul Ginsborg is more of a monograph while the volume by Clara M. Lovett is a broader biographical survey. The latter work, however, concentrates on the protagonist's intellectual interests and public activity because the Ferrari archive of the *Museo del Risorgimento* yields little about his personal life. Nonetheless Lovett has made the most of the correspondence, comments, and glimpses of family, friends, and other contemporaries in her attempt to reconstruct Ferrari's private world.

Both Ginsborg and Lovett have tapped a wide range of primary sources, particularly archival, and Ginsborg provides a useful commentary on the many archives in which he has worked and the documents contained therein. The authors share a certain sympathy for the central figures in their studies and, in general, assess their contributions positively. Nonetheless, neither has permitted appreciation for the protagonist to cloud objectivity. Thus, although Ginsborg cites the decisiveness and superb leadership provided by Manin in the revolutionary events in Venetia in the spring and early summer of 1848, he does not hesitate to expose Manin's failure to forge an adequate political and military strategy for the preservation of the republic he was instrumental in creating. Likewise, Lovett reveals that, although politically Ferrari remained true to his radical orientation, philosophically he moved increasingly away from the belief in historical progress and the optimistic view of human nature that had nourished his political outlook. Lovett

does not delve into the anomaly of Ferrari's advocating the elimination of inherited fortunes to bring about political equality while enjoying a comfortable existence and pursuing an academic, literary, and political career made possible only by his own inheritance.

Ginsborg's detailed account of the making of the revolution in Venice and its fight for survival in the ensuing months is the first overall account since Vincenzo Marchesi's *Storia documentata della rivoluzione e della difesa di Venezia negli anni 1848-49* (1916) and G. M. Trevelyan's *Manin and the Venetian Revolution of 1848* (1923). Resting upon a broader documentary base than these earlier works, it avoids the hagiographic excesses of Marchesi and Trevelyan. In a clear and concise manner, if not always with literary flair, Ginsborg presents the events of the revolution within the social and economic setting of the Veneto in the nineteenth century as well as within the broader perspective of the European upheaval. Although the first two chapters provide a capsuled picture of Venetian society and chronicle the events leading to the outburst, the remaining chapters concentrate upon the revolution and Manin's role in it. The contents not only justify the title but also make an important contribution to the literature of the revolutions of 1848.

The Lovett volume, the first full-length study of Ferrari in English, provides insights both into the operation of the parliamentary left as well as the political process in Italy prior to the formation of mass parties. It makes available to a wide audience Ferrari's thoughts and writings as a radical intellectual, political theorist, and philosopher of history. Lovett maintains in her introduction that Ferrari was a political theorist no less original than Leroux, Proudhon, and Renan but notes in her conclusions that his socialism was derivative and remained vague while his arguments against *laissez-faire* liberalism were never developed into a coherent critique of bourgeois society. The aspect of his thought that she finds most original and significant for the history of the Italian left was his lifelong commitment to secularization embodied in his call for *irreligione*. Within the pages of this study in intellectual history, the author demonstrates that Ferrari played an important role in the international brotherhood of radicals that sought the secularization condemned by Pio Nono.

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VINCENZO G. PACIFICI. *Le elezioni nell'Italia unita: Assenteismo e astensionismo*. (Istituto di Storia Moderna, Università di Roma. Storia, number 6.) Rome: Edizioni dell'Ateneo e Bizzarri. 1979. Pp. 286 L. 9,000.

Although he acknowledges in full the import of the gap between *paese legale* and *paese reale* at the time of Italy's unification and the all too slow reduction of this gap by the extension of suffrage during the liberal era up to the eve of the First World War, Vincenzo Pacifici contends that the complexity of the problem cannot be appreciated without an examination of the twofold phenomena of electoral absenteeism and abstentionism: the former deriving from indifference, indolence, and logistic and climatological factors; the latter, an act of protest. In emphasizing the significance of absenteeism-abstentionism, and the gradual reduction of their incidence through the efforts of those very governing classes so much criticized for their slowness in closing the hiatus between the two *paesi*, Pacifici endeavors to demonstrate the liberal regime's capacity and determination to achieve a more perfect identity between the governing classes and the country as a whole.

Somewhat more than a third of Pacifici's work is devoted to an analytical summary of electoral proposals and legislation between 1848 and 1912, the theme of which is the gradual movement from an electoral system based on tax-paying qualifications to one based on minimal educational requirements. This movement is confirmed in the book's second chapter, which consists of an elaborate statistical national and regional study of the electoral body and its growth and composition, with an interesting aside on the extent of government intervention designed to influence the voters' choices. Only at the midpoint of his work—somewhat late—does Pacifici address himself systematically and consistently to the problem of absenteeism, which is examined in reference to such quantifiable factors as electoral constituencies that were slowly and imperfectly adjusted to population changes; the geographical area of constituencies; relevant transportation facilities; and voter turnout as influenced by weather conditions.

The incidence of Catholic abstentionism, inspired mainly by papal hostility toward a state created at the expense of the church's temporal prerogatives, is treated both in statistical terms and in the light of a profuse literature on the subject. Pacifici allots fewer pages to the briefer abstentionism of the groups on the extreme left, which were numerically less significant. A concluding chapter examines the incidence of absenteeism-abstentionism in twelve sample cities chosen for such qualities as historical antecedents, economic activity, topographic peculiarities, traditional devotion to the church, or marked preference for the left.

Although not the definitive study on the nature and significance of electoral absenteeism and abstentionism, Pacifici's work does much to deflate the notion that these phenomena were evidence of in-

herent and irremediable flaws in liberal Italy's political framework.

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ALEXANDER J. DE GRAND. *Bottai e la cultura fascista*. Translated by PIETRO NEGRI. (Storia e Società.) Rome: Editori Laterza. 1978. Pp. vii, 299. L. 8,000.

Alexander J. De Grand's excellent study of Giuseppe Bottai deals with a highly intelligent and cultured Fascist, and one of its chief merits is that it accepts the credibility of Bottai's role as a Fascist *intellectuel engagé* without falling victim to the beguiling myth of Bottai the "good" Fascist. Fascism—like other modern political movements—had more than its share of faithful idealists who were dissatisfied with the realities of a regime they had helped to create.

De Grand's purpose was not to write a biography but rather to examine the nature of Bottai's ideas and aspirations, his efforts to implement them through the Fascist bureaucracy, and his overall impact on Fascist political culture. Based on Bottai's works and the periodicals he edited, as well as on public and private archives, the book offers a persuasive assessment of Bottai's thought by balancing his own internal vision with the external evidence of the sources.

A talented and elusive personality, Bottai (1895–1959) came to fascism in 1919 as a nationalist, an officer in the *Arditi* corps of World War I, and a futurist. He rose quickly in Mussolini's regime, serving from 1926 to 1932 as undersecretary and then as minister of corporations and from 1936 to 1943 as minister of national education. Bottai was bent on reconstituting the nation's "ruling class" in order to produce fundamental changes in the Italian economic, social, and political order. His original hope was to forge the Fascist party into the nucleus of a new, technocratic elite, but when he was blocked by Mussolini's statist policies he sought, unsuccessfully, to use the corporative system to achieve his ends. De Grand concludes that Bottai was essentially a conservative modernizer with technocratic tendencies.

Bottai's real importance was in his role as an intellectual catalyst. He maintained close ties with non-Fascist elements in Italian cultural life, vigorously defended modern currents in art against the reactionary tendencies within the regime, and worked desperately to stimulate and sustain an open dialogue of ideas and self-criticism within fascism. This effort to put himself at the center of an internal but loyal intellectual opposition attracted many of Italy's finest young writers and artists who, like Bottai himself, were becoming increasingly al-

ienated from Mussolini's dictatorship. His participation in the 1943 coup that unseated Mussolini reflected his belief that the regime had bankrupted itself but that its failure was one of leadership, not of system.

De Grand has produced a first-rate book, crisply composed, soundly researched, and skillfully structured. It will serve as a point of departure for future work on Fascist cultural policy, and, in its methodology and approach, it is a model study of the kind that is much needed in the history of Italian fascism.

PHILIP V. CANNISTRARO
Florida State University

ROMAIN RAINERO. *La rivendicazione fascista sulla Tunisia*. Milan: Marzorati. 1978. Pp. 580. L. 10,000.

This prolific Italian historian has given proof of his ability to write critically on various issues of Italian colonialism, such as *L'anticolonialismo italiano da Assab e Adua* (1971), in which, besides using archival documentation, he complemented the research with accounts of personalities who participated in the political process. With *La rivendicazione fascista sulla Tunisia*, Romain Rainero's contribution is the exegesis of Fascist imperialism through the historical method; he reaches conclusions from a socialist point of view, hence the reason for incorporating the reaction of the political opposition to Fascism and the attitude of the Arab indigenous population. A large part of the volume is set aside to reproduce published and unpublished documents from 1868 to 1965. This is the ultimate work available to date on the Fascist claim to Tunisia.

The Fascist claim was facilitated by the presence of more than 100,000 Italians. Mussolini's demand for Tunisia on other occasions was based on French nonfulfillment of the Treaty of London of 1915. Mussolini took advantage of the territorial claim and turned it into a political showcase for internal use and a tool of diplomatic barter in the international arena. At one point in 1935, Mussolini was willing to abandon the protection of the Italians in Tunisia in exchange for concessions from Laval in Ethiopia. But in 1937–38, when France, after a change of government, refused to recognize the Italian Empire of East Africa, Mussolini threatened to repatriate all Italians from Tunisia although this would bring economic hardship to both the Italian community in North Africa and to Italy. Even more unrealistic was the Fascist hope to take advantage of Tunisian nationalism by supporting Arab political aspirations and allowing Bourguiba to broadcast against France from the radio station in Bari. It is difficult to understand how the Tunisians, who had seen Italian suppressive colonial policy at work

in Libya and Ethiopia, could have supported the Fascist cause in their homeland.

The implementation of Fascist policy in Tunisia was made possible by the Italian consulate and the activities of pseudocultural institutions. They coerced the Italian emigrants to follow the dictates of Rome, which maintained that all Italians residing in Tunisia were to be considered Italian citizens and should not be given French citizenship. Those Italians who were not affected by Fascist propaganda cooperated with Italian political refugees, who availed themselves of the press and mass media to accuse Fascism of preventing the Italians from deciding their own future within the French community.

Rainero shows, with empirical evidence, that the Italians in Tunisia became the political pawn of Mussolini's mercurial diplomacy; the Duce exploited them to further Italy's expansion in the Mediterranean. Rainero, together with other historians, is of the opinion that Mussolini's policy toward Tunisia was not consistent and can be explained in terms of opportunism and lack of organization. More specifically, the incoherence of Fascist foreign policy resulted from a policy of enhancing Fascist prestige before the Italians at home and confusing its enemies. Mussolini acted, vis-à-vis France, as a leader of a powerful nation that, in reality, was weak and divided within. To Mussolini's bluffs, France counterposed an attitude of patience and at times of intransigence, thus exposing the Duce's untenable policy and embarrassing him before the world.

La rivendicazione fascista sulla Tunisia makes for pleasant reading. The author's style is clear and to the point. Historians, students of colonialism, and persons interested in Fascism will find entertainment and pertinent information, and they will appreciate the well-balanced opinions and conclusions reached by the author. Rainero's work should also reach the Anglo-Saxon public through a translation of the book into English.

ALBERTO SBACCHI
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C. M. WOODHOUSE. *The Struggle for Greece, 1941-1949*. Brooklyn Heights, N.Y.: Beekman/Esanu. 1979. Pp. xii, 324. \$29.95.

C. M. Woodhouse offers his readers as nearly unbiased an account of the internal military struggle in Greece between 1941 and 1949 as anyone is ever likely to be able to produce. He was himself a conspicuous actor in the first round between Communist and non-Communist Greeks when he headed the British mission in occupied Greece, 1943-44.

During this time he became well acquainted with many of the leading personalities in the struggle, and in subsequent years he followed events in Greece closely and continued to collect information from all sides. This book is therefore a record of Colonel Woodhouse's involvement in and understanding of Greek public affairs during the violent upheavals of 1941-49.

The genesis of the book imposes limitations as well as conferring the strength of authenticity. For example, Woodhouse has not poured over British or American archives, where details of official actions and policies are now available for historians to disinter from one-time secret files. He is, in fact, not much interested in the American role in Greece after 1947 and concentrates instead on the disagreements that plagued the Greek Communist Party leadership throughout the so-called third round. Since such disagreements were carefully hidden from outsiders at the time, this aspect of the book is indeed interesting and instructive. So far as I can tell, he is fair-minded in what he says, though his distaste for Nicholas Zacharaidis and his sympathy for Markos Vaphiades is obvious. But then, Zacharaidis does appear to have been an unwise as well as an unpleasant man; and Vaphiades was undoubtedly brave and at least comparatively straightforward in the policies he advocated.

A more important limitation of the book is this: Woodhouse remains content, for the most part, to recount what men said and did. He spends very little attention on the setting in which they acted—neither the international setting, nor the domestic Greek setting. When he does make remarks about Greek society, they appear as *obiter dicta* and do not carry much conviction, at least to this reviewer. Thus, for example, Woodhouse says of the year 1947: "The following year was to be one of military stalemate while that struggle [for the hearts and minds of the Greek people] was grimly fought out. It was not a struggle between classes, for all classes were equally represented on both sides" (p. 226). Yet a few pages later (p. 232) he points out that the democratic army failed to win recruits from the cities and had to depend almost entirely on men and women who lived in remote hill villages. It follows that there were significant class and geographic differences that distinguished the one side from the other. But Woodhouse has not thought much about these matters, and in the quoted passage, presumably, had a handful of leaders of the two camps mainly in mind.

But a history of words and deeds that is not tied into an analysis of the social setting within which men acted easily becomes a burden on the memory rather than an enlightenment of the mind. Woodhouse's book, for all its merits and admirable detachment, flirts with triviality for readers who did

not themselves live through those times or participate in the struggle he so meticulously chronicles.

WILLIAM H. MCNEILL
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JERZY KOZEŃSKI. *Agresja na Jugosławię, 1941* [Aggression against Yugoslavia, 1941]. Summary in English. (Studium Niemcoznawcze Instytutu Zachodniego, number 32.) Poznań: Instytut Zachodni. 1979. Pp. 213. 55 Zł.

Jerzy Kozieński, a scholar at Poznań's Western Institute, specializes in the contemporary history of Eastern Europe. In this study, a work of diplomatic history, Kozieński describes the steps that led to the Nazi domination of southeastern Europe. He develops the topic along three major lines: the German-Italian confrontation and cooperation, Hitler's preparations for a war against the Soviet Union, and Berlin's policies toward Belgrade. The narrative takes a rather classic—one is tempted to say, dated—approach to the issues. History is made by personalities, foreign ministries, and diplomats. Economics, social forces, and political powers do not play a significant role in the scenario and sometimes are neglected altogether. Furthermore, the analysis of events preceding the military defeat of Yugoslavia by the Axis powers and the German invasion of the Soviet Union offers few new insights. A historian familiar with the region will derive only modest benefits from reading this slender volume.

Kozieński's sources are not unique, either; he uses mostly secondary and published primary material. Of the unpublished sources, the most noteworthy documents are those of the Central State Archives of the DDR and from the Slovak Central State Archives. No Yugoslav original papers were used. Wartime press and much of the contemporary emigrant literature—such as reminiscences of the Croatian Ustashi—were either overlooked or ignored.

Several significant details stand out in this book and justify its reading. Potsdam's files cast a new and intriguing light on the relations between Italy and the Third Reich; the story of the creation of the independent state of Croatia profits much from this provenance. (The slight attention paid to internal events in Yugoslavia and to radical movements like the Ustasha certainly reduces the value of the volume.) The not infrequent disagreements with postulates and clichés of Yugoslav historiography are interesting and rewarding. The disagreements include, among others, the evaluation of the political attitudes of Cvetković's government vis-à-vis Berlin (p. 74), the description of Maček's activity in April 1941 (pp. 127–35); the comments on the origin and role of the Chetnik-Mihailović resistance (pp. 163–66, and 174); and the discussion of the claim that

Yugoslav partisan warfare constituted the first "second front" in Europe (p. 168). The tenor of the writing is significantly non-Marxist, and the terminology and phraseology of historical materialism can hardly be detected.

The reviewed work is mainly a contribution *pro domo* to Polish historiography and to the Polish appreciation of the Second World War.

YESHAYAHU JELINEK
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HANS-JOACHIM HOPPE. *Bulgarien—Hitlers eigenwilliger Verbündeter: Eine Fallstudie zur nationalsozialistischen Südosteuropapolitik*. (Studien zur Zeitgeschichte, number 15.) Stuttgart: Deutsche Verlags-Anstalt. 1979. Pp. 309. DM 36.

This monograph is the fifteenth volume in a series of twentieth-century German internal and diplomatic history being published by Munich's *Institut für Zeitgeschichte*. Hans-Joachim Hoppe sees the significance of the German-Bulgarian relationship in the unique "independence" the Balkan kingdom exercised during the Second World War; hence the "eigenwilliger"—self-willed—of the title. Outstanding among Bulgaria's "self-willed" actions were the country's nonparticipation in the Jewish Holocaust—at least within the prewar boundaries—and its refusal to join the war against the Soviet Union with even a token force. Yet because a number of other "unique" relationships existed between the Third Reich and small European countries during the war, for example, Spain, Finland, Hungary, and Rumania, it would seem that, although Hoppe's points on Bulgaria's independence of action are partially valid, the value of his work lies in its description of Bulgarian-German relations rather than in revealing some universal key to German wartime diplomacy.

Hoppe has thoroughly exhausted relevant German archival material and pertinent secondary works in German, English, and Bulgarian. He has used some printed Bulgarian primary materials but has neglected other important accessible documents such as the published extracts of Prime Minister Bogdan Filov's diary, the minutes of the national assembly, and the official gazette. (Available Bulgarian archival material from this era, especially for use by Western scholars, is of course severely limited.) A few minor errors have been carried over from the author's sources. None of these significantly damage the work, and only one needs to be called to attention. The barracks conspiracy of April 1942 was apparently not related to the espionage trial of General Vladimir Zaimov. Filov's diary makes this quite clear, although the German embassy reports confuse both matters. Hoppe, like Marshall Miller,

has continued this mistaken amalgamation. Both affairs are still rather mysterious. Surprisingly there has never been more than scant mention of the barracks uprising (allegedly led by Communists) in postwar Bulgarian historiography; and, although much has been written about war hero General Zaimov, the precise nature of his activity has not yet been fully revealed.

Much of Hoppe's material has been covered in recent English monographs and articles by Miller, Nissan Oren, and this reviewer, but this new book still is valuable even for English-speaking scholars. Hoppe's detailed accounts of German-Bulgarian diplomatic dealings both in the thirties and during the war are unmatched by the previous literature. Hoppe also spends significant and profitable effort in examining the important German-Bulgarian economic relationships. Scholars will find his appendix listing members of Bulgaria's governments, his detailed explanatory notes, and his extensive bibliography of interest as well. In short *Bulgarien—Hitlers eigenwilliger Verbündeter* is a welcome addition to the scholarly literature on twentieth-century Bulgaria.

FREDERICK B. CHARY
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GERALD J. BOBANGO. *The Emergence of the Romanian National State*. (East European Monographs, number 58.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press. 1979. Pp. xiii, 307. \$17.00.

Gerald J. Bobango sets out his purposes as (1) to introduce Western readers to a critical period in nineteenth-century Rumanian history, drawing on an impressive array of Rumanian scholarly works that other Western historians have ignored; and (2) to illustrate the pattern of the development of nationalism in Eastern Europe and of liberation and nation-building in the Balkans. On the first count he has no need to apologize for the shortcomings of Western scholarship. A new generation of historians since that of the elder Seton-Watson and Thad Riker has moved into the field of nineteenth-century Rumania and made good use of the voluminous work of Rumanian scholars—who have flowered with the official cult of the national past, even though theirs remains a controlled profession. The dialogue between Rumanian and American historians has been a continuing phenomenon over the past decade. Nor has the author any need to apologize for his own work, which stands as a solid, original contribution and not merely a summary of the research of Rumanian scholars.

On the second count, the pattern of nationalism, Bobango does little more than try to see how Rumanian cultural and political developments fit the

general theories of nationalism worked out by Kohn, Snyder, and others and to make a rough comparison with other Balkan nations. The analysis is not carried very far, and indeed it is a little difficult to draw any profound conclusions from the very limited period the book covers, the seven confused and strife-torn years from Prince Alexandru Cuza's double election in the two Danubian Principalities in 1859 to his forced abdication in 1866, except perhaps that they illustrated the growing pains of the "emerging national state" (to borrow the wording of the title).

The book's great merit lies not in the realm of social theory but in its painstaking, detailed, and objective account of Cuza's reign. Building on the pioneer work of A. D. Xenopol and on C. C. Giurescu's recent biography, drawing also on source material from the Cuza archives and elsewhere, the author covers the main issues (merger of the two principalities, relations with outside powers and with "liberation movements," secularization of the Greek monasteries, the experiment in parliamentary government and its breakdown, the Bonapartist coup d'état, and the land reform) and covers them well. It is refreshing, too, that his judgments are bold and clear, not cloaked in cautious qualifications. For example, he is sympathetic to Cuza and his moderate colleagues who practised the art of the possible, especially Mihail Kogălniceanu, probably the only real statesman of the time. On the other hand the so-called radical-liberals, such as Ion Brătianu and C. A. Rosetti, earn no laurels. Those two were to play leading roles in Rumanian political life long after Cuza passed from the scene, but on the record of these years they come through as selfishly ambitious, hypocritical, and dishonest.

Because he has done his research so thoroughly, but avoided getting bogged down in it, Bobango is able to catch the spirit and tone of Rumanian politics, and in that sense much of what he writes has meaning beyond the short span of the years of Cuza's reign.

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EMILIA ȘONEA and GAVRILĂ ȘONEA. *Viața economică și politică a României, 1933–1938* [The Economic and Political Life of Rumania, 1933–38]. Summary in English. Bucharest: Editura științifică și enciclopedică. 1978. Pp. 330. 15.50 L.

MIHAIL RUSENESCU and IOAN SAIZU. *Viața politică în România, 1922–1928* [Political Life in Rumania, 1922–28]. Bucharest: Editura politică. 1979. Pp. 261. 11.75 L.

Here are two more much-needed monographs on early twentieth-century Rumania, based on essen-

tial published and unpublished material that has lain fallow since the end of World War II. The authors, especially Emilia and Gavrilă Șonea, have assiduously investigated a rich variety of sources in the *Arhivele Statului București*, the very important *Arhiva Institutului de studii istorice și socialpolitică de pe lângă Comitetului Central al Partidului Comunist Român*, and elsewhere. Unfortunately, the cryptic archival citations of the Șoneas, Mihai Rusenescu and Ioan Saizu's sloppy footnotes, and the lack of separate bibliographies lessen the potentially great usefulness of both volumes to the scholar.

This sort of inattention to the reader's viewpoint should not, however, obscure the very real contributions of both these works. *Viața politică în România, 1922-1928* is particularly valuable for its description of the political orientations and programs of the parties. Its authors document a large number of examples of election abuses and party maneuverings, but they emphasize, quite rightly, the process of consolidation and settling-down that characterized Rumanian political life in the mid-1920s.

By contrast, the Șoneas' subject is almost unbelievably grim. Although economic recovery did begin in the mid-1930s, their real story is the increasing demoralization of politicians and the disintegration of even the forms of parliamentary government. In spite of the changes in old parties and the proliferation of new groups and political theories, *Viața economică și politică a României, 1933-1938* contains little background on party beliefs, membership, or programs. It does, however, present considerable new detail on legislation, electoral manipulations, interparty negotiations, intraparty bickering (especially the quarrel between *bătrânii* and *ținerii* in the National Liberal Party), and on the royal camarilla's use of political instability to further King Carol's dictatorial plans.

All four of these authors share certain distinctive strengths, weaknesses, and attitudes toward historical writing. They shine in straightforward, well-documented narrative. Sometimes, however, their ideological stance is rather obtrusive. Both books, predictably, devote substantial chapters to the Communist Party, without comparable space being given to other small groups. The rather mechanical repetition of set phrases ("the deepening contradictions within the bourgeois-landlord class") have a deadening effect on style. Further, a common anti-Great Man orientation results in little information or evaluation in either book of such pivotal figures as Ionel Brătianu or the two kings. Statements of motive tend to be relegated to footnotes.

For the most part, however, these predispositions do not mar the authors' clearly genuine attempts at historical objectivity. Except for an excessively shrill and negative tone adopted by Rusenescu and Saizu

toward Iuliu Maniu, the judgments given on the work of "bourgeois" parties and governments are calm, balanced, and well supported. The authors are all well aware of the complexity of the issues.

My principal reservation about these books is related to neither their accuracy nor their objectivity, but rather to the curiously traditional nature of their approach. Ironically, while Western writers of Marxist views have enriched historical studies by taking into account previously neglected social and economic factors, many Rumanian political historians seem to belong to an earlier and narrower school, focusing exclusively on the political activities of a small elite of party leaders.

As a result, some very intriguing questions simply never are raised. For example, the authors of both these books merely assume the close connection between National Liberal politicians and the major economic enterprises in Rumania, rather than probing into it, on the lines, say, of the innovative work of Al. Gh. Savu (*Sistemul partidelor politice din România, 1919-1940* [1976] and *Dictatura regală* [1970]). Savu began to explore the ramifications of the political-economic network for the mid-1930s, but nothing of the sort has been done for the 1920s, and much more information still is needed on the 1930s. King Carol's political takeover would be much clarified by data on specifically how the Malaxa-Auschnitt-Blank group of industrialists around the king and Prime Minister Gheorghe Tătăreanu managed to breach the old Brătianu liberal economic monopolies. The Șoneas' chapters on the economy are not sufficiently integrated with their political chapters. Although they allude to both domestic and foreign pressures from the right and, like Rusenescu and Saizu, are very critical of foreign economic penetration, the Șoneas nowhere consider the identity of the foreign investors or their political significance. This sort of imaginative analysis is sorely wanted.

Nonetheless, despite their shortcomings, these two books, because of their detailed presentation of many of the basic facts of the period, will surely be required reading from now on for anyone concerned with interwar Rumanian political life.

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VÁCLAV ZÁČEK. *Josef Václav Frič. (Odkazy Pokrokových Osobností Naši Minulosti.)* Prague: Melantrich. 1979. Pp. 391. Kčs. 35.

Because the Czechs lack a distinct revolutionary tradition in modern history, post-1948 Czech historiography has understandably highlighted Czech individuals who were somehow connected with "progressive," democratic, and revolutionary move-

ments in Europe. Josef Václav Frič (1829–90), romantic poet-dramatist and revolutionary, was such an individual. The son of a Prague lawyer, Frič was a university student when the Czech revolution broke out in 1848. He joined the young Prague radicals, participated in the June Uprising, and helped organize the ill-fated May Conspiracy of 1849. His penalty for these patriotic activities was imprisonment and eventually exile to Transylvania. In 1859 Frič left Austria for what turned out to be a twenty-year odyssey, half of which was spent in promoting revolution against Austria and the other half in seeking repatriation with his family in Prague. Successful only in the latter, he lived out the last decade of his remarkable but tragic life on Czech soil.

Václav Žáček, the leading authority on nineteenth-century Czech-Slavic relations, has chosen to focus primarily on Frič's political and revolutionary activities of the 1860s, thus filling a significant gap in the literature on Frič. The value of this excellent monograph is enhanced by the fact that Žáček bases it partially on the first systematic study of the rich personal and family papers of Frič, which were made available to researchers only after World War II. Žáček painstakingly details Frič's relentless efforts to have the Czech question placed on the agenda of the European revolutionary movement. Frič made contact with Bakunin, Garibaldi, Herzen, and Kossuth, as well as with émigré German and Polish revolutionaries. With little success he attempted to tie Czech emancipation to the anti-Austrian Italian unification movement. When the Poles revolted in 1863, Frič, a lifelong Polonophile, urged the Czechs to support the rebellion in the hope that the Poles, once free, would help to liberate the Czechs. After the Polish debacle, he participated in a project to interest Garibaldi in an anti-Austrian coalition of Croats, Czechs, Hungarians, and Poles, which was never realized. In the Austro-Prussian War of 1866 Frič was involved in a plot to provoke a Czech uprising, in return for which Bismarck was to reward the Czechs with independence. However, the revolt never took place, and the defeat of Austria resulted in the Austro-Hungarian Ausgleich, which was extremely detrimental to the Czech cause.

Although Frič was certainly the single important Czech revolutionary figure in the nineteenth century and did much to acquaint Europeans with the plight of the Czechs, the prominent place in the history of modern Czech national and political life that Žáček assigns to Frič is questionable. Frič's repeated calls for Czech liberation from Austria met with scant support either from abroad or from the Czechs themselves. And, as Žáček points out, Frič's intense hatred of Austria and the Habsburgs led him to collaborate with some unreliable allies and to adopt at times unwise policies and tactics. A revolutionary without a party, Frič remained well

outside the mainstream of the Czech national movement, and his goal of Czech independence was left for a future generation to accomplish under much more favorable circumstances.

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JON BLOOMFIELD. *Passive Revolution: Politics and the Czechoslovak Working Class, 1945–1948*. New York: St. Martin's Press. 1979. Pp. 290. \$22.50.

Jon Bloomfield offers an interesting angle of approach to the ascendancy of the Czechoslovak Communist Party to undivided power in the period 1945–48. His focus is on working-class support for the party's policies. With the help of a fairly detailed reading of existing literature, contemporary press, and such documentary evidence as can be marshaled, he is able to conclude quite confidently that the Czechoslovak working class "was a largely willing accomplice of the revolution, but not its driving force." This was "a passive revolution," to which the impetus "came from above and from abroad." As such, the revolution of 1945–48 "had enormous implications for democracy in the future socialist state," namely, political power did not pass fully into the hands of the workers but rather into those of the leadership of the Communist Party. For a professed Eurocommunist, such as Bloomfield, the aberration is evident. February 1948 in Czechoslovakia—the takeover—was neither the culmination of a masterful Communist strategy backed enthusiastically by the masses nor a simple coup d'état. What it was, he is less clear in saying.

Bloomfield's vantage point is not without merit. He can say and prove that the view of the Communist takeover as no more than the result of domestic and international power politics is much too simplistic. There *was* popular, above all workers', pressure for socialism and support for the Communists. The population was not wholly and intrinsically opposed to the plans that Gottwald and company laid out before them. In fact, the willingness with which large crowds assembled at the Communist leaders' beck and call, and even executed patently undemocratic and unlawful orders issued from Communist secretariats, casts doubt on Bloomfield's taxonomic usage. There was nothing "passive" about the street demonstrations, the general strike, the attacks on non-Communist party offices, the pressure on President Beneš, or the formation *en masse* of the so-called Action Committees and the Workers' Militia. Bloomfield may argue that large segments of the working class condoned the thrust of the activists while not steering the course of the revolution themselves. But then, a truly working-class revolution, self-managed, as it were, by the

toiling masses themselves, is a bookish phenomenon. In real life things simply do not happen that way.

To this reviewer it is anyway not the adjective but rather the noun that calls for clarification. The Communist road to power in Czechoslovakia was undoubtedly "active," not "passive." But was it a revolution? Or a coup? One might perhaps profitably distinguish between the process of transformation that occurred in Czechoslovakia from 1943 (Beneš's first trip to Moscow) to, say, 1949-50 (the consolidation of a Soviet-type system) and the actual events of 1947-48, especially the planning and execution of the February takeover. Whereas one would not hesitate to perceive of the six or seven years of societal transformation as revolutionary—they saw the institution of a new sociopolitical and economic order—the shorter conflict of 1947-48, central as it was to the long-term process, had many of the characteristics of a sharp and swift coup d'état. The one important exception was that the size of the group of its planners and executors exceeded the usual small numbers of dedicated putschists. Precisely because the February 1948 events had a wider context, both internationally and in terms of popular participation at home, their designation as a coup is inadequate. The concept of a *takeover*, in the sense of assumption of full, undivided control, would seem to possess the most apposite descriptive quality.

All this taxonomic quibble would of course remain without much benefit should we lose sight of the content of our nominal devices. It pays to the historians of Communism to practise "realism," not "nominalism." Bloomfield's book should be read as a useful bringing together of a wealth of facts about the participation of the Czechoslovak workers—or the lack of it—in the process that led to the institution of a Communist order in their country. His conceptual framework need not be accepted: it has logical holes, and it makes the mistake of virtually all idealists in passing judgments in the name of a utopia, in this case a socialism that would be neither social-democratic nor Stalinist but somehow democratic and Marxist at once.

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TADEUSZ WYRWA. *La pensée politique polonaise à l'époque de l'humanisme et de la renaissance: (Un apport à la connaissance de l'Europe moderne)*. Preface by PIERRE CHAUNU. Paris: Librairie Polonaise or Poets and Painters Press, London. 1978. Pp. 68¢. 110 fr.

Anyone writing in a Western language about the *terra incognita* of medieval and early modern Poland is faced with the choice of either producing a con-

cise but narrow monograph that non-Polish readers will have difficulty relating to wider historical concerns or else attempting an all-encompassing treatment that supplies the broad narrative background with which those who do not read Polish cannot be familiar. Wisely—but at the price of asking the reader to go through a very hefty volume—Tadeusz Wyrwa has assumed the obligation of being comprehensive and comparative. He introduces his reader to many of the complexities of Polish history down to 1600, and his comparative sweep ranges from Spain to Russia. This is truly an ambitious book.

One hesitates to suggest that so formidable a study oversimplifies and lacks thoroughness; yet, regrettably, it does. Wyrwa overlooks important modern Polish and Western scholarship (major names absent from the bibliography include Brock, Piąza, Schramm, Sucheni-Grabowska, Weintraub, and Williams). Readers may find disturbing the constant resort to broad, sometimes tendentious or obsolete, abstractions, such as juxtaposing medieval to "modern" Renaissance ideas and portraying Polish-Russian conflicts as the confrontation between an aggressive despotism and a peaceloving *Rechtsstaat*, thus ignoring the Jagiellonians' and Batory's expansive policies. Seeing Polish preference for Calvinism over Lutheranism simply as an antipathy to German authoritarianism may be dated. Wyrwa might also have considered the great importance of Wittenberg and Königsberg universities for the Poles, the political and social outlook of Polish Calvinism, and the disillusionment that many szlachta felt toward Calvinist authoritarianism after 1560. Some of these matters could have been illuminated by the use of recently published synod protocols.

Wyrwa rightly emphasizes the importance of efforts at legal codification as a factor in the formation of szlachta political ideas during the sixteenth century. Much more, however, might have been done to relate legal reform to the Execution of the Laws movement, the protracted drive by the Sejm's lower chamber to effect restoration of alienated royal domains and restrain overmighty magnates and churchmen. The Execution movement was closely linked to szlachta appeals for royal support of the Reformation and union with Lithuania. Indeed, until 1572 almost all Polish political writings dealt with questions raised by the calls for Execution or Reformation. Contemporary West Europeans would have seen the Execution movement as an effort to defend "the police" of the commonwealth. Wyrwa conveys little of the sense of times out of joint that pervades Sejm protocols and the writings of the prolific gentry publicist Mikołaj Rej—sources unaccountably not exploited in this book.

Late medievalists and early modernists will nevertheless find much of value here. Wyrwa is good on a number of important figures—the conciliarists,

Jan Ostroróg, Stanisław Orzechowski, Jan Kochanowski, and especially the Erasmian political theorist Andrzej Frycz Modrzewski. Above all, this book should make Renaissance scholars more anxious to extend eastward the conventional geographical boundaries of their field.

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KAY LUNDGREEN-NIELSEN. *The Polish Problem at the Paris Peace Conference: A Study of the Policies of the Great Powers and the Poles, 1918-1919*. Translated by ALISON BORCH-JOHANSEN. (Odense University Studies in History and Social Sciences, number 59.) Odense, Denmark: Odense University Press. 1979. Pp. 603. 120 KR.

This is a very important study. Even though much has been written about the Polish settlement in Paris, Kay Lundgreen-Nielsen, a young Danish historian, has produced a highly original monograph on this topic. Covering roughly the period from the 1918 armistice to the signing of the Treaty of Versailles, this is the first book based on such exhaustive documentation drawn from British, American, French, and Polish archives and methodologically conceived along novel lines. Unlike other historians, Lundgreen-Nielsen did not confine his very detailed analysis to the policies of the great powers but examined extensively those on the Polish side. His claim that only a study of Allied-Polish interaction allows us to get a full picture is entirely vindicated.

The *Polish Problem* is not an easy book to read, and the English translation could have been better. Nor is the index very helpful. But these are minor flaws. The author has done his best to provide the reader with guideposts in the form of short conclusions at the end of each section. To the specialist, he has given a richness of documentation and insights that is most rewarding. The 150-odd pages of footnotes that follow some 430 pages of text are a mine of information. Lundgreen-Nielsen has thoroughly digested and mastered the material, and in presenting it displays a remarkable precision and a high degree of accuracy.

The author, however, has done much more than to put together, in a judicious and balanced way, a vast amount of material. His main achievement lies in drawing important conclusions and advancing new interpretations. Lundgreen-Nielsen has demonstrated, for instance, the existence of a broad variety of views within the British, American, and, to a lesser extent, French groups of experts, a fact thus far either unappreciated or glossed over. Thus, he could show the machinery of the process of making decisions by the Big Three. Concerning the Polish side, the author has rejected the prevalent view of a

de facto division of labor between Piłsudski and Dmowski, the former concentrating on winning frontiers in the east, the latter obtaining the western borders at the peace conference. Lundgreen-Nielsen shows convincingly that Piłsudski clearly perceived the connection between the settlements in the west and the east and was concerned with both. The thesis that the dualism in Polish foreign policy, of which the Allies were perfectly aware, damaged Poland's case is persuasive and perceptive. The stress on Dmowski-Paderewski differences is amply documented.

A short review cannot do justice to the many important points Lundgreen-Nielsen makes in his book. In view of the fact that neither he nor his publishers are Polish, the minimal number of linguistic errors is very much to their credit. The author's evenhandedness in the treatment of an exceedingly difficult topic deserves the highest praise. While I would hesitate to call any book the last word on any subject, I consider Lundgreen-Nielsen's work to be *the* book on the Polish problem at the Paris Peace Conference.

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SEWERYN AJZNER. *Związek Związków Zawodowych, 1931-1939* [The Union of Trade Unions, 1931-39]. Warsaw: Książka i Wiedza. 1979. Pp. 458. 100 Zł.

Polish historiography has been recently enriched by a number of pioneer studies allowing better understanding of the political-economic mosaic of the pre-1939 Poland. Seweryn Ajzner, a reputable Communist Party historian, devotes his latest monographic work to the Union of Trade Unions (ZZZ), a Polish variety of *Zubatovshchina* and a rather little-known chapter in Polish history.

The ZZZ was a powerful labor organization of the 1930s, established to channel trade union activities toward progovernment support, but that by 1939 became the loudest exponent of the most radical circles of the *Sanacja* left. Its origins were rooted in the 1926 split in the National Workers' Party (NPR) and Józef Piłsudski's influence in the Polish labor movement.

By 1930 influential circles within the Piłsudski government came up with an idea to unite all Piłsudskian trade unions and labor organizations. It was hoped that the new organizational body, by cleansing its members from any party influences and creating a formidable challenge to the Polish Socialist Party (PPS), could consolidate the labor movement under the banner of cooperation with the *Sanacja* camp.

This task was entrusted to Jędrzej Moraczewski, a former member of the Piłsudski cabinet and a former member of the PPS. In May 1931 the diffi-

cult job was completed, and the newly created ZZZ with its one hundred and twenty to one hundred and thirty thousand members became the third largest labor union organization in Poland. It was comprised of some thirty trade unions nationwide.

The honeymoon did not last long, however. By 1933 a strong rift developed between the ZZZ and the government. The main conflict was over the antilabor laws sponsored by the authorities, and the ZZZ's liberal proposal for dealing with the economic crisis. The ZZZ leadership and its representatives in the Sejm repeatedly criticized the government's social policies and warned against the workers' rejection of the capitalist system. At the same time, the organization took a very strong stand against the danger of fascism as exemplified by the rise of the National Radical Camp (ONR). It loudly condemned antisemitic excesses throughout Poland and anti-Ukrainian discrimination. In 1937 the ZZZ added to its propaganda arsenal the slogan "people's democracy." This soon led to unjust accusations of pro-Communist sympathies and ultimately to the demise of the ZZZ.

Ajzner's work is of considerable value to students of Polish affairs. Although a Marxist scholar, his work presents a vivid picture of the activities of a dynamic syndicalist organization armed with a formidable program of social, economic, and political reform. A few exaggerations made about the importance of the Communist movement in Poland at the time do not mar seriously this valuable book, for in addition to a well-researched study of a Polish radical labor organization, the reader gets a new insight into political developments in Communist Poland.

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L. V. CHEREPNIN. *Zemskie sobory russkogo gosudarstva v XVI-XVII vv.* [*Zemskie Sobory of the Russian State in the Sixteenth and Seventeenth Centuries*]. Moscow: Nauka. 1978. Pp. 416. 3 r. 20 k.

This posthumous work of L. V. Cherepnin is a summing up of a long-term study of the Muscovite *Zemskie sobory* (nine earlier essays in different languages on this topic have been contributed by him since 1960). In this book the author traces, first of all, the historiographical and methodological background of his topic in great detail. Then in eight chronologically arranged chapters, he gives us a most exhaustive factual history of Muscovite representative assemblies from 1549 to 1683 (57 *sobory* in all). In this presentation he follows the available sources as closely as possible, submitting them to a rigorous critical analysis. While providing a factual account of these assemblies, the author also seeks to trace the

evolution of the Russian *Ständestaat* ("russkaia soslavno-predstavitel'naia monarkhiia," [p. 4]). Finally, in the concluding chapter of his book, Cherepnin undertakes a rather cursory study of more general problems pertaining to the history of *zemskie sobory*, such as their classification and periodization (a most important concern of Soviet historical science!); their relation to autocracy and to the people; the problems of "feudal ideology" (the reviewer's quotation marks!) concerning the *sobory*, as well as the international situation of Muscovy connected with them; and, finally, a very brief (four pages) comparison of the *zemskie sobory* with the "soslavno-predstavitel'nye institutions of other medieval European countries" (p. 397; "medieval" in the broad Soviet sense, the chronological range of the institutions mentioned by the author going from 1188 to 1650). Cherepnin himself, in his preface, did not go beyond considering this comparative survey as only a most modest *Problemstellung*.

While both the historiographical and the factual part of this book hardly call for any substantive criticism even on the part of a non-Marxian historian who can only recognize the great value of Cherepnin's last performance, the ideological and the comparative aspects of the book call for certain reservations. The conceptual framework, which leads to considering representative assemblies as a feature of the "feudal state" (p. 396) or even "mature feudalism" (p. 397), confuses even the most attentive and unprejudiced Western reader. It is true that so far Western historians have neglected the representative assemblies of Muscovy. No place has been found for them in *Ständische Vertretungen in Europa im 17. und 18. Jahrhundert* (1969). (In A. Marongiu's *Medieval Parliaments* [1968], quoted by Cherepnin on page 397, the Muscovite assemblies have not been mentioned either, but there were none in the Middle Ages in the Western European understanding.) So, the author's comparative correction is called for, and in this he has an illustrious predecessor: Otto Hintze, who in his "Weltgeschichtliche Bedingungen der Repräsentativ-verfassung" (*Staat und Verfassung* [1962]) accepted the classification of seventeenth-century Muscovy as a *Ständestaat*, albeit with the reservation that it had "eine viel schwächere Bildung von ständischer Repräsentation," which lacked the corporate independence of the Western estates. Such is also this reviewer's point of view, one with which, I am afraid, the late Soviet scholar might differ. All in all, however, Cherepnin's *Zemskie sobory* represents a most worthy conclusion to his remarkable scholarly career, and one must hope that it will contribute to the proper understanding of the governmental structure of Muscovy in the two centuries prior to Peter the Great.

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V. G. CHERNUKHA. *Vnutrenniaia politika tsarizma s serediny 50-kh do nachala 80-kh gg. XIX v.* [The Domestic Policy of Tsarism from the Mid-1850s to the Early 1880s]. Leningrad: Nauka. 1978. Pp. 246. 2 r.

This book is devoted to three topics discussed in government and public circles during the reign of Alexander II. Each topic is treated in a separate chapter that parallels the others chronologically. Chapter 1, "The Autocracy and Representative Institutions," studies the idea of introducing a representative body at the national level to participate in legislation. This section analyzes the interplay between demands made by social groups in periods of unrest and responses by bureaucrats who drafted reform proposals to calm the social unrest of the moment.

Calling bureaucratic initiatives to create representative institutions "government constitutionalism," V. G. Chernukha divides the period 1861–82 into four subperiods during which five initiatives occurred. Each subperiod is treated as a subchapter focusing on proposals submitted by P. A. Valuev (1861–63), Grand Prince Konstantin Nikolaevich (1865–66), P. A. Shuvalov (1866–75), and M. T. Loris-Melikov and N. P. Ignatiev (1878–82).

Based upon printed and archival diaries and memoirs of major government officials, correspondence among the parties concerned, and very broad reading in the contemporary and recent secondary literature (but in Russian only), this section constitutes the most systematic, detailed study of this topic available in one volume.

Chapter 2, "Unity of the State Administration," traces government and public discussion of reforming the central executive agencies during the same time period. Three subchapters are devoted to discussion of that question: (1) in 1857 during the creation of the Council of Ministers, (2) in 1861 during formulation of the Council's procedures and area of jurisdiction, and (3) in the period 1862–82.

Chapter 3, "An Income Tax and Its Socio-Political Essence," is based, in the first subchapter, upon archival materials that reveal substantial government consideration of introducing an income tax from 1862 to 1867. The second subchapter discusses zemstvo responses to proposed reform of the soul tax in 1871, and the third traces the idea of an income tax through its consideration in the Valuev Commission in 1872 and its fate to 1880.

This chapter calls attention to a topic even more important in Russian political thought than the author indicates. The public debate on the soul tax reform in the provincial zemstvo assemblies and in the press produced a general acceptance among literate, aware Russians of the idea of an income tax to replace the existing tax system. After 1871 an income tax to be paid by all classes was a standard plank in the liberals' platform.

By choosing to devote two pages to B. N. Chicherin's views on the income tax (the "liberal" whom Soviet scholars always parade out when a "liberal view" is desired), the author provides an atypical glimpse of zemstvo reaction to the government's proposed reform. In volume 22, part 1, of the Special Commission's *Trudy* (the author cites part 2 on page 224) Chernukha could have found many proposals radically different from Chicherin's. An example is the lament by the Nizhegorod County Zemstvo Board that the government's proposal "considers the word 'taxpayer' to be a synonym for the word 'peasant'" (pp. 96–97). The board favored a tax upon all classes, as did thirty of the thirty-two provincial zemstvos that considered the draft.

A couple of lapses in tonal quality do not appreciably detract from the value of this serious, soundly researched, and generally well-balanced addition to the literature on Russian political and social ideas during the reign of Alexander II.

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V. A. TVARDOVSKAIA. *Ideologiya poreformennogo samoderzhavii (M. N. Katkov i ego izdaniia)* [The Ideology of Postreform Autocracy (M. N. Katkov and His Publications)]. Moscow: Nauka. 1978. Pp. 277. 2 r. 20 k.

Soviet historiography of conservative nationalist thought, politics, and personalities has received considerable attention since the pioneering efforts of P. A. Zaionchkovskii in the mid- and late 1960s. V. A. Tvardovskaia's work is within this tradition. Her short monograph is divided into seven parts: an introduction in which the author makes a case for an actual ideology of postreform autocracy with M. N. Katkov as its primary ideologue, and in which she opens a polemic with such Western historians as R. F. Byrnes, E. C. Thaden, and M. Katz; chapter 1, in which Tvardovskaia tries to prove that Katkov's leading publication, *Moskovskie Vedomosti*, is a governmental organ; chapter 2, in which she discusses the socioeconomic ideas of "conservative" (the quotes are hers) publicism of the 1860s through the 1880s; chapter 3, in which she deals with the "reactionary reformism" of M. N. Katkov of the 1860s and 1870s; chapter 4, in which Tvardovskaia discusses "the ideologue of autocracy in the period of the revolutionary situation in the late 1870s and early 1880s of the nineteenth century"; chapter 5, which treats counterreforms; and a short conclusion. The work is enhanced by an index of names and by footnote citations on the bottom of the pages, but it does not have a bibliography.

Tvardovskaia's case for Katkov as an ideologue of postreform autocracy is only partly convincing. Certainly there were others, such as K. P. Pobedo-

nostsev and D. A. Tolstoi, who played an equally strong role on the right wing of the government. Also the author's playing down of "conservative nationalism" and "conservative Westernism" and her failure to take into account the origins of these features in Katkov's childhood and early attraction to Schellingianism make Katkov appear only half real. She has a tendency to underestimate the impact of the Polish rebellion of 1863 and the corresponding integral Great Russian nationalism of the Moscow publisher.

Her polemics with Western historians are forced. She denies the paradox of modernization and conservative nationalism, which Western historians such as Pipes, Byrnes, Thaden, and Katz have emphasized in their works. This denial has little or no substance. She escalates her whole polemic to the level of an attack on those in the West who are not convinced by historical inevitability, especially the inevitability of the October Revolution (pp. 12-13).

On the positive side of the ledger is Tvardovskaia's use of manuscript and archival materials, which in the past Western scholars were not able to use. Also the writing of a quasi-biographical work on Katkov by a Soviet scholar is a gigantic leap forward for Soviet historiography since the beginning of the 1960s.

Tvardovskaia de-emphasizes the role of personality in history, especially in her attack on Byrnes and Katz. She also underestimates the importance of Katkov's nationalistic struggle with such dynastic cosmopolitans as Valuev and tries to show that the two men were merely two sides of the same coin (p. 72). Her arguments on this score are not very convincing.

Tvardovskaia is at her best when discussing Katkov's economic policy, but she is not far sighted enough to see Katkov's subordination of class, social, and economic interests to integral nationalism. She flatly refuses to see, for instance, that Stolypin's "wager on the strong" was a distant echo of Katkov's desires several decades before. The author further attacks Thaden and Katz for emphasizing Katkov's proclivities toward the industrialization of Russia, especially the development of railroads. She indicates that Katkov only thought of railroads as auxiliaries to agriculture, but what she does not say is that the Moscow editor was well aware of the mining and metallurgical industrial development required to support railroads.

She correctly observes that Katkov supported the interests of the Russian *pomeshchik*, but she does not mention the complexity of reasons he had for doing this. She does not indicate that this stratum was the most fervently nationalistic of all, especially after the Polish rebellion of 1863. Yet what Katkov really wanted was "*veseloslovnost'*," the unity of all estates under the crown.

Tvardovskaia also displays a genuine lack of in-

terest in Katkov's educational policies, "classicism against nihilism." In doing so she misrepresents what I said in my work on Katkov. She indicates that I said that classicism was a humanizing force, when I was only presenting Katkov's ideas on the subject.

In summation, V. A. Tvardovskaia's monograph is a welcome addition to the growing number of works, both Soviet and Western, on the conservative thought and politics of imperial Russia.

MARTIN KATZ

University of Alberta

JAMES C. MCCLELLAND. *Autocrats and Academics: Education, Culture, and Society in Tsarist Russia*. Chicago: University of Chicago Press. 1979. Pp. xiv, 150. \$14.00.

Modernization in Russia often meant borrowing from the West and in no area was this borrowing more faithful to the original than in education, particularly higher education. James C. McClelland points out that Russian education developed from the top down; the empire had an Academy of Sciences before it had grammar schools, and Russian universities became sophisticated centers of scholarship and research while the national literacy rate was the lowest among the European powers. By the end of the nineteenth century, the empire was second only to the United States in numbers of students enrolled in higher education, and the rigorous standards produced individuals capable of extraordinary achievement, especially in such fields as the natural sciences and mathematics.

The author notes, however, that the government was always uneasy with professors and students who resented interference by the state in the universities and challenged the social and political status quo. Moreover, the imperial government was never quite able to decide just what role the universities should play—whether they should be institutions for pure research on the German model or whether they should train persons of talent for the practical needs of Russian society—and this ambivalence together with the autocracy's fear of their revolutionary potential resulted in periodic tampering with curricula, faculty appointments, and student admissions. In the end, the universities emerged as centers for much of Russia's articulate disaffection, and by the early years of this century they became the launching pads for actions and demonstrations against the government.

This work raises a number of interesting questions concerning the place of education in imperial policy. One question of considerable importance is why the state did not develop a more practical system to serve the day-to-day needs of the empire. The author implies that both prestige and the belief

that scientific discoveries would contribute directly to the state's military power underlay this choice. While this undoubtedly had validity, it appears to discount the values of the scholarly community itself, which, if A. V. Nikitenko is to be considered typical, accepted pure scholarship and research as the natural and exclusive role for the university. Professors trained either in Germany or in the German academic tradition could hardly be expected to seek a university structure or purpose substantially different from that of their model, and this undoubtedly served also to narrow their views on what constituted appropriate preparation for university study, thereby critically affecting secondary curricula. One is tempted to wonder what might have been the result if the government had permitted the universities to govern themselves as Russian professors wished. In such a circumstance would the academic minds have been any more anxious than was the state to abandon their privileged role and substitute for it a mission of teaching peasants and artisans to become mechanics and accountants?

This work often tends to ramble, and the author makes allusions without regard to the needs of the more general reader. The system of primary education is treated in only cursory fashion, and McClelland pays insufficient attention to the vast array of schools not within the purview of the Ministry of Education, particularly those in the army, where practical curricula did evolve and where some of the most significant assaults upon illiteracy were made. Nevertheless, we must be grateful to the author for providing a framework for more specialized studies in the future and for provoking some very interesting questions that lie at the very heart of the history of the empire.

FORRESTT A. MILLER
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N. M. DRUZHININ. *Russkaia derevnia na perelome, 1861-1880 gg.* [The Russian Countryside at the Turning Point, 1861-80]. Moscow: Nauka. 1978. Pp. 286. 2 r. 50 k.

The succinct title of N. M. Druzhinin's latest monograph conveys a topic, a time, and a thesis. Note, however, that the "Russian countryside" it advertises is limited to the territory of the "main directing nucleus of the Russian state at the time" (p. 4)—thirty Great Russian provinces of central European Russia. The period covers the pivotal first decades of emergence from serfdom, decades spanning the interval between the "revolutionary situations" of 1859-61 and 1879-81.

The work essentially synthesizes, and to some extent supplements, previous scholarship on the con-

dition of the peasantry in the era of great reforms. The objective is to demonstrate concrete linkages between the emancipation, economic developments, and the emergence of a new political crisis. This is a complex task, but the author bears impressive credentials. Best known for his classic study of the 1837-41 reform of the state peasant administration, the indefatigable Druzhinin (b. 1886) has been a productive scholar of varied interests.

The first four chapters of his book detail economic and institutional aspects of the emancipation and describe the relative status of different categories of peasants. The remaining four deal with peasant landholding, the peasant economy, regional differences, and the crisis of 1879-81. A brief conclusion concisely summarizes the entire presentation. The sources cited are primarily archival and published documentary materials, but recent research of Soviet scholars is broadly utilized.

Druzhinin's contention that the peasantry fell under mounting stresses after emancipation rests on basically familiar ground. He points to initial land losses, excessive redemption payments, increasing inadequacy of allotments, disparity between land income and taxation, and social bias in state policies. Most of the argument has been well rehearsed in the literature, but Druzhinin moves it forward by emphasizing the early impact of these factors, assembling some new evidence, and offering some provocative reappraisals (for example, of peace arbitrators). Sixty tables of data provide information ranging from the number of peasants at the time of emancipation to the reduction of redemption payments in 1881. Despite some lapses (inadequate discussion of communal landholding; a top-sided view of peasant self-government), the work projects a persuasive picture of escalating economic pressures created and compounded by government policies.

Yet the picture has its brighter side. The emancipation, as Druzhinin repeatedly observes, secured the personal liberation of the peasants and simultaneously released great potential for economic development. Some significant gains, in fact, are shown to have been achieved in these decades.

The historical prospect, then, is one of countervailing tendencies. Economic growth was providing new and expanding opportunities for some peasants, but the burdens on the peasantry as a whole were increasing. Agricultural productivity was rising, but production per capita was declining. As Druzhinin presents it, the contradictions were the result of tensions between surviving elements of the old feudal system and an emergent capitalist formation.

If peasant economic difficulties are convincingly tied to the emancipation, they nonetheless fail to provide an adequate explanation of the 1879-81 crisis that Druzhinin himself describes as "sociopolitical."

tical." The chapter dealing with this topic is the most problematic in the book. By all accounts (including this one) the peasants were not politically active; it was the terrorism of the populist intelligentsia that created the immediate crisis. And, as Druzhinin concedes (pp. 273-74), the radicals were a small group with weak ties to the peasantry. A severe methodological (or logical) weakness of the work at this point is its failure to demonstrate either that radical politics were directly related to peasant economics or that the government perceived them as such. The case is more proclaimed than proved.

Whether from fear of revolution or simply because peasant problems were becoming more evident, the government did take measures (both before and after the 1881 assassination of Alexander II) to alleviate the situation of the peasantry. With what results? Unfortunately, Druzhinin's story ends right here. Concluding pages set the stage for a tale of ongoing and deepening agrarian crisis, leading to the ultimate revolutionary situations of 1905 and 1917. But the book itself would suggest that in the early 1880s the future of the Russian countryside had by no means been foreclosed.

DOROTHY ATKINSON
Stanford University

P. G. RYNDZIUNSKII. *Utverzhenie kapitalizma v Rossii, 1850-1880 gg.* [The Consolidation of Capitalism in Russia, 1850-80]. Moscow: Nauka. 1978. Pp. 293. 2 r. 30 k.

P. G. Ryndziunskii has been an active, and frequently very perceptive, historian of the late-nineteenth-century Russian economy and society. The purpose of his latest monograph is to draw together the findings of specialized enquiries into industry, trade, finance, agriculture, and so forth, and to relate them to the formation of both bourgeoisie and proletariat during the early stages in the development of capitalism. The study is restricted to conditions in European Russia. The book's contribution lies not so much in bringing to light new evidence on the "consolidation of capitalism in Russia" in the 1850-80 period—though some archival data are presented and considerable use is made of a variety of nineteenth-century statistics—but in synthesizing a portion of the huge volume of Soviet scholarship on the topic.

The book is divided into three parts: the demise of serfdom; the formation of capitalist structures; and the formation of the classes of capitalist society. Chapters in the first part deal with the characteristics of trade and commerce, including the formation of a European Russian market, industrial development and the agrarian economy—all during

the decade or so prior to the emancipation of 1861. The final chapter in the section summarizes, yet again, the illiberal consequences of the emancipation and sets the stage for viewing, among other things, the reductions in cultivated area and the imposition of redemption payments as factors in fueling rural-urban migration and swelling the ranks of the lumpenproletariat. Emphasis throughout is placed on an orthodox interpretation of Marx. Thus, historians like G. P. Asaev, who have ventured to interpret the beginnings of industrial capitalism in terms of simple technological change rather than in terms of control over the means of production, are duly upbraided (pp. 25-26).

The second part of the book examines these same themes up to the 1880s. By this time more specialized market areas had emerged, much facilitated by the rapidly expanding railroad network, and with market specialization and industrialization came a complex system of regional interdependencies. The rapid development of rent relationships is accorded special attention as one measure of the intensification of capitalism—and by the 1880s there is no question that they were a prominent feature of the rural economy. Ryndziunskii regards the industrial revolution in Russia as having been completed by the mid-1880s. In the ensuing decades industrial capitalism intensified, to be sure, but the period 1850-80 was the critical one in terms of its taking root in Russia. While technical innovation in industrial production gained momentum, some spheres of production, such as textiles, still made extensive use of homeworkers (*nadomniki*) in the 1880s.

The final part of the study discusses the formation of bourgeoisie and proletariat. The growing concentration of industrial and merchant capitalism is highlighted. A simple correlation analysis clearly shows how closely the two are linked spatially throughout European Russia. Among the institutions administering credit, "such great spatial concentration . . . is evidence of the high degree of development of the capitalist system in the country" (p. 240). In describing the formation of the proletariat, Ryndziunskii focuses his attention on the conditions in the countryside forcing peasants to seek employment outside agriculture, either seasonally or permanently. It is clear that the link with the village is maintained. The question therefore arises: just what were the consequences of this continuing link with the village among so many urban factory workers in terms of their becoming fully assimilated into the proletariat? But Ryndziunskii does not stray from the orthodox view that a factory worker *ipso facto* is proletarian. This may make it easier to measure the development of the proletariat, but it does not do much to recreate an authentic picture of time and place. In summary, the book serves as a useful, but largely predictable, survey of

economic and social change during this important period in Russian history.

JAMES H. BATER
University of Waterloo

ROBERT N. NORTH. *Transport in Western Siberia: Tsarist and Soviet Development*. Vancouver: University of British Columbia Press and Centre for Transportation Studies. 1979. Pp. 364. \$22.00.

Robert N. North has produced a substantial monograph that embraces not only various modes of transport, especially railways, but also other aspects of economic development, including industry, agriculture, and trade, and their mutual relationship from the 1850s up to 1975. About one-third of the text treats the prerevolutionary era, with almost equal space being given to the periods before and after the coming of the Trans-Siberian Railway in the 1890s. The remainder is divided into chapters of about thirty pages each, dealing with successive stages of Soviet rule: to the end of the NEP period, the first three Five-Year Plans, the Second World War and the last years of Stalin's regime, the Khrushchev era, and finally the Brezhnev era. The last part of the book contains appendixes that present *inter alia* numerous meticulously researched and compiled tables and maps. There is also an abundance of maps throughout the text.

The main part of each of these chapters is devoted to a very detailed description of economic developments taking place during the period under discussion. The reader is treated to a wealth of facts and statistical data that at times becomes almost overwhelmingly informative. At the end of each chapter there is a brief summary and a discussion of continuity or change with respect to the preceding period. There are also attempts to compare and contrast the economic development of the region with that of other parts of the USSR, especially the Urals and Eastern Siberia but also some of the industrial areas of European Russia.

Because of the time span involved, the author's treatment of the pre-1914 era has to be somewhat superficial. He seems to be most at home in discussing post-1945 events. His sections on developmental problems in chapter 8 and on continentality and economic development in chapter 9 are the most analytical and broadly interpretive and hence will hold the greatest interest for the general reader. The discussion of recent developments in the Siberian oil industry will also be of considerable interest in view of recent world events. However, all parts of the book contain a wealth of factual and bibliographical information that will be welcomed by specialists wishing to delve more deeply into specific topics.

The author has an extensive command of his bibliography, both Russian and English (although he does not utilize several relevant secondary works on Russian railways in the nineteenth century). In addition to monographs and journal articles, the author draws upon tsarist and Soviet government publications. But complete, reliable, and consistent data for all periods and branches of industry are difficult to obtain. Therefore, on some specific points the author must resort to suppositions, nearly all of which seem quite reasonable.

In summary, North's book, although narrow in geographical focus, is well researched and informative. Many more such specialized works should be written if our understanding of Russian and Soviet economic development is to be increased.

RICHARD MOWBRAY HAYWOOD
Purdue University

MANFRED HILDERMEIER. *Die Sozialrevolutionäre Partei Russlands: Agrarsozialismus und Modernisierung im Zarenreich (1900-1914)*. Cologne: Böhlau Verlag. 1978. Pp. xviii, 458. DM 98.

The party of bombthrowers and agrarian protest has at last found a historian in Manfred Hildermeier. His valuable study provides a useful introduction to the works of Oliver H. Radkey and others who investigated the vicissitudes of the Party of Socialist Revolutionaries (PSR) during the Russian Revolution.

The monograph is based on a wide range of printed sources in Russian and other languages. In addition, Hildermeier has made good use of the PSR records in the International Institute for Social History in Amsterdam and of the Okhrana files at the Hoover Institution at Stanford University. On the other hand, he does not seem to have consulted the material on the PSR in the Archives Nationales and the Archives de la Préfecture de Police in Paris.

The book begins with an account of the attempts to form the party in the 1890s. This is followed by a detailed analysis of the party's program and stand on many issues, although not on the nationality, women's, and cultural questions. Three well-documented chapters are devoted to the spread of the PSR network on the eve of the 1905 Revolution, the role of the party in 1905-07, and the strengths and weaknesses of regional party organizations. A short account of party finances precedes an analysis of the background of PSR leaders and activists. Unfortunately, neither here nor elsewhere in the book does the author examine in depth the contribution of students to the development and image of the PSR. The remaining chapters deal with the PSR in the years of the Stolypin reaction and on the eve of the First World War. Hildermeier rightly pays much

attention to differences of opinion among prominent SRs and to the effects of terrorist activities in which the party engaged and which Hildermeier considers counterproductive.

In a regrettably brief conclusion, Hildermeier ponders over the reasons for the failure of the PSR, which, he thinks, was more prone to factionalism than other major political parties in Russia. The failure to grasp the importance of organization also hurt the PSR, although the author admits that the times were not ripe for an "agrarian mass party" (p. 400). The PSR program receives no accolade from Hildermeier. Its utopian character harmed the cause of Socialist Revolutionaries. A populist party could not act as a modernizing force in Russian society once capitalism had evolved beyond a certain point.

IVAN AVAKUMOVIC
University of British Columbia

L. P. LIPINSKII. *Stolypinskaia agrarnaia reforma v Belorussii* [The Stolypin Land Reform in Belorussia]. Minsk: Belorusskii Gosudarstvennyi Universitet. 1973. Pp. 221. 2 r. 10 k.

In this study of the Stolypin reforms in Belorussia, L. P. Lipinskii claims to have investigated for the first time both their premises and character. He does not, however, depart too far from the accepted Soviet interpretation that the Stolypin reforms there, as everywhere in Russia, were the autocracy's answer to the Russian Revolution of 1905-07—an attempt to neutralize the influence of the revolution and to destroy the basis for revolutionary upheavals in the future. In this view the character of the reforms was clearly reactionary. This accords well with the author's view that, given the demonstrable development of capitalism in the rural economy of the area, the social stratification in the countryside, and the tendency toward settlement on private farmsteads well before the revolutionary years of the early twentieth century, they were little more than attempts to shore up the landlord regime.

It is little wonder, then, that Lipinskii, drawing largely on secondary materials, further argues that the results of the Stolypin reforms in Belorussia were the mobilization of the landowners, the concentration of further economic power in their hands, and the subsequent proletarianization of the peasantry with attendant class polarization. He attempts to buttress these arguments with a number of tables that, while giving some quantitative atmosphere to his arguments, are not always rigorously analytical and frequently raise as many questions as they are intended to resolve.

Perhaps the most interesting aspect of Lipinskii's work, however, is his effort to critique the "unscien-

tific views of bourgeois historians" who, he claims, have in their respective works on this era "idealized Stolypinism." In fact, the larger question at issue is one of immense concern for those interested in the historiography of this period since its juxtaposes Lipinskii's traditional Marxist-Leninist interpretation with the Western (and especially Anglo-American) theory of "modernization." He singles out for attack the works of Theodore von Laue, George Yaney, Cyril Black, Alexander Gerschenkron, Leopold Haimson, and George Takmakov. Collectively and individually these historians and their lesser-known American and British colleagues are accused of a multitude of shortcomings and outright concealments that range from underestimating the revolutionary potential of the peasantry to ignoring the survivals of serfdom in the countryside and the acuity of the struggle between the antagonistic forces at work in Russian society. Such views he brands as "absurd" and "falsifications of historical truth."

Given such rigid viewpoints and categorizations, the book may be interesting at one level, but as a useful and objective study of the Stolypin reforms in Belorussia, it leaves much to be desired.

GEORGE E. SNOW
Shippensburg State College

NEIL HARDING. *Lenin's Political Thought. Volume 1, Theory and Practice in the Democratic Revolution*. New York: St. Martin's Press. 1978. Pp. 360. \$25.00.

The dustjacket of Neil Harding's study depicts a pensive Lenin, his narrow eyes focused somewhere in the distance, perhaps in the future. The appropriateness of this portrait in red becomes quickly apparent as one moves through the text. Lenin's political thought was never a disembodied philosophy of the state or political morality; it was neither a fixed extrapolation and application of Marxist prescriptions nor an ultimately flexible hodgepodge of ex post facto rationalizations. Rather, argues Harding, it was a remarkably constant and consistent elaboration of political strategies based on a fundamentally orthodox Marxist analysis of the socioeconomic situation in tsarist Russia; it was the evolution of theory and practice appropriate for the ever-anticipated democratic revolution.

Just as Lenin in most of his major writings set up targets for critique and laid out positions to be defended, so Harding begins by attacking the "conventional wisdom which runs through almost all Western commentary, criticism and biography of Lenin" (p. 1). That wisdom holds that Lenin was an inconsistent, unorthodox theorist of Marxism and, as a revolutionary thinker, closer to Russian Jacobins like Tkachev and several Populists than to Marx, Engels, and Plekhanov. This widely held

view has neglected to examine adequately Lenin's economic and social analyses that, in Harding's reading, "provide the clue to coherence or consistency in his more expressly political strategies" (p. 4).

Harding contends that in the period 1894–1914 Lenin held a view of Russia's revolutionary potential based on his study of the level of capitalist development in Russia. Just as Russian economic evolution moved through phases, so did the social development of its constituent classes. For this reason political strategies and organizations also had to evolve. Up to World War I Lenin's political outlook was based on his assessment of "Russia's ripeness for a radical democratic revolution" (p. 6). After 1914 he began to doubt capitalism's progressive potential and concluded that imperialism had created conditions for an international *socialist* revolution. In his close analysis of Lenin's prewar writings, Harding provides several useful correctives to the usual misreading of *Chto delat'*? ("What is to be Done?"). He argues that this work was limited in its applicability to the specific political situation at the time it was written, that Bolshevism, organizationally and strategically, changed later in response to the mass upheavals of 1905–1907, and that this sacred text of the "party of a new type" gives one no insights into explaining the phenomenal success of the mass appeal of the Bolsheviks among workers in 1912–1914. The consistency of Bolshevism stemmed from Lenin's constant adherence to Plekhanov's original notion that in Russia the proletariat would play the leading role in the democratic revolution in place of the pusillanimous local bourgeoisie.

Although Harding stays perhaps too close to Lenin's own words and does not investigate thoroughly enough the attitudes of his opponents and the actual historical context in which Bolsheviks and Mensheviks operated, he has given us one of the most original reconsiderations of Lenin's thought in recent decades. One might question why he does not use Leopold Haimson's earlier analysis or ask, more fundamentally, whether Lenin's views accurately reflected reality. But given the more modest scope of his purpose, Harding has managed to do for Western scholarly literature and Lenin what Soviet historians under Khrushchev did for many of Stalin's victims. He has rehabilitated Lenin as a serious and consistent Marxist thinker, as a political strategist who based his actions not on mere expedience but on a thoroughgoing consideration of the social and economic limitations of his own time.

RONALD GRIGOR SUNY
Oberlin College

ROBERT SERVICE. *The Bolshevik Party in Revolution: A Study in Organisational Change, 1917–1923*. New York: Barnes and Noble. 1979. Pp. 246. \$24.50.

Robert Service has written a concise but factually rich history of the Bolshevik party from the revolution to 1923, the time of the major defeat of Trotsky by the troika of Zinoviev, Kamenev, and Stalin. Its specific virtue is that it does not confine itself to contests of personality and policy but uses them as a backdrop to describe the evolving structure of the party. The account includes the thoughts and polemics of the participants as they sought to comprehend and direct this changing structure. Service presents the change as an "internal metamorphosis" from "democratic anarchism" to monolithic centralism (pp. 2, 4).

The author's standpoint is that of democratic socialism; hence the evolution he describes is one of tragic degeneration. What are its causes? His implicit thesis is that no single factor or category of them constitutes an explanation: neither Lenin's will and his organizational theory, nor so-called objective circumstances, such as economic conditions and the Russian political tradition, nor the imperatives of large-scale organization, nor morale suffice. By means of historical reconstruction, Service seeks to juxtapose the various factors, to reveal the tensions between and among them and the specific contribution of each to the outcome. He brings to bear a critical understanding of social theory, especially the ideas of Weber and Michels. The larger problem he addresses and for which the narrative constitutes evidence is the roots of Stalinism. In what sense and to what degree is Stalinism a consequence of Leninism?

The conception of the narrative is superior to its execution. Some of the weakness probably stems from the publisher's effort to reduce costs—hence the absence of a discussion of sources and a historiographical discussion that is too elliptical and sometimes fails to provide even the titles of works discussed. Other shortcomings are the author's own. In the early chapters he relies too greatly on secondary sources. His grasp of the secondary literature on the underground period is incomplete. A conventional St. Petersburg-centered account results, which does not introduce the reader to the subject. His portrayal of 1917 is equally narrow in focus and conventional. The pace quickens and the author assumes firm command as he takes us into the party crisis of 1918.

The descriptions are sometimes overdrawn and the contrasts too stark. The beginning point is Bolshevism as "democratic anarchism"; the end point is Bolshevism in 1923 in a state of near-perfect centralization. The author has exaggerated certain tendencies in both cases. In the first place, even if Bolshevism was swayed by tides of anarchy in 1917, it did not succumb entirely. If it had, how could it have mobilized the mass institutions created by revolution—factory committees, soviets, and so forth—a process the author correctly calls to our attention?

In the second place, the dominant image of the party in 1923 is one of unremitting centralization; "regimented grotesquerie" (p. 210) is the author's phrase. Hindsight, however, informs us that the party had much further to travel down this road. In effect, Service has provided two models of organization in place of descriptions of concrete realities.

Perhaps these shortcomings are overstated. Service's work is successful both as a narrative and analysis: it will clarify understanding of the early development of Bolshevism, of political and organizational behavior in general, and of the nature of historical causation.

GEORGE ENTEEN

Pennsylvania State University

E. G. GIMPEL'SON. *Velikii Oktiabr' i stanovlenie sovetskoi sistemy upravleniia narodnym khoziaistvom (noiabr' 1917-1920 gg.)* [Great October and the Formation of the Soviet System of Managing the National Economy (November 1917-20)]. Moscow: Nauka. 1977. Pp. 310. 2 r. 30 k.

This book assesses the establishment and development of the Soviet system of economic management from the revolution in November 1917 until the end of the period of War Communism (early 1921). Unlike many Soviet authors writing on this period, E. G. Gimpel'son presents an overall view of the development of the system of economic management and stresses how organizations changed over time and how they interacted with each other. His book is thus crammed with compact detail, most of it taken from works published in the 1920s and from later studies. Still, the author's outlook prevents the work from being merely derivative, although at times it reads like a text on administrative law.

Overall, the work is a defense of Soviet policies, with the expected fulsome praise of Lenin (described, for instance, as "a brilliant scholar and great organizer"). The author manages a certain frankness in discussing some of the problems encountered in implementing economic management, without actually admitting that, for the most part, utter administrative confusion in fact existed at the time. He fully supports the now standard Soviet view that the Civil War, foreign military intervention, economic blockade, and other factors forced the regime to adopt the policy of War Communism and what he calls "several unusual organizational forms and methods of leadership" as temporary expedients, thereby implicitly denying any role of ideology in the undertaking of these measures.

Gimpel'son is concerned more with the organization of economic management than with economic consequences. He thus focuses attention not so much on the nationalization of almost all industry

and the seizure of peasant "surpluses" as on the difficulties in coordinating the work of the *sovmarkhozy* (regional economic councils, organized on the territorial-production principle) with that of the eventually predominant *glavki* (central directorates, organized on the branch-production principle). He treats sympathetically the inability of VSNKh (Supreme Council of the National Economy) to cope with all the tasks of coordinating and directing the economy during this period and emphasizes the "deciding role" of the Council of Workers' and Peasants' Defense (later called the Council of Labor and Defense) under Lenin in accomplishing this.

Chapters are devoted to the organization of management in industry, transport, and construction, the leadership of agriculture, organizational forms and methods of mobilizing labor and material resources, first steps in planning, and the leading role of the working class in economic management. In maintaining the conventional Soviet view on the role of the working class, the syndicalist tendencies of the unions at the time are virtually overlooked, and opposition to Lenin on the issues of one-man management and the subordinate role of the unions is minimized. However, of interest are Gimpel'son's data indicating that workers were more numerous than bourgeois specialists in industrial management at the end of 1920, if not before, and his view that workers played a greater role in economic management than has been recognized.

The value of the book is that, within the limits of the accepted Soviet viewpoint, it provides a comprehensive overview of attempts to implement economic management during this chaotic period. Moreover, the material on interrelations between organizations is often interesting and informative. It is thus a useful foundation study of the period, although it leaves a number of important questions (from the Western viewpoint) unasked and unanswered.

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TEDDY J. ULDRICKS. *Diplomacy and Ideology: The Origins of Soviet Foreign Relations, 1917-1930*. (Sage Studies in Twentieth Century History, number 9.) Beverly Hills, Calif.: Sage Publications. 1979. Pp. 239. Cloth \$22.50, paper \$9.95.

Teddy J. Uldricks has set himself the task of providing a chronicle and analysis of the origins and early development of the Soviet diplomatic corps. In this he has succeeded admirably, for he has produced a clearly written and well-developed study of the structural and organizational development of the People's Commissariat of Foreign Affairs (Narkomindel) during the tenure of its first two commissars, Lev Trotskii and Georgii Chicherin.

Uldricks begins with a description of the chaotic conditions that surrounded the birth and early history of the Narkomindel, when former Provisional Government diplomats refused to turn over Russian embassies and consulates abroad and when the new Soviet state maintained no diplomatic relations with foreign governments. He then discusses the revival of diplomatic activity during the period of the New Economic Program and the significant expansion and reorganization of the commissariat during these years along lines parallel to those followed by other countries.

One of the more interesting portions of *Diplomacy and Ideology* treats the personnel of the diplomatic corps recruited under Trotskii and Chicherin. Given the refusal of the vast majority of pre-Soviet Russian diplomats to serve the new state, the Narkomindel became the most completely "Bolshevized" department of government. The personnel of the commissariat, however, was much more highly educated and much more heterogeneous than the members of either the Communist Party or the government as a whole (87 percent of personnel who served from 1917 to 1930 had at least some university training and only 42 percent were Great Russians).

In his treatment of the relationship of the commissariat to political developments in Soviet Russia, Uldricks argues that the Narkomindel enjoyed a relatively great degree of freedom in conducting foreign policy during the power struggles of the mid-1920s, so long as it operated within the general guidelines established by the Politburo. It was more than a mere executor of policy established by the party and was able to influence the overall development of Soviet foreign policy.

Uldricks disagrees with a number of what have become "accepted" interpretations of the early development of Soviet diplomacy. For example, the Narkomindel never became an active center of oppositional strength, nor did it become a primary "dumping ground" for defeated political figures, even though a small number of diplomatic appointments did apparently result from political disgrace.

The author concludes with a discussion of the destruction of the diplomatic corps during the Great Terror of the 1930s. He estimates that, of the top levels of commissariat officials, more than 62 percent—and possibly as high as 70 percent—died during the Terror. Since 14 percent had either died or defected before 1935, the impact of the Terror on the commissariat was even greater than a figure of 70 percent would indicate.

Diplomacy and Ideology makes an important contribution both to the early history of Soviet foreign relations and to our better understanding of the evolution of the governmental apparatus of the Soviet state. The author has used effectively a vast array of

both primary, including archival, and secondary sources to document and explain the evolution of an important instrument in Soviet policy.

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EDGARS DUNSDORFS. *Pirmās latviešu bībeles vēsture* [The History of the First Latvian Bible]. Summary in German. Lincoln, Nebr.: The Latvian Evangelical Lutheran Church in America. 1979. Pp. 233. \$28.00.

Edgars Dunsdorfs is one of the most distinguished Latvian economists and historians and certainly the most prolific one. He started his career at the University of Latvia and continued his activities at the Baltic University in Germany and the University of Melbourne in Australia. His books and articles in scholarly journals are too numerous to mention here. Although his basic field is economic history, he has done most of his research in general history, starting out in the seventeenth century but later extending his interests in both directions to earlier and later centuries, dealing even with post-World War II problems.

In this volume—*Pirmās latviešu bībeles vēsture*—Dunsdorfs has returned again to the seventeenth century. We might wonder what kind of history one can write about translation of the Bible into one or another language. Dunsdorfs has proved that one can not only fill 233 pages with a text but also make it interesting and valuable reading. In addition to this, Dunsdorfs's present volume can also serve as an exceptionally good example of fine historical scholarship of the highest caliber. Basically the book deals with the first translation of the Bible into the Latvian language, officially published in 1689 in Riga under the auspices of, and dedicated to, Charles XI, the king of Sweden. In preparation for this study the author has utilized completely unknown archival sources found in Stockholm and Riga, and he has critically evaluated all existing literature on the subject. He has also dealt in considerable detail with the principal persons involved in the sponsorship and publication of the Bible, revealing their personalities and various activities. Particular attention quite naturally has been paid to the translator himself, Probst Ernst Johann Glück (1654–1706), whose stepdaughter Martha, by the way, later became the mistress and finally the wife of Peter the Great of Russia and subsequently the Empress of Russia under the name of Catherine I. Besides presenting to the reader entirely new information on the planning of the translation and the publication of the Bible in two Estonian dialects as well, the book also contains a wealth of informa-

tion on the political, social, cultural, and economic conditions in the Swedish Baltic provinces of Estonia and Northern Latvia in the seventeenth century. Footnoting is extremely meticulous and detailed, compensating for the lack of a separate bibliography. Sixty-eight pages of the text are devoted to reprinting of original documents or presenting their facsimiles. The book also contains detailed indexes of persons, place names, and subject matters, as well as a good summary in German that, along with the texts of documents in German and Swedish, might be useful to Western scholars.

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ber of geographic, religious, political, and other factual errors and inconsistencies. The references to Muslims as "Mohammedans" (p. 2 et seq.) alone would entitle the author to 100 lashes in the *Bedestan* (prisoner-market) of Algiers. The Kabylia region is consistently referred to as a tribe and its sedentary cultivators as nomads. The author also uncovers three "sects" of Islam previously unknown to Islamic historicity (p. 95). What is really regrettable is that after five centuries of interaction the same old myths and misconceptions about the Islamic world are still purveyed from the same tired platform.

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NEAR EAST

JOHN B. WOLF. *The Barbary Coast: Algiers Under the Turks, 1500 to 1830*. New York: W. W. Norton. 1979. Pp. xii, 364. \$16.95.

The emergence in the 1980s of a revived militant Islam as a powerful force in world affairs, buttressed by vast petroleum reserves and a key strategic location between established power blocs, underscores the need for understanding of the historical antecedents of Islamic-Christian interaction not only for policymakers and academicians but also for the general public as well. John B. Wolf's study of the corsair state of Algiers meets this criterion in general and as such is a welcome addition to the historical literature available in English on North Africa. Although he has overlooked at least one "general book in English" (by this reviewer), his book provides a thorough description based on European archival and national libraries and the extensive accounts of Europeans who dealt with the corsair state during its three centuries of vigorous existence. The book's value is enhanced by a fine selection of photographs, although a location map of Algiers in North Africa would have helped.

Wolf begins his study with a brief analysis of the fifteenth-century Maghrib and Mediterranean political conditions that permitted successive Spanish and Ottoman conquests of Algiers. The balance of the book examines the formation of the "regency," its government, social structure, and corsair and prisoner communities, and Algerian-European relations through 1830 and the final French conquest.

Unfortunately, the accuracy of Wolf's "portrait" of Ottoman Algiers is badly flawed, the result of total reliance on European sources and the absence of in-depth analysis of internal Maghribi relationships. The Ottoman documentation clearly indicates that the Algiers case was anything but a "300-year government by a foreign army of occupation" (p. xi). The author's unfamiliarity with Islamic North African society may explain in part the appalling num-

WERNER ZÜRRER. *Persien zwischen England und Russland, 1918-1925: Grossmachteinflüsse und nationaler Wiederaufstieg am Beispiel des Iran*. Bern: Peter Lang. 1978. Pp. 501. 98 FR.

The recent Iranian revolution, the demise of the Pahlavi monarchy, and the continuation of the "Great Game" in Central Asia between the Soviet Union and the United States (in place of Britain) have some of their roots in the rivalry between Britain and Bolshevik Russia over Persia during the immediate post-World War I era. The subject has been dealt with by the former Polish diplomat George Lenczowski in his survey of *Russia and the West in Iran, 1918-1948* (1949) and (inadequately) by N. S. Fatemi in his semi-official study of Anglo-Russian power politics in Iran between 1917 and 1923, but these studies were not solidly based on British, German, French, and American archival materials and some important Soviet publications. Werner Zürrer, who has published excellent studies of great power rivalry in the Caucasus and the Transcaspian region during the 1918-21 period, has attempted a detailed account of Anglo-Russian rivalry and the rise of the Pahlavi dynasty in Persia from 1918 to 1925.

During World War I, Russian and British forces repeatedly invaded and occupied parts of Persia to terminate German influence in the country. As the Russians withdrew after the Bolshevik coup in 1917, Persia lapsed into virtual anarchy until a British force occupied the northeastern region in early 1918 to block an alleged Bolshevik threat to India via Persia. Following the end of the war, the Persian government dispatched a delegation to the Paris Peace Conference that asserted claims to the Transcaspia, parts of Russian Central Asia, and a frontier on the Euphrates River. In early August 1919 the British concluded an agreement with Tehran that reaffirmed the independence of Persia and provided technical and financial assistance. Although the ac-

cord was rejected by the Majlis, Persian mercenaries assisted British and White Army forces in their war against the Bolshevik regime.

As the Red Army triumphed in the Transcaspiia and British forces withdrew from northern Persia in January 1921, an officer of the Persian Cossack brigade, Reza Khan, seized Tehran and concluded peace with the Bolshevik regime. To counter British influence in Persia, the Soviet government granted a generous settlement that, with the assistance of an American financial mission and the support of the mullahs, enabled Reza Khan to assert his authority over the country. He was especially successful (with Russian support) in quelling British-inspired separatism in the oil-rich southwestern part of Persia. By the end of 1925, Reza Khan had established himself as dictator, deposed the last shah of the Kajar dynasty, and proclaimed himself the Shahanshah Reza Pahlavi. It was the beginning of a dynasty that endured until the Khomeini revolution.

Zürer's study is not only thorough and well balanced but also highly interesting. It supplements F. Kazemzadeh's *Russia and Britain in Persia, 1864-1914* (1968) and will remain the definitive work on the subject until data from the Soviet archives provide an additional dimension and some new interpretation for the story.

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AFRICA

J. D. FAGE, editor. *The Cambridge History of Africa*. Volume 2, *From c. 500 BC to AD 1050*. New York: Cambridge University Press. 1978. Pp. xvii, 840. \$69.50.

This is the fourth volume, out of a projected eight, to appear of the *Cambridge History of Africa*. When the set is completed, African history will have come of age. Not taught on a regular basis in universities, even in Africa, until the 1960s, African history is now blessed with the sanction of academic approval that is provided by the "Cambridge History" imprimatur. This set is very much in the vein of previous Cambridge histories, a sound but very orthodox and somewhat unimaginative work of reference. Volume 2, edited by J. D. Fage, covers the period from around 500 B.C. until A.D. 1050. Its eleven lengthy chapters synthesize what is known of the history of the continent on a regional and chronological basis. Some of the most valuable aspects of the volume are the comprehensive bibliographic essays and bibliography, which, together with the superb index of 70 pages by Marion Johnson, make up nearly a fifth of the total length.

Much of the source material for the present volume is nondocumentary. In a rather curious way

the editor, J. D. Fage, classes as a "beginning to history" the use of iron to make weapons and tools (p. 2). Although certainly a useful periodization marker, a technological change of this nature does not mean history. African historians have a tendency to begin their African history courses at the beginning of the Iron Age, but this is still a period when most of Africa's past is chiefly accessible by the tools of the archeologist, the word lists of the linguists, the fossil plants of the paleobotanists, and the interpretations of the ethnographers.

Fage further calls this period "definable methodologically as a work of what has come to be known as proto-history." When this term was originally used by European prehistorians at the turn of the century, it was reserved for such periods as the Belgic occupation of parts of southern Britain for which there were accounts by historians like Julius Caesar or data derived from numismatics but no internal written sources. Except for North Africa, parts of Ethiopia, and very small areas of West Africa and the East African coast, there are not even remotely indirect documentary sources for at least two-thirds of the continent. At an international conference concerned with terminology at Burg Wartenstein in 1965, prehistorians working in Africa decided to drop the term, and it is a pity that the historians have not followed suit.

In the manner of most Cambridge histories, the book consists of a series of largely independent essays starting off with the "Legacy of Prehistory" by J. Desmond Clark, which, although thorough, occasionally lapses into too detailed trait description. This essay is followed by chapters on North Africa by R. C. C. Law, the Nilotic Sudan and Ethiopia and Christian Nubia by P. L. Shinnie, trans-Saharan contacts and the Iron Age in West Africa by Raymond Mauny, the emergence of Bantu Africa by Roland Oliver and Brian Fagan, the Christian period in the Mediterranean by W. H. C. Frend, the Arab conquest and the rise of Islam in North Africa and the Fatimid revolution by Michael Brett, and finally the Sahara and the Sudan from the Arab conquest of the Maghrib to the rise of the Almoravids by Nehemia Levtzion. There are few attempts to outline the major themes on a broad basis except in the chapter on Bantu Africa, no comparative study of the demographic aspects of the continent as a whole, and a paucity of discussion of recent theoretical ideas concerning the emergence of complex societies, the development of exchange systems, or the underdevelopment theory.

The authors do not draw together the artistic achievements of sub-Saharan Africa; one looks in vain for photographs or even adequate coverage of the terracotta art of Nok in Nigeria or of the Lydenburg heads of the Transvaal, which are surely as significant in terms of intellectual or religious his-

tory as pots and stone tools are to economic and technological history. One also looks in vain for a challenge to, or an analysis of, the provocative views of Cheikh Anta Diop on the relationships between Black Africa and the Nile Valley civilizations. One hopes that some discussion of his theories will be included in volume 1, as they have influenced many African scholars, particularly in francophone Africa, as well as black historians in America.

The most interesting chapters are those dealing with North Africa. There has too often been a lamentable lack of integration in African history with Africa equated with Black Africa south of the Sahara and anglophone writers demonstrating a far too hazy appreciation of the sources in French and Arabic. These chapters stress the importance of North Africa in the history of the Mediterranean. Here Alexandria emerged as the most important commercial center while the Phoenicians reached their greatest power in Carthage.

Two matters concerning iron working are perhaps worth correcting. On page 359 it is stated that "early iron technology is not thought to have been able to produce tempered metal." Research in South, East, and West Africa indicates that the African smiths produced iron of such quality that there is talk of using the term "steel" rather than "iron" for the age in question. On page 331, in castigating Oliver Davies's 1000 B.C. date, Mauny has failed to take notice of Davies's 1973 full report on Ntereso in which he retracted that early date.

The inclusion of numerous figures and plates adds to the usefulness of the volume even though the pictures of the Zimbabwe ruins would have been more appropriate in volume 3, which covers the period of the main building of the Great Enclosure and the Acropolis walls. There is a lack of coverage of the East African coast and Madagascar for the period under review, and it should perhaps have been pointed out by the editor that they are included in volume 3. Though conventional, this is nevertheless a work of elegant scholarship and an invaluable reference tool.

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ADRIAN HASTINGS. *A History of African Christianity, 1950-1975*. (African Studies Series, number 26.) New York: Cambridge University Press. Cloth \$34.50, paper \$9.95.

African church history continues to attract scholarly attention precisely because organized Christianity remains an effective force on the continent. In recent years attention has been given to the state

of Christianity in Africa since the attainment of independence; the book under review is of this genre.

The study is divided into four major chronological periods—1950, 1951-58, 1959-66, 1967-75. Within this framework, Adrian Hastings discusses three major themes: (1) the relationship between the church and the state, (2) the operations of the mission churches, and (3) the African independent churches. Hastings accurately observes that the colonial era was clearly the age par excellence of the expatriate missionary. Not only did European missionary presence predominate in practically all the mission churches, but also these same missionaries tended to run the churches as colonial estates. The close identity of interest between the churches and the colonial state was reflected in the attitude of the former toward African nationalists. Indeed, "the churches tended so strongly to disapprove of the independent expression of political views by their members" (p. 31), thus creating the impression that they were antagonistic toward African political emancipation.

The Africanization and indigenization of the mission churches receive significant treatment in this book. Many churches, particularly the Roman Catholic Church, deliberately resisted the training of a significant number of local priests and so gave the Africans cause to believe that they had come to Africa not merely to serve but to dominate. By 1950 there was only one black Catholic bishop in all of Black Africa—Bishop Joseph Kiwanuka of Uganda. Others were elevated much later, especially after independence. Take the case of Nigeria. At independence in 1960 there were nineteen bishops in the country. Of these only four were Nigerians. Today, there are thirty bishops of whom twenty-six are Nigerians. In most of the other African countries, rapid Africanization is in progress. Africanization has invariably removed expatriate domination and overwhelming racial arrogance (pp. 46, 117).

Since independence, the mission churches have faced challenges on several fronts. First, there is the increasing demand for speedier liturgical adaptation to make African Christianity a distinctly African institution, adapted to the African milieu. The independent churches may reflect more of the African situation than the mission churches, hence their increasing popularity. Another area of concern to the church is the increasing secularization of the nation-states. Demand for more authentic indigenization by some nationalist leaders has taken the appearance of frontal war against Christianity. President Mobutu's challenge to the Catholic Church in Zaire in the early 1970s is a case in point. "We are now embarking on our cultural liberation," Mobutu declared, "the reconquest of our African, Zairian soul" (p. 191). Certainly Mobutu's

search for "authenticity" posed a problem to the Christian church in Zaïre just as Nkrumaism did in Ghana in the 1950s and 1960s.

All in all, this is a very informative book, a significant contribution to Africanist literature. Specialists, especially those concerned with African church history, will find it informative and valuable. Though based largely on published works, it is both comprehensive and balanced. The author's assessment of the role of the church, especially in the southern part of Africa, where liberation movements predominate, is instructive. Granted that the churches have in one form or another criticized the injustices and racialisms practiced in this region, "the overall achievement remained limited, both on account of the ambiguity of the church's own internal practice, and because of a basic inability of the bishops to face up to the inherent moral rights of the liberation movement challenging a ruthless and oppressive minority government which has refused for years all peaceful change." Africans, Hastings concludes, are "looking for deeds not words, for a major change of law not a well worded protest" (pp. 216-17).

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L. H. GANN and PETER DUIGNAN. *The Rulers of Belgian Africa, 1884-1914*. Princeton: Princeton University Press, for the Hoover Institution. 1979. Pp. xv, 265. \$17.50.

Lewis H. Gann and Peter Duignan are best known as the editors of a massive, five-volume study of *Colonialism in Africa, 1870-1960* (1969-73) and of *African Proconsuls: European Governors in Africa* (1978). They also co-authored a militant apologia for Western colonialism in sub-Saharan Africa (*Burden of Empire* [1967]) and more recently have devoted themselves to a series of parallel studies of pre-World War I colonial rule as part of the Hoover Institution's "Builders of Empire" project. The present volume is a companion to *The Rulers of German Africa, 1884-1914* and to *The Rulers of British Africa, 1870-1914*, but it is not likely to add much to the two authors' fame.

Those who are acquainted with Gann and Duignan's earlier writings will hardly be surprised to find them once again engaged in their familiar activity of justifying Western colonialism. The present volume conforms to what the authors complacently call their "unfashionable" approach, which consists of arguing (*pace* Marx, Engels, Livingstone, and Kipling) that European colonization "played a progressive as well as a negative role in the backward regions of the Victorian world" (p. xi). Like other allegedly "revisionist" views of the neoconservative

school, this line of argument will only comfort those who passionately need to believe that, whether it be in Indochina, Chile, Iran, or Africa, the West is necessarily on the side of the angels.

One may legitimately wonder what new perspectives can be added to Gann and Duignan's case by the study of Belgian rule from 1884 to 1914. The book may serve some useful purpose by pointing out that Belgian colonial rule was not substantially different from that of other imperialists, or by documenting the fact that Belgian colonialists were neither worse nor better than their British or German counterparts. The choice of the 1884-1914 time frame, which makes obvious sense in the case of the German, and even of the British, territories, is singularly inappropriate for the Congo, however. On the outbreak of World War I, less than six years after the 1908 *reprise*, direct Belgian rule had barely begun, and it was not until the following decade that the Belgian colonial system developed the distinctive features that it was to retain until the 1950s. Whether in terms of socioeconomic change, of native administration, or of the specific coalition of interest groups that presided over Belgian colonial policy, the year 1914 represents neither the beginning nor the end of an era.

As a study of either the Leopoldian or of the Belgian systems, therefore, the book falls short of earlier works that concentrate more exclusively on one or the other, such as Ruth Slade's *King Leopold's Congo* (1962) or Roger Anstey's *King Leopold's Legacy* (1966). Gann and Duignan offer valuable insights into the social backgrounds and motivations of the soldiers and officials of the early colonial period. The treatment of this material is far from systematic, however, and consists for the most part of impressionistic vignettes that will add little to the information of those already familiar with Belgian sources. Descriptions of the Free State administration and of the post-1908 structure of Belgian rule (later to be substantially altered) will also be of some use for nonspecialists.

Aside from these limited merits, however, the book suffers from such an accumulation of factual errors as to jeopardize its value as a reference work. The list of such errors, which ranges from inaccurate geographic data to misinterpretations of existing information on Belgian and African societies, would be too long to detail. When added to the occasional contradictions, to the discrepancies between statistical data, and to startling naivetés (the downfall of the Congo "Arabs" is attributed, *inter alia*, to their "lack of seapower"), not to mention misquoted sources and typos, they achieve a level of carelessness that one had not been led to expect from books published under the Princeton imprint.

Paradoxically, and for all their proliferation, such

errors do not significantly detract from the cogency of the authors' thesis, which rests only marginally on facts and derives its momentum from ideological postulates that can only be accepted or rejected *in toto*. Theirs is a vision of colonialism that owes its appeal to emotional rather than rational arguments. Except for a brief conclusion, this vision is not, in point of fact, developed at any great length, since most of it has been presented in Gann and Duignan's earlier writings, and the authors are, for the most part, content to justify colonialism in general—and Belgian colonialism in particular—through such oblique references as their approving quotation of "journalist-historian" George Martelli's smug (and inaccurate) remark that "every building in the Congo more than six feet high was put there by the Belgians" (p. 219); their casual dismissal of the Mahdist uprising in the Sudan as "an exploitative theocracy misnamed a revolutionary state by its latter-day admirers" (p. 57); or their matter-of-fact characterization of the Africans who resisted Leopold's brutal conquest as "an enemy who would use the most refined methods of torture imaginable" (p. 53). Adding to the emotional charge are the occasional parallels (quite out of context, as a rule) between colonial violence and twentieth-century counterinsurgency campaigns, in which Belgian colonial soldiers are successively described as "counterguerrillas by profession" (p. 58) and then equated by virtue of their backgrounds with "the most famous partisan leaders of our time" (p. 65).

The disappointment one experiences when reading Gann and Duignan's latest work is mitigated by the fact that competent and scholarly studies of Belgian colonial rule already exist. As for their reappraisal of Belgian colonialism, it will only persuade those who are already convinced of the merits of "benevolent imperialism."

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ADAMU MOHAMMED FIKA. *The Kano Civil War and British Over-rule, 1882-1940*. New York: Oxford University Press. 1978. Pp. xvii, 307. \$33.00.

This book will be of value to those scholars interested in African history, Islam, imperialism, and the impact of colonial rule. Kano is one of the two or three largest cities in Nigeria, and, in the nineteenth century, it was the capital of the most prosperous emirate in the Sokoto Caliphate, which itself was the largest state in nineteenth-century Africa. Until now, there has been no general history of Kano, and this lack has affected the reconstruction of the whole history of this empire. Because of the size of its population and its economic potential, the emi-

rate was one of the more important prizes in the scramble for Africa. The British conquest in 1903 introduced colonialism here much later than for most of Africa. Nonetheless, the policies introduced at Kano had far-reaching consequences as a prime example of Frederick Lugard's theories of "indirect rule." Adamu Mohammed Fika's exploration of the events leading up to the conquest, especially the civil war that shook Kano in 1893-94, and his examination of the impact of British rule make this book a significant contribution to the field. Fika's presentation of a point of view that hardly accepts the desirability or inevitability of colonialism adds balance to this area of history, which even now is heavily weighted in favor of apologists for colonialism.

The civil war raises interesting questions about the history of the Sokoto Caliphate. The failure of the Sokoto government to impose its candidate for succession to the Kano throne demonstrates the limits of centralized authority. The fact that the successful candidate secured support from nominal enemies who were not part of the caliphate highlights the diffused nature of power. By contrast, the British regime, as unorthodox in its imposition as the independent governments of 1893 and 1894, was much more powerful. Fika's examination of this transition is perceptive. He shows how British rule increased the authority of the central government at the expense of titled officials, both free and slave. Colonial aims were very narrow—rationalization of taxation, elimination of slave officials, prevention of social unrest, and abolition of slave trading. The only economic measure of importance was the construction of a narrow-gauge railway to Kano, completed in 1911. The issue of slavery was treated lightly.

Fika's methodology is one that is now standard in the study of African history—a combination of archival sources, Arabic literature, published European accounts, and interviews that the author conducted in Kano. Indeed the documentation is impeccable. One might wish that the interviews had been tape recorded and deposited at archives for the use of scholars, but there is no question about the standards employed in gathering the material. There is much new information here, and consequently much of the history of Kano is clearer by far than has hitherto been the case.

The only weakness in the book is the treatment of the economy. The *laissez-faire* policies of the British limit the value of colonial documents, while a reliance on interviews with aristocrats invariably prevents a full understanding of merchant, peasant, and slave activities. These are topics for others, however. Fika's contribution explores official behavior—both British and Fulani. The analysis of taxation, aristocratic alliances, the role of slave offi-

cials, the response to British aggression, and similar topics is a major contribution to the study of the Sokoto Caliphate and British Nigeria.

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JOHN D. HARGREAVES. *The End of Colonial Rule in West Africa: Essays in Contemporary History*. New York: Barnes and Noble. 1979. Pp. xvi, 141. \$23.75.

John D. Hargreaves of Aberdeen University is well known for his important volumes on the late nineteenth-century European partition of West Africa and his brief interpretive history of the states of former French West Africa. In the four essays that form the core of this volume he examines several aspects of political decolonization in the Gold Coast, Nigeria, and Sierra Leone during the 1940s. References to the French territories are limited to Senegal, Ivory Coast, and Guinea, plus the trusteeship territory of Cameroun, which is usually considered part of Equatorial Africa or Central Africa. They serve primarily as comparisons with the experiences in the British territories. At the same time, references to the 1950s are chiefly retrospective. They look back to the pre-1939 origins of the transfer of power rather than ahead to the final stages that culminate in independence in the late 1950s and early 1960s.

One of the essays, which treats the historical backgrounds, appeared as a booklet in 1976. Two others are scheduled to appear in 1980 in their original form in volumes edited by Prosser Gifford and William Roger Louis on transfer of power in Africa (1945-65) and by W. H. Morris-Jones and Georges Fischer on the British and French experiences with decolonization and its aftermath; one of them deals with the formation of British policy during and right after the Second World War and the other with decolonization in Sierra Leone through 1951. Only the fourth essay, which discusses the rise and course of African nationalist movements, particularly their collaborative role in the decolonizing process, was specifically composed for this collection. In all but the first essay the author utilizes recently opened archives from the British Colonial Office. He enriches them with personal insights from his involvement with Africa during three decades, including a period as a professor at Fourah Bay College, Sierra Leone, in the early 1950s. Taken as a whole, his essays show that British policy formulated in wartime mainly in the light of interwar thinking had to shift priorities from long-term economic and social development to an accelerated transfer of power in the wake of the decolonizing forces generated by the conflict. They show how the British largely retained political initiatives

by collaboration with the new urban-based African movements that had succeeded in establishing ties with the countryside. They place the developments of the 1940s in a context that reaches back to the onset of the colonial period.

The three shorter essays that make up the last quarter of the text deal with the author's experiences in Sierra Leone, British colonial universities, and the relationship between African and contemporary history. They are delightfully provocative because of their perspicacity and wit.

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TESHOMÉ G. WAGAW. *Education in Ethiopia: Prospect and Retrospect*. Ann Arbor: University of Michigan Press. 1979. Pp. xv, 256. \$16.50.

Compared to what this book might have been—stimulating, provocative, insightful—it is disappointing. Supposedly “an expository analysis of the development of education in Ethiopia from the early part of the Christian era to the present” (p. ix), this work treats Ethiopia's first eighteen centuries in twelve pages, arriving at the “Foundations of Modern Education” in chapter 2. Teshomé G. Wagaw practically ignores Ethiopia's fascinating system of traditional church education, while deeming mere mention sufficient for less formal Moslem, Jewish, and animistic structures. Wagaw discusses neither socialization nor the values of Christian Ethiopian society, material that would undoubtedly clarify early church and parental resistance to modern education and the recurring failure to attract sufficient females into the system.

Wagaw is not uncritical of his country's educational progress, but his conclusions are hardly astounding. Despite considerable imperial attention, pre-1935 educational development was, at most, modest; neither did progress following the Italian war leave much room for back-patting. Educational programs were generally improvised, mismanaged, and wasteful. Thus, after a generation of effort, Ethiopia, a nation proud of its heritage as a leader of African independence, was shocked to discover in 1961 that it was among the continent's most educationally backward countries. Redoubled efforts largely failed, so that by 1970 nongovernmental schools were doing relatively more to meet the nation's needs. Wagaw seems content with gentle admonition: “...even within existing limitations, more progress and fundamental change could have been achieved” (p. 196).

Although the author charts statistical progress (student-teacher ratios, the number of classroom units, the levels of expenditure), he never really penetrates the heart of Ethiopia's educational diffi-

culties. Wagaw clearly prefers to side-step controversial political developments. Eritrea provides one interesting example, as an area that progressed substantially under the 1950s federation, mostly at its own expense, only to see success falter in the 1960s as part of Ethiopia's underdeveloped periphery.

Wagaw hardly mentions the growing political turmoil in the country from the abortive 1960 coup onward. One wonders how these disturbances affected academic quality and whether the emperor's commitment to education was consequently re-evaluated. The controversial Sector Review of the early 1970s can hardly be separated from the developing revolution. Like its predecessors, this plan was mismanaged and largely idealistic. Was the government's decision to implement it honest or calculated? Certainly funding for such an ambitious undertaking was indefinite, and resistance from an aspiring middle class, already frustrated by lack of advancement and opportunity, was, surely, to be expected. Wagaw has little to say on these questions.

One last point. The book contains a bibliography of over eight hundred individual entries but only half as many footnotes. One wonders how the remaining materials were employed, why this section was not "abridged" in line with the text. Apparently, the University of Michigan Press is incredibly short of publishable material.

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CHARLES PERRINGS. *Black Mineworkers in Central Africa: Industrial Strategies and the Evolution of an African Proletariat in the Copperbelt, 1911-41*. New York: Africana Publishing. 1979. Pp. xvii, 302. \$35.00.

Can one "concretize a theory of proletarianism"? Following the path of Central African historical revision, Charles Perrings tries. He shows how the labor needs of the copper mines of Katanga and Northern Rhodesia, set in an overall economic framework based on complex, differing attitudes toward the structural attributes of indigenous employment, led to the displacement of migrancy by stabilization as the predominant mode of worker utilization. He goes on to conclude, with the kind of logical leap that characterizes so much of this genre, that stabilization established the preconditions for proletarianization.

Perrings also sees the beginnings of proletarianization in the security that came to African miners in the 1920s (primarily in Katanga) when they were on the favorable side of the demand-supply equation. Later, in Northern Rhodesia, Perrings be-

lieves that by the early 1940s Africans had achieved proletarianization: their attitudes toward employers had changed; their attitudes toward their conditions of employment had altered; and, subjectively, they were proletarian. What is never clear is the extent to which being proletarian—if it ever existed—made any difference to the lives of the Africans involved.

One fears that Perrings, like some of his immediate predecessors, has sought evidence for models and constructs that are at best, given the data available, difficult to apply to Central Africa. Moreover, it is conceivable that theoretical notions derived from a very different European context cannot blithely be used to analyze Africa. Perrings and several of his predecessors remind us, albeit unwittingly, how important it is first to understand Africans on their own terms and in the context of what, in this case, would mean their own response to the opportunity to act in a proletarian fashion.

Perrings has understood these conceptual caveats far more fully than his predecessors. Packaged between each chapter's opening and closing attempts to be theoretical is a running comparative history of labor and capital in adjoining areas of copper exploitation during the formative years (1910-41) of the industry. The author shows with painstaking clarity how both the Katangan and Northern Rhodesian industries were established; how their labor recruitment policies altered in accord with greater supplies of labor; how they received and controlled the resultant labor forces; and in what degrees and why the Central African mines (unlike the gold mines of South Africa) ultimately depended upon a stabilized, married, trained cadre of black workers rather than short-term migrants.

The strengths of this book reside in the author's grasp of the ways in which the geological structures of the mining properties on both sides of the border imposed different economic imperatives on the employers; in his emphasis on the differential impact on labor supply of discouraging (Northern Rhodesia) and not discouraging (Katanga) effective African agricultural entrepreneurial activity; and in his delineation of the ways in which various Northern Rhodesian ethnic groups responded to industrial opportunity.

Perrings's analysis of the etiology of the 1935, 1940, and 1941 strikes is unconvincing. In these cases, unlike much of the rest of the book, his conclusions outrun the evidence. He ends his analysis arbitrarily in 1941 (when the proof of his thesis is surely to be found in the 1940s and 1950s). He pays little attention to life in the compounds and is overly dependent upon a narrow range of published and unpublished data.

ROBERT I. ROTBERG
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THOMAS H. HENRIKSEN. *Mozambique: A History*. London: Rex Collings; distributed by Rowman and Littlefield, Totowa, N.J. 1978. Pp. 276. \$22.50.

Thomas H. Henriksen has the distinction of having published the first history of Mozambique ever to be written. It must certainly have been a daunting task to try in 232 pages to do what no one has attempted before, and the result is inevitably something of a "curate's egg." The most successful part of the book, and the part that contains the author's own research, is the last four chapters describing the origins of FRELIMO and the war of liberation. A lot has been written about FRELIMO, most of it political myth making, and Henriksen has tried to present an account of the movement, its successes and failures, that avoids the excesses of political commitment of other writers. It is a balanced and very sensible account that is probably as good as the present state of available information allows.

FRELIMO was unusual among liberation movements. It was formed abroad and had no roots inside the country. Indeed, virtually nothing is known about protest movements inside Mozambique. After the murder of Mondlane it was dominated by its military wing. Its history has been beset by defections and personal and ethnic hatreds, yet no rival to it has ever succeeded in establishing itself. This is surprising for, as Henriksen points out, FRELIMO was always coldly received by the country's biggest ethnic group, the Makua, and made virtually no progress among the people of the *Sul do Save* region. FRELIMO still remains something of an enigma. At one level it was clearly an effective military and civil organization capable of mastering large areas of the country and organizing guerrilla campaigns on numerous fronts. On the other hand its successes after ten years were limited; it did not succeed in interfering with the Cabora Bassa dam; it had not penetrated any of the urban areas nor won over the population in at least half the country; it was taken by surprise by the Portuguese collapse in 1974; and the history of Mozambique since is clear evidence that the implications of victory had not been thought through.

These four chapters can be recommended as a good, up-to-date summary of the war of liberation, but the same cannot be said for the rest of the book. Though the author refers to many Portuguese works, the early chapters are principally summaries of well-known and easily available works in English. His coverage of these is far from complete, and there are no references to many of the theses written on Mozambique in the last fifteen years—for instance, those by Hafkin, Hedges, Alpers, Langworthy, Phiri, Schoffeleers, Mudenge, and Bhila.

There are other limitations to this first section as

a scholarly contribution to African studies. There are factual mistakes, sufficiently numerous to require comment. For instance, on page 105 we are told that the Boror Company was a subsidiary of the Zambesia Company and that the Niassa Company's charter ended in 1926 and the Mozambique Company's in 1942. All these facts are wrong. Another drawback is the journalistic style. The use of cliché, slang, and journalese not only jars the ear but also can detract from the force of what the author is trying to say. For example, "Mwene Mutapa's empire arched high in a golden curve before descending to fragmentation" (p. 8); "He sailed the Portuguese imagination to the outer edges of Mercator's projection" (p. 23); "smash and dash raids" (p. 32); "This thumping victory became a foundation of sand" (p. 38); and many, many more.

It is significant that the really bad writing occurs only in the part of the book where the author is summarizing the work of others. This section is really very slight, and it is not easy to imagine for what audience it has been written. It is a pity that more room could not have been found to expand upon the sensible and important points the book has to make in the last four chapters.

M. D. D. NEWITT
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NORMAN ETHERINGTON. *Preachers, Peasants, and Politics in Southeast Africa, 1835-1880: African Christian Communities in Natal, Pondoland, and Zululand*. (Royal Historical Society, Studies in History Series, number 12.) London: The Society. 1978. Pp. xi, 230. \$18.70.

Despite the pleasing alliteration of its title, the subtitle of this book is the better guide to its contents and aim, namely, "to focus attention on the African response to Christianity in southeast Africa" (p. 4). The response of the overwhelming majority to European missionary activities was one of suspicion and resistance. In Natal missionaries could have uses—strengthening a tribe's position, assisting in relations with colonists, providing education and other services—but acceptance was limited by irrelevance and fear that Christianity would destroy traditional relations of kinship and mutual obligation. In light of the extensive tribal fragmentation in Natal, this "generalised hostility" (p. 55) was remarkable. Resistance outside the colony was still more effective. The use of African state power meant that by 1873 "the combination of official restrictions and popular resistance had brought missionary operations to a virtual standstill in Zululand" (p. 83); Cetshwayo's accession hardly marked a turn for the worse!

Missionaries' early optimism soon evaporated.

Some circumstances were clearly unfavorable—for example, Natal “lacked the fluid social and economic conditions which led, elsewhere in Africa, to competition between Christian and non-Christian chiefs” (p. 70). Moreover, missionary numbers and denominational variety “provided a range of choice . . . rarely open to would-be converts” elsewhere in Africa (p. 100), provoked competition for converts, and lessened missionary bargaining power. Except for Bishop Colenso, whose hopes of partnership between church and state were dashed by the Langalibalele affair, most missionaries matched Nguni resistance with their own steady conversion to imperialism and support for the Zulu War of 1879. Yet these years still saw the increase of the *kholwa* (believers), who numbered perhaps 10,000 by 1880.

If religious inducements to conversion were few, the ability of missions to provide employment, homes, and ultimately land, were crucial in attracting converts where insecurity was rife and “the homeless were numerous” (p. 92). Norman Etherington examines the origins of mission station residents and illustrates how their sense of isolation from both white and black neighbors encouraged material and educational progress. Their adaptation to colonial capitalism was rapid and successful until stifled by colonial controls after 1880—responses paralleling those charted in recent writing on the peasantry of southern Africa. A most interesting chapter on *kholwa* political and religious initiatives demonstrates their essential moderation, as evident in the *Unzondelelo* movement of 1875. It is argued that the new self-confidence of *kholwa* communities only inspired religious separatism and sustained political activity when their skills and ambitions were denied broader fulfillment: by the sharp decline in secular opportunities open to Africans after 1880.

Using a wealth of material from American, European, and South African archives, Etherington writes well and relates his findings most constructively to the work of other historians like Sundkler and Ayandele. His book is of considerable interest and use to historians both of Africa and nineteenth-century missionary enterprise.

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R. KENT RASMUSSEN. *Migrant Kingdom: Mzilikazi's Ndebele in South Africa*. London: Rex Collings. 1978. Pp. 262. \$17.50.

Migrant Kingdom is essentially a narrative of the first twenty years in the history of one of southern Af-

rica's most important nineteenth-century states, that of Mzilikazi's Ndebele. As such, it is a far different book from the one that R. Kent Rasmussen originally intended to write. He had assumed that the broad outline of Ndebele history was well known, since so much had been written about them. On closer examination, however, he concluded that “most of what has already been written on early Ndebele history is so erroneous that it is almost worthless” (p. 1). Thus, he took upon himself the task of getting the facts about Ndebele history straight and undertaking a thorough investigation of their existence in South Africa prior to their migration north of the Limpopo River in 1838–39.

Rasmussen divides his study into five chapters, each representing a critical phase of Ndebele history. In the first, he examines Mzilikazi's early life and concludes that he had no special relationship with the great Zulu leader, Shaka. The second chapter focuses on the formation of the Ndebele state. Most historians have the Ndebele settling in the eastern Transvaal only briefly and then migrating directly to the Pretoria region. Instead, the Ndebele moved continuously about the eastern Transvaal for approximately two years and then migrated to semipermanent settlements along the Vaal River, before finally moving to the area of Pretoria. This phase lasted from 1821 until 1827, not 1823 to 1825 as previously thought. Next, the author focuses on the Ndebele in the central Transvaal from 1827 to 1832, concluding that the extent of their military power and the geographical scale of their warfare has been considerably exaggerated. In the fourth chapter, dealing with the Ndebele in the western Transvaal during the years 1832–37, Rasmussen finds far fewer problems with earlier interpretations. This is largely because of the existence of accurate secondhand and firsthand accounts. The final chapter covers the best-remembered event in early Ndebele history, that of the 1838–39 migration to present-day Matabeleland, with the author again rectifying several prominent errors of fact.

Rasmussen demonstrates the firm grasp of relevant secondary and primary sources necessary for a study such as this. Extensive footnotes refer the reader to previous interpretations and Rasmussen's reinterpretation of the extant documentary evidence. He combines his knowledge of the source material with a clear organizational and writing style to produce a thorough and sound study. He thus has made a significant contribution to the historiography of nineteenth-century South Africa. From now on, *Migrant Kingdom* will be the starting point for those writing about the early history of the Ndebele and related peoples.

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ASIA AND THE EAST

LOUIS ALLEN. *The End of the War in Asia*. Brooklyn Heights, N.Y.: Beekman/Esanu Publishers. 1979. Pp. xiii, 306. \$24.95.

Louis Allen, who served as a Japanese-language officer in the India-Burma theater of World War II, has written a number of books and articles on the military and diplomatic aspects of the Pacific War. In his latest work, Allen has attempted to provide an account of the human and political consequences flowing from the end of the war over a vast geographic expanse, covering Burma, Thailand, Indonesia, French Indo-China, the Indian National Army, Korea, Manchuria, and China. His book tries "to show the impact of [Japan's] surrender on the Japanese forces overseas . . . what problems it created for the returning Allies, what fears arose in the minds of the Allied prisoners-of-war as liberation came nearer, and most of all, what political changes it brought in its wake" (p. xii).

Using personal contacts that he established during the war and later renewed and expanded in addition to archival sources, Allen has explored the human dimension of the story with great skill. The admixture of a strident Japanese chauvinism and the naive idealism of pan-Asianism that often motivated Japan's wartime alliances with Asian nationalists is shown by Allen to have been based often more on emotional rather than rational bonds between the leaders involved. Its inner contradictions were quickly exposed toward the end of the war under the strains brought about by Japan's approaching defeat. The story of the personal betrayal and disappointment that this meant for many is told very well.

The author's treatment of the agony and suffering experienced by overseas Japanese settlers when they confronted the reality of Japan's shattered imperial design is often moving. His sketch of the plight of a Japanese settlement at Halahei, Manchuria, which vengeful Soviet troops and natives practically gave a bloodbath, is drawn with the feeling of a poet and the artistry of a craftsman. The dry statistics on war casualties can convey their poignant message only when they are "fleshed out" by such tragic episodes.

Allen's handling of the intricate political developments in Asia ensuing from Japan's defeat is, I am afraid, rather shallow. This may be because the author has attempted to cover far too many countries in one book. Much more detailed, sophisticated, and satisfying descriptions and analyses of the immediate postwar scene in Asian countries are available elsewhere. Allen is aware of many, though not all, of the other, more scholarly works on the subject, but, nevertheless, he seems to have convinced

himself that he could offer a book at once shorter and better! His description of the Indian National Army's ups and downs, for example, is appallingly brief and superficial. His analysis of the Korean political situation is, likewise, both brief and unfresh and shows no awareness of such fine works on the subject as Cho Soon-Sung's *Korea in World Politics*, (1967) and Frank Baldwin's *Without Parallel* (1974). Allen is informative and insightful on Korea only when he discusses the human problems stemming from the U.S. occupation's need for a temporary reliance on some Japanese bureaucrats in the teeth of Korean opposition.

It is because of his inadequate understanding of the political consequences of the war's end that Allen is able to issue such astonishing declarations as "the liberation of millions of people in Asia from their colonial past is Japan's lasting achievement" (p. 262). The distinction between forces that act as prime movers and those that act as catalysts was obviously lost on the author.

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PAUL HENG-CHAO CH'EN. *Chinese Legal Tradition under the Mongols: The Code of 1291 as Reconstructed*. (Studies in East Asian Law, Harvard University.) Princeton: Princeton University Press. 1979. Pp. xix, 205. \$19.50.

In this work Paul Heng-Chao Ch'en has made a significant contribution to the limited body of material available in Western languages on Chinese law. The book falls naturally into two parts. Part 1 is an extended description of Yuan legal institutions, divided into sections on the development of Chinese Codes, the penal system, and the administration of justice. In the section on codes he has not only provided a detailed history of the codification movement but also some ingenious alternative interpretations of the interrelationships of different collections of laws. His sections on the penal and judicial systems are also by and large good. However, one problem does arise. Ch'en has set himself the task of making clear the distinctive legal achievements of the Yuan. Thus he compares the Yuan systems with the legal systems of other dynasties. Unfortunately, because so little has been published on earlier Chinese law, he singles out various Yuan usages as distinctive traits when they are in fact very similar to earlier Chinese practices. For example he says (p. 46) that the Yuan granted amnesties "more frequently" than did other dynasties when actually they granted them far less often than had dynasties like T'ang and Sung. On page 73 he implies that the Yuan was distinctive in that its prisons were merely holding facilities, not places where men were

being punished by imprisonment. In fact this was true in theory of all dynasties. In summary, Ch'en has provided an excellent description of Yuan practices. But his assertions that the Yuan system differed in specific ways from the systems of other dynasties must be treated with great care. Read in this light, part 1 can provide the student with the clearest picture of Yuan law available in any Western language.

Part 2 is a meticulous translation of Ch'en's reconstruction of the Chih-yuan New Code, a body of laws promulgated in 1291 and subsequently lost. The author has assembled from other Chinese sources ninety-six fragments he believes formed parts of the original work, grouping them under ten headings: public regulations, standard of selections, governing of the people, management of finances, taxes and corvée, taxes and levies, warehouses, construction and manufacturing, prevention of thefts, and investigation of cases. The Chinese text immediately follows. A careful comparison shows the consistently superb quality of the translation. This part might have been improved by giving content footnotes to such Chinese institutions as the "paste-wall" and by using translations of technical terms (for example *ts'ao-tsei*) rather than romanizations, but these are minor blemishes on an outstanding translation. No doubt it will be widely used, as it deserves to be.

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HILARY J. BEATTIE. *Land and Lineage in China: A Study of T'ung-Ch'eng County, Anhwei, in the Ming and Ching Dynasties*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1979. Pp. x, 208. \$27.50.

Among the most important contributions to the social history of late imperial China were the studies of the Chinese "gentry" by Chang Chung-li and Ho Ping-ti published in 1955-1962. In different ways, Chang and Ho stressed the need for each individual to pass highly competitive examinations to acquire gentry status and the great upward and downward mobility within the gentry stratum that resulted. In *Land and Lineage in China* Hilary J. Beattie challenges this view by looking at the Chinese ruling class as it existed at the local level. Her arguments are drawn from an essay by Chang Ying (1638-1708) about the importance of landholding and from her study of his home country in Anhwei.

In the most important section of the book, chapter 2, Beattie gives a history of T'ung-ch'eng county and its elite families from the Sung until the twentieth century. Drawing on three gazetteers dated 1490, 1696, and 1827, she is able to trace the pro-

gressive settlement and economic development of the county. She is also able to show that a large proportion of the eminent people of the county came from six families, all well-established by the end of the Ming, and all closely connected through marriage. Only a small number of the men in these families acquired degrees, but the social standing of the families remained high, based on wealth derived from landholding and on the education it made possible.

In the third chapter, Beattie pursues the complex issue of the economic returns from landholding. Her principal argument is that the tax burden placed on landlords by the government was not so heavy as to make land an unattractive investment. Besides discussing the changes in the tax system, she shows how landlords could protect their interests. For instance, she cites cases of leading figures in T'ung-ch'eng, such as retired officials, petitioning for decisions favorable to local landowners when various fiscal reforms were proposed or instituted.

For an area in central China, T'ung-ch'eng had an unusually large number of established lineages. Genealogies for almost seventy different kinship groups survive, nearly ten times the number for any other county in Anhwei. Most of these lineages were formed in the late sixteenth century, the first steps initiated by ambitious or successful men who wanted to enhance their families' prestige. The history of these lineages is discussed in the fourth chapter.

The central question here is whether lineage activities were a major factor in enabling ruling-class families to preserve their wealth and power or whether such activities merely enhanced men's prestige, in much the way that other charitable or cultural activities did. Beattie implies that lineages were very important, but the evidence she presents could just as well have been used to reach the opposite conclusion. As she shows, the corporate property of the lineages was small, less than that of a prosperous individual landlord. The activities of most lineages seem to have been limited to annual ancestral sacrifices, compilation of a genealogy every few decades, and, in some cases, provision of small subsidies to students. There is no evidence that upper-class members of the lineages were interested in their peasant kinsmen or ever organized them in order to further their interests vis-à-vis other local groups. And many lineages apparently had no eminent members. While Beattie presents fascinating evidence of the sort no other scholar has assembled, the full significance of lineages of local social structure still awaits further research.

Land and Lineage in China deserves a wide audience. It refocuses many of the issues in the debate about China's ruling class, and it provides a model of the kind of close study of particular localities that

is greatly needed if we are to advance our understanding of Chinese social history.

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SILAS H. L. WU. *Passage to Power: K'ang-hsi and His Heir Apparent, 1661-1722*. (Harvard East Asian Series, number 91.) Cambridge: Harvard University Press. 1979. Pp. xv, 252. \$22.50.

This book deals with the intricate succession crisis created by the aspiring sons of the K'ang-hsi emperor during the last three decades of his reign (1662-1722). The emperor deposed, redesignated, and redeposed his heir apparent, Yin-jeng, and from the chaos emerged the Yung-cheng emperor, one of the contenders. This was first covered in chapter six of Silas Wu's previous publication, *Communication and Imperial Control in China* (1970); the present work grew out of that chapter.

The *Passage to Power* is the first book-length study of the subject. Unlike other similar studies, it attempts a psychological analysis. This attempt is commendable because the succession struggle is by nature a good background for the psychoanalytic treatment. The book has benefited from some primary sources. While preparing it, Wu traveled to Taiwan, China. There he had access to the Ch'ing archives, notably the records on K'ang-hsi's daily official activities, housed in the National Palace Museum. This archival material is valuable to the study of Ch'ing history.

Wu's work contains some problems, however. For instance, he neglects the fact that Yin-jeng was in a defensive position throughout the struggle. As crown prince he became the common target of his ambitious brothers and was constantly challenged by them. To consolidate his status he had to counteract their challenges. The interplay of challenges and counterchallenges threw the entire court into confusion. This situation led K'ang-hsi to accuse him of many "crimes" and to dismiss him. Although he was the victim of the struggle, the throne's accusations were recorded as the official interpretation of the succession issue. Finally, from this interpretation appeared a variety of seemingly independent versions. Accepting at face value the official verdict and its derivatives, Wu fails to see the nature of the struggle, which remains as much of a mystery as ever.

Wu has a tendency toward dilation. His charges against Yin-jeng and his supporters of coup and assassination (pp. 1, 123, and 152) are not documented. Wu also overextends the sexual theory. According to him, for example, Buddhist temples were

the "sites of amorous adventures" (p. 91). For "pretty girls" and "fair-looking boys" Yin-jeng even set up "a fake river project" (p. 95). But none of these assertions are verifiable.

Wu's dilation reaches its climax with the story of Tohoci, a Manchu of the Joogiya clan and once the Peking police chief in support of Yin-jeng. By mistake or convenience Wu describes Tohoci as an imperial consort's brother, and from that background portrays Yin-jeng's addiction to sex and plots (pp. 149-150). These dilations result partly from Wu's faulty notation. Most errors happen with the citations from the *Veritable Records*. Altogether, nearly 10 percent of the 529 notes require a major overhaul. For instance, page 219 note 57 and page 222 note 27, and the first references in page 198 note 2 and page 208 note 12 are either inaccurate or incomplete. Sometimes the references are actually nonexistent books (p. 223 n. 45).

Most serious, some of Wu's major points are identical to those in my *Autocracy at Work* (1975), which devotes more than 30 pages to the succession problem. But nowhere does Wu's book cite or list it. For example, my book is the first study identifying Yin-chih as an active contestant after 1712 (pp. 71-75). Wu treats Yin-chih in the same way (pp. 165-67). Moreover, I conclude that "as early as 1707" Yin-chih tried to win imperial favor by inviting K'ang-hsi to feast in his garden (p. 74). Wu writes: "As early as 1707, K'ang-hsi had begun to accept invitations to visit Yin-chih's garden" (p. 165). I attribute the defeat of Yin-chih in the struggle to his "single-handed" approach and to his lack of "strong supporters" (p. 74). Wu also concludes "... he [Yin-chih] was in fact no more than a 'single-handed' faction, without a strong power base..." (pp. 166-67). My book changes the first imperial son's name to "Yin-shih" (pp. 27, 313 n. 4), who had heretofore appeared as "Yin-t'i" in Western sources. Wu adopts the new name, too (p. 35). These examples, although one could give some more, sufficiently establish the reason for my grave concern.

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BRIAN HARRISON. *Waiting for China: The Anglo-Chinese College at Malacca, 1818-1843, and Early Nineteenth-Century Missions*. Hong Kong: Hong Kong University Press. 1979. Pp. xiv, 212. \$25.00.

Sixty years ago a biography of Robert Morrison, first Protestant missionary to China, could be subtitled "master builder" and aver in the preface that a fresh and promising chapter in the evangelization of China was about to start. No one talks that way today. The serial is over, the accounts in; the pre-

vailing verdict—it is certainly my students'—is that Christian missions were a piece of Western arrogance that deserved to fail and did.

Brian Harrison, professor emeritus at the University of British Columbia (and thus old enough to have once shared the earlier enthusiasm, did it suit him), proposes an alternative view. Morrison and his colleagues were not bigots. They were scholars and sensitive men. Through hard work they came to know a good deal about China, and (like the Jesuits of a previous age) they respected what they saw. They did not wish to ram Christ down Chinese throats. "Their aim was rather to set in motion a whole process of educational and cultural change... in the hope that new conditions favourable to the acceptance of Christianity would thereby be created..." (p. xiii). They wanted China Christian. But they expected conversion to occur naturally, as that isolated and straitjacketed society was gradually nudged into mankind's common stream.

"Waiting for China" (as if in contradistinction to Jonathan Spence's "To Change China") is an alluring epithet, an attractive proposition. It offers us the possibility of rehabilitating simultaneously, as it were, Morrison, his successors, and—to the degree we are tarred with their brush—ourselves. It is impossible, then, not to be pleased with the thing as it is put to us, briefly in the opening pages of this slight volume, lengthily at the end. But the evidence for the proposition is slim.

It is drawn entirely from the short life of the Anglo-Chinese College at Malacca—and that institution does not seem to me to have amounted to much. Morrison, who reached China in 1807, built the college on Malaya's west coast because he could not undertake it (or much else) at Canton. The Opium War lifted some China coast disabilities, whereupon the college moved to Hong Kong, changed its name and purpose, and effectively disappeared. Using chiefly the London Missionary Society's archive, Harrison meticulously traces its history. What his text does not give us in names, dates, things published by the college press, his three appendixes do. In particular, we follow in detail the strained relations between the society and Morrison. The society thought the college should train Christians. Morrison was determined it should turn out educated men. That determination is almost the whole of Harrison's case for the new Morrison, the "waiting for China" Morrison. It is not enough.

The proposition remains just that, a proposition, and the new Morrison is distinctly shadowy. Is Harrison himself really familiar with the man? Would you write of a person you knew well, an interpreter struck down by raging fever and stomach cramps at the height of a crisis that had exhausted him by its official demands, as Morrison was in 1834—would

you write simply that the poor fellow had "passed peacefully away" (p. 100)?

PETER WARD FAY

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N. M. KALIUZHNAIA. *Vosstanie ikhetuanei (1898–1901)* [The Boxer Rebellion (1898–1901)]. Moscow: Nauka. 1978. Pp. 362. 3 r. 20 k.

In June 1900 China shocked the world by declaring war on all foreign powers. It marked the culmination of the antiforeign movement led by a traditional type of secret society, I-ho-tuan, better known to Westerners as the Boxers. This outburst of popular frenzy with imperial patronage provoked the Allied siege of Peking and brought China nearly to national extinction. As a monumental historical event, with its far-reaching consequences, the I-ho-tuan Uprising has been a much-researched and interpreted topic. Collections of documents and memoirs as well as secondary studies abound in Chinese and Western languages. Now N. M. Kaliuzhnaia adds another tome to the growing number of monographs on this subject.

In her survey of existing historiography on I-ho-tuan, Kaliuzhnaia deplors that a study under true Leninist guidance is wanting and that Western bourgeois and Chinese historians share the tendency to overemphasize the imperialism of tsarist Russia in the event. Relying on a large corpus of source materials, she gives a conventional description of the causes of social unrest in China at the end of the nineteenth century and the origin of the I-ho-tuan. She then presents a phase-by-phase account of the uprising as the I-ho-tuan swept across northern provinces of China and into Manchuria. In analyzing the different components in the ranks of insurgents, she points out the role of conservative gentry and literati in guiding the movement. But her discussion of the bourgeoisie in China, which was represented by groups of reformers and revolutionaries, hangs loosely in the flow of events. She makes a fairly convincing, though not very original, point in asserting that the official reaction to the I-ho-tuan often reflected the local balance of political and social forces. When it comes to the crucial issue of Russian invasion and occupation of Manchuria, she glosses over key developments and obfuscates the issue with desultory vignettes on the insurgents. The Allied siege of Peking and the protocol signed at China's defeat are covered in haste as she approaches the end of her rambling narrative. She concludes that the I-ho-tuan Uprising was doomed to failure because feudal China was as yet too backward to withstand the onslaught of capitalist countries and that after the catastrophe the Chinese people began to understand the futility of freeing

themselves from imperialist bondage under the Manchu regime.

This garbled account of the uprising and a theme that has become trite could still be tolerable if the book provided some gems of the rich Russian documentation of this period that might throw new light on the movement. However, the paucity of Russian sources, except lengthy verbatim quotations of Lenin, used in this monograph is disappointing. Materials from the Central Government Historical Archive (Leningrad) are occasionally quoted to footnote minor points. For example, the meager wage given to Chinese workers in construction of the Chinese Eastern Railway is cited as an evidence of the imperialist exploitation of Chinese masses while motives behind building the railway are not discussed. It is also frustrating to read archival citations that contain only the classification number without reference to the agency of the source or the context in which the information was recorded.

In short, *Vosstanie Ikhhtuanie* may be a useful textbook for Russian readers who find a good deal of factual information and synthesis of secondary studies on the I-ho-tuan, couched in Leninist phraseology familiar to them. For others, it does not enhance a better understanding of the movement from a different perspective nor with the use of new documentation.

NAILENE CHOU

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ROBERT RUPEN. *How Mongolia Is Really Ruled: A Political History of the Mongolian People's Republic, 1900-1978.* (Histories of Ruling Communist Parties; Hoover Institution Publication, number 212.) Stanford: Hoover Institution Press. 1979. Pp. 225. \$7.95.

Robert Rupen, a professor of political science at the University of North Carolina, whose main fields are Russian government, Sino-Soviet relations, and U.S. national defense, is more widely known for his work on Mongolia. This most recent book is a summary and update of the neglected revolution of 1921, the first Communist revolution in Asia, from which the Mongolian People's Republic (MPR) emerged as the first satellite in the Soviet system and a prototype for those that later emerged in Eastern Europe.

Mongolia has been treated as a small country peripheral to everybody's concern. But now with new rumblings in Afghanistan, Iran, and other areas of Central and Inner Asia, it is imperative for more people to become better informed about the historical roots of contemporary problems. This study is

timely, also, in that it focuses on a strategic buffer state between the Soviet Union and the People's Republic of China.

Rupen demonstrates a commendable grasp of the facts and a generally detached approach. He does not idealize or romanticize the Mongolian revolution and its long, tortuous aftermath, and his critique in controversial areas is well documented. On balance, while he is sympathetic to the Mongols, he is critical of the overinvolvement of the Russians from the beginning and of the tragedies produced by revolutionary excesses of a Stalinist nature. The reader will look in vain for another volume that brings developments in Mongolia up to date with as much penetrating, critical analysis as Rupen.

The study is organized in three parts. Part 1, "Beginnings," includes an introduction, and covers the background from 1900 to 1920. Part 2, "The Age of Choibalsan," includes a discussion of the revolution and its aftermath, the period from 1929 to 1939 with its overshadowing influences of Stalin and Japan (until the latter's attack at Nomonkhan [Khalkhin-ghol]), and the period through World War II until the death of Choibalsan in 1952. The main contribution comes in part 3, "The Age of Tsedenbal," with sections on "China—Cooperation and Competition: 1952-1962," "China—Threat and Confrontation: 1963-1973," and a misnamed third section on "Developing Erdenet: 1974-1978." Larger than any of the three parts noted are the appendixes, notes, and bibliographies (87 pages).

While Rupen contributes insights to MPR political history, this is a very concise treatment, and one must still look to Charles R. Bawden, *The Modern History of Mongolia* (1968), for a fuller treatment of the earlier parts and to the *History of the Mongolian People's Republic*, translated and annotated by William A. Brown and Urgunge Onon (1976), for the extended, official Mongolian interpretation.

A dominant theme in the book, international relations, focuses on the problems of the triangular relationship of the MPR, the Soviet Union, and China. The role of Japan comes in for considerable discussion. The book is generally true to its design of being an analytical history, of the role of the Communist Party in Mongolia, but, considering the nature of the story, it is not surprising that the narrative often comes out in terms of military history and power politics. To this reviewer, there seems to be an overabundance of facts and figures in the narrative. The interplay of nationalism, ethnicity, and Communism are highlighted throughout the book. Rupen stresses that the party has been the main instrument by which the USSR has maintained dominance in the Mongolian People's Republic, but the real inside story of how Mongolia is ruled is still not clear. We still only have circumstantial evidence as

to how Russian advisors manipulate and how Mongolian leaders arrive at decisions.

PAUL V. HYER

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J. W. DOWER. *Empire and Aftermath: Yoshida Shigeru and the Japanese Experience, 1878-1954*. (Harvard East Asian Monographs, number 84.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1979. Pp. xiii, 618. \$20.00.

Shigeru Yoshida (1878-1967) is one of the least known and most self-assured statesmen of the postwar era. As prime minister for most of the period from May 1946 to December 1954, Yoshida guided Japan through liberalizing reforms and economic recovery under American military occupation, and he was the chief Japanese architect of arrangements for regaining national autonomy in April 1952.

This provocative and thoroughly researched book, by John W. Dower of the University of Wisconsin, is no standard political biography. Instead, it is a set of stimulating essays on Japanese capitalism and imperialism, linked but not confined to the public life of a diplomatist-turned-politician who was at once both pragmatic and principled. If *Empire and Aftermath* sometimes does not bring Yoshida fully to life or detail the record of his five cabinets, this seems a small price to pay for its sharp insights into wartime and postwar Japanese policy making.

Although well-born and closely associated with the throne, Yoshida, like most other Japanese imperialists in the 1920s, worried about Chinese nationalism, communism, and Soviet expansionism because they menaced Japan's continental interests. Despite his image in the West as a liberal and an Anglophile, Yoshida lobbied quixotically in the 1930s to reconcile the British and Americans to the "active policy" (p. 65) he consistently favored in China. Capricious and impulsive in method, Yoshida was a monarchist to the core.

Empire and Aftermath skillfully describes the anti-military maneuverings of courtiers, senior statesmen, and other top elites to topple the cabinet of General Hideki Tōjō in July 1944 and press for a negotiated peace the following spring. As a leader of this group, Yoshida feared both insurrection from below (perhaps led by Communists) and revolution from above (displacing the key prewar elites by "fascist" military officers or—after 1945—by new rulers under the Americans).

When he became prime minister in 1946, Yoshida tried to perpetuate the national leadership and goals of the pre-1937 period. Since this aim conflicted with the early democratizing reforms of

the occupation, Yoshida worked especially hard to blunt the new rights enjoyed by labor and the Communist party. Dower argues persuasively that the occupation underwent a "reverse course," emphasizing industrial recovery and rearmament, in 1948. The Yoshida era correspondingly blossomed from this point forward.

But Yoshida proved to be no reactionary. He helped to harmonize "the old imperial consciousness and patterns of authority" with the ideals of the "democratic revolution" (p. 317) of 1945-1947. Distasteful as he found many of the reforms, Yoshida worked to remove their sting rather than wipe them out. Because he believed the country's fate was tied to a postwar *pax Americana*, he realistically committed Japan to a mutual security treaty with the United States, despite charges of a sellout to the Americans from both left and right. Yoshida accepted American bases in Japan "not so much to guarantee its security as to regain its sovereignty" (p. 377)—the signal achievement of a remarkable career.

THOMAS R. H. HAVENS
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THOMAS R. METCALF. *Land, Landlords, and the British Raj: Northern India in the Nineteenth Century*. Berkeley and Los Angeles: University of California Press, for Center for South and Southeast Asia Studies, University of California, Berkeley. 1979. Pp. xiv, 436. \$22.50.

Western and Indian scholars of the high caliber of Thomas R. Metcalf have brought conclusively to light what has been wrong with Indian history writing up to recently. We simply did not have the facts. From Max Weber to Barrington Moore (*Social Origins*) and Gunnar Myrdal (*Asian Drama*), tempting generalizations about the Indian rural economy have floated into our literature—and, alas, our lectures—without much evidence to verify them. The "unchanging hierarchy of caste-bound control of land," the "transforming effects of British land policies," the "feudalistic zamindari system," and so on are attractive concepts that only the most meticulous researchers can effectively investigate. Metcalf has done it for the nineteenth-century central Ganges region, currently Uttar Pradesh state; it has been a work of some fifteen years, mostly carried on in the archives of Allahabad and Lucknow.

While this book's twelve chapters (arranged in three parts: pre-British, North-Western Provinces, Oudh), are smoothly written, some of them require close and repeated reading. The detail interrupts any attempt at rapid perusal. Readers unfamiliar with the lay of the land may scrutinize attentively each example cited under a general category and

become bogged down. Furthermore, Metcalf is cautious: he will not strive for meanings that go beyond the evidence. And the overwhelming impression that his chapters convey, particularly the early ones, is of the unmatched confusion of India's land-holding patterns. Occasionally Metcalf reports the lack of reliable statistics; for the rest he steers us manfully through a sea of treacherous diversity and contradiction, lands us briefly on an island of familiar meanings, and then plunges us back again. It was physically hazardous, he writes (p. 38), for the taluqdars (holders of estates) to venture into the countryside in the pre-British period. It is intellectually hazardous for us to do so now!

The book is about the taluqdars (zamindars of great holdings), on whom the British rested their control of the countryside and through whom they collected taxes from the villages. About half of the nearly four hundred pages sets those once-ruling proprietors into their post-Mutiny, mid-nineteenth century position; the remainder discusses their functioning as little more than landlords, intermediaries between the state and the farmers. As felicitously as possible, Metcalf juxtaposes the North-Western Provinces with Oudh, hoping thereby to elicit revealing comparisons. But in fact the effort results in two separate books, not one. In the North-Western Provinces, taken over one-half century before Oudh, British policies eased "the taluqdars down gently from their former preeminence" (p. 99) and replaced them with commercial zamindari classes of Brahmins and Banias. In Oudh, after a fling at breaking up taluqdari estates, the British decided after the 1857 rebellion to confirm that "gentry" class in permanent possession of their lands.

Metcalf is sympathetic to the taluqdars of Oudh. Within the limits set by their dependence on the British, they did fulfill their main function of managing usually vast estates. While noting their eccentricities, he avoids calling them rapacious or even exploitive (p. 307). They contributed to the preservation of old and cherished ways and upheld certain fused Hindu-Islamic cultural standards in the face of westernized urban leaders.

In assessing British policies, Metcalf describes their inconsistency: on the one hand they propped up the taluqdars as the focus of traditional loyalties and on the other hand tried to use them as English-style gentry to sponsor rural progress. It was left to independent India to cut through this dilemma and try to abolish the taluqdar-zamindar class. Both by accident and design, nineteenth-century rural society was transformed, if not upheaved, under British rule. Metcalf's patient probing of the often microscopic evidence reveals the many ways in which this happened.

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ROBERT ERIC FRYKENBERG, editor. *Land Tenure and Peasant in South Asia*. New Delhi: Orient Longman; distributed by Land Tenure Center, University of Wisconsin, Madison. 1977. Pp. x, 312. \$15.00.

The volume under review contains twelve essays ranging from analyses of early British land settlements in the late eighteenth and early nineteenth centuries to analyses of rural credit, farm mechanization, and land reform in India during the 1960s and 1970s. The authors range from historians (five), through political scientists (two), and economists (two), to a sociologist, an anthropologist, and a geographer. The editor's objective was "an anthology which could approach questions of land tenure from the perspectives of many disciplines" and that would "show agrarian problems as sets of symbiotic relationships" that "could not be dealt with effectively without reference to their entire past and present conditions." In the first respect the volume succeeds, and quite well: the contrasts among the various disciplines come out quite clearly. The effort to achieve symbiosis in holistic analyses is moderately successful, although the essays on British settlements do not go much beyond the analyses of fit and misfit between early British policies and the systems upon which the British impinged, with a revival of the old-now-become-new emphasis on privilege and exploitation. However, essays of nine to twenty-two pages (ten of the essays) hardly permit "holism" or reference to any "entire past and present." The closest one comes is the fifty-three page chapter by Hart and Herring on "Political Conditions for Land Reform: Kerala and Maharashtra": it is also one of the three most interesting essays in the volume.

Of the six strictly historical essays, four deal with south India, with more of the technical terminology of *inam* tenures than nearly anyone would want to know (Frykenberg may have earned the Heiden prize with nine technical terms and eleven caste names in eight lines of print). Adas's chapter on tenures of lower Burma is a good, short summary history, but the other essays among these six presuppose a fairly detailed knowledge of the history of early British Indian land policies and of the beliefs that have become common among scholars.

Perhaps because as an economic historian I am more economist than historian, I found the later essays more stimulating. Herring's "Land Tenure and Credit-Capital Tenure" neatly puts the proposition that the same conditions that give larger landowners disproportionate power in village life also give them disproportionate access to credit, with the two disproportions mutually reinforcing each other. Elder also neatly argues the plausibility (and does not try to argue a proof) that castes rising in the hierarchy align themselves with Brahmins of doubtful

authenticity who then validate the twice-born status of the rising caste and simultaneously validate their own status with the lands bestowed upon them. Hart and Herring (*supra*) make a strong case that some land reform and some decrease in inequality make further progress along these routes more difficult.

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C. M. H. CLARK. *A History of Australia*. Volume 4, *The Earth Abideth For Ever, 1851-1888*. Carlton, Victoria: Melbourne University Press; distributed by International Scholarly Book Services, Forest Grove, Oreg. 1978. Pp. xv, 427. \$16.00.

This book again proves that Manning Clark is one of Australia's greatest but most controversial historians. This fourth of a five-volume *History of Australia* shatters many orthodoxies held to be self-evident by historians on the period 1851 to 1888. Clark arrived at his conclusions from a series of studies beginning in 1950, when he published *Select Documents in Australian History, 1788-1850*. In addition to making available both well- and little-known documents, he provided short, illuminating introductions to eight sections. In 1955 there appeared a larger collection of documents covering the period from 1851 to 1900, most never seen before, and supplied with longer and more original and insightful introductions. In 1957 the *World's Classics* series produced Clark's *Sources in Australian History*, repeating none of his previously published documents, yet traversing the beginnings to 1918. Encouraged by many, this reviewer included, to write a multi-volume history of Australia, Clark responded with his first volume in 1962. It aroused enormous controversy. He paused to produce a stimulating but more traditional *Short History of Australia* (1963), then returned to his magnum opus, producing volume 2 in 1968 and volume 3 in 1973. He admits weariness in his preface to this fourth volume, and closes the story in 1888 (the centenary of Australia's British foundation), not his projected 1900. God and his health willing, he will complete in retirement (from the nation's first professorship of Australian history) a fifth volume, and with it a much needed annotated bibliography, absent since volume one.

In the preface to volume 4, Clark writes that he is summing up the themes introduced in earlier volumes: "the influence of the spirit of the place on human behaviour, the struggle between classes for the ownership of wealth, the struggle for political power, and the confrontation between Catholic Christendom, Protestant Christianity and the Enlightenment." He is concerned "with the debate in

Australia about the life of man without God." The major theme, indeed, is "the impending breakdown of bourgeois society, succeeded by an age of ruin—which is still with us" (p. vii). He does not agree with A. G. L. Shaw that these years brought the beginnings of a high standard of living for the general population or with Russel Ward that there was a noble mateship on the frontier. He does not believe that the capitalistic builders and engineers softened Australia's harsh environment, and bush life to Clark is brutalizing, disheartening, and degrading. Clark includes more cultural history—analyses of writers (but alas too few painters and architects), churchmen, and educators—than any previous historian. Superb vignettes, some quite long, appear of such writers as Marcus Clarke, A. G. (Banjo) Patterson, Adam Lindsay Gordon, and above all Henry Lawson. There are equally superb accounts of such political figures as Sir Henry Parkes, Jack Robertson, and Sir Redmond Barry, and of the bushranger Ned Kelly. Volume 4 reveals Clark writing at his best. Drawing many a phrase and sentence from the King James Bible and the Anglican Book of Common Prayer and Hymnal, he attains the highest standards of narrative history. Where can one find a better account of the lives and works of Clarke, Lawson, or Gordon, or the ill-fated Burke and Wills expedition, or the eruptions and degradations of the larrikins? His insights into the role of women in a man's world, the Sydney *Bulletin*, the Salvation Army, and the suffering aborigines, to cite just a few diverse examples, are fascinating.

Although brilliant, the book is flawed. Details are not always correct. The prose turns an occasional purple. The methodology is sloppy, particularly the footnotes, which are unclearly cited and overloaded with the primary materials he knows so well, but which fail to cite some of the latest scholarship. An example is the omission of Geoffrey Blainey's *Triumph of the Nomads* (1975) when discussing aborigines. Clark scatters moral judgments on too lofty a scale. He plays the psychiatrist too often when probing the hearts of his characters. Despite unevenness and prejudice, however, Clark has almost completed a multivolume history of Australia that has asked and answered the really tough questions and is worthy of comparison with Bancroft's work in the grandeur of its design and execution.

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W. G. MCINN. *A Constitutional History of Australia*. New York: Oxford University Press. 1979. Pp. xiii, 213. \$29.95.

It seems useful to begin this review by remarking on how, with an Australian focus, W. G. McMinn selects and treats material within the genre of constitutional history. The book's language is parsimonious and its arguments are terse. This helps but does not overcome the problem of compressing nearly two centuries of material into two hundred textual pages.

The reader is guided through the fits and starts from which emerged, in what was originally a convict settlement, the advisory, representative, and responsible governmental institutions and the legal norms that, as of 1901, forged an Australian political entity. McMinn's historical craftsmanship is sound. While he notes trends and consequences, he quite properly discounts any serious interpretation that constitutional growth in the "colonial" period prior to 1901 deserves to be romanticized as a popular struggle for democracy or attributed to carefully tailored plans in London. Moreover, in keeping with his stated purpose of writing a study of the working and evolution of the Australian governmental system rather than a strict study of constitutional law, McMinn skillfully weaves together the influence of economic trends, social change, personalities, and fortuitous factors and the synthesis between Australian originality and borrowing from British, American, and other experience.

The less than half of the book devoted to post-federation development is in these respects less successful. There is rather less of a "holistic" treatment of constitutional growth, though in partial defense it can be argued that by 1901 an identifiable Australian community and ethos had already emerged. Understandably, much attention is paid by McMinn to judicial renderings and rationales, but the reader does not quite get a sense of Australian jurisprudence. McMinn is justified in stressing the movement and ordering of federal and state forces. It gives rise to his insistent thesis that the system has moved from balance to federal primacy to federal domination. McMinn does little with the political and judicial extension of Commonwealth external powers to sustain this thesis, following his allusion to the 1930s version of the theme. Moreover, he skims over the recent yet highly significant party-political and federal-state bargaining features of "new" or "cooperative" federalism. While the subject has not held the same prominence in Australian as it has in American society, more than casual mention of personal liberties and group rights would have been welcome.

In general, my reservations about the book refer much more to what McMinn only fleetingly discusses rather than to the quality of what he does discuss. Any reviewer will raise some questions of interpretation about any particular book. It should, however, be emphasized that while McMinn has

opinions, they are on balance informed and reasoned, and he is careful about being tendentious. An example is his explanation of the Governor-General's 1975 action in dismissing the Whitlam Labor Government. Furthermore, McMinn's presentation is not fitted into some special philosophical or partisan casing, and it is largely free from being argumentative. McMinn's work is not readily characterizable as radical, revisionist, economically or judicially behavior-determinist, or whatever. While it is unlikely to inspire heated controversy, it will serve as a reliable, workmanlike source.

HENRY S. ALBINSKI
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ROB PASCOE. *The Manufacture of Australian History*. New York: Oxford University Press. 1979. Pp. 207. \$28.50.

The Manufacture of Australian History is essentially a historiographical study of some fifty or so Australian historians whose primary interest lies in Australian domestic history and especially in the problem of Australian national identity. The Australian past, marked by an evolutionary weaning away from Britain in the growth to nationhood, rather than the revolutionary break of the American experience, has presented many Australian historians with the interesting task of creating a national image or identity by seeking answers to such questions as what made the transplanted Briton an Australian, just when the change occurred, and what brought it about.

Of course the focus of the study primarily on those historians who have confronted such issues bars from consideration those who have dealt with other matters than the shaping of the images of the national past. Further, it puts out of court much of the work of those who, while they have written on Australian themes, have also written on other subjects. Sir Keith Hancock's work in the British civil history series of World War II is unnoticed, as is his biography of Jan Smuts, though much time is given to measuring the influence of his seminal work, *Australia*. Those who might want a broader survey of Australian historiography should resort to J. M. Ward's essay in A. L. McLeod's *The Pattern of Australian Culture*.

In his consideration of the work of his various authors, Rob Pascoe has utilized the classification developed by the American philosopher Stephen C. Pepper. Pepper said that there were essentially four main approaches to the historical process: the mechanistic, the formist, the organicist, and the contextualist. These various conceptions have certainly not made any great penetration into American historiographical studies, although they have

been used and lucidly explained by Hayden White in *Metahistory: the Historical Imagination in Nineteenth Century Europe*. But where White used this classification of ideas in his consideration of some eight major figures in European historiography, Pascoe applies the various categories to a much greater number of historians and in a much shorter book. The terms of the ideas they convey thus become more labels than tools of intellectual analysis.

All this leaves the reader in some confusion. Ward had rather deprecated the idea that there were substantially different schools of interpretation of Australian history. Pascoe gives schools aplenty, for in addition to Pepper's system, he also employs more commonplace categories such as conservative, liberal nationalist, Old Left and New Left, and a few others beside. And indeed, it is really in the latter terms that Pascoe discusses the historians who interest him.

The author writes that the boot of the European has left a lasting imprint on the Australian landscape; one senses as well the strong imprint of the European Left on Australian historical writing. Pascoe devotes more effort to the so-called New Left than is perhaps justified. For however much they have rattled skeletons and led to controversy in the pages of Australian quarterlies, the actual production of votaries of that school is rather scanty, even with the inclusion of the works of the odd sociologist or two whom Pascoe admits to Clio's shrine.

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UNITED STATES

HENRY SAVAGE, JR. *Discovering America, 1700-1875*. (New American Nation Series.) New York: Harper and Row. 1979. Pp. xvii, 394. Cloth, \$17.95; paper, \$6.95.

WAYNE FRANKLIN. *Discoverers, Explorers, Settlers: The Diligent Writers of Early America*. Chicago: University of Chicago Press. 1979. Pp. xiii, 252. \$15.00.

These two authors begin with the assumption, set forth on the dust jacket of Wayne Franklin's work: "that the discovery of the New World was not a single glorious act in 1492. It was instead a long process of progress and retreat . . . a process to which the power of the word was essential, and the diligence of the writers most basic." Henry Savage, Jr., deals with the physical America—the look of the lands beyond the horizon—and Franklin with the New World of the Mind.

The Savage book, one in the New American Nation series that has produced such notable works as

Billington's *The Far Western Frontier* (1956), is a useful but limited account of the "discovery" of America through the reports of explorers and naturalists. The assignment is extensive and produces awkward restrictions. The territory covered is the forty-eight contiguous states. The figures dealt with are men whose activities were widely reported in their time—thus ruling out, for example, Jedediah S. Smith's trailblazing travels. But there are substantial chapters on Lewis and Clark, Pike, the Astorians, Frémont, Powell, and outstanding naturalists such as Thomas Nuttall, John James Audubon, and Alexander Wilson. With a less comprehensive title, the author could have avoided the need to explain what he calls in his preface "the painful process of exclusion" (p. xvi), which has crowded out the Spanish in the Southwest and, to a lesser extent, the British and French. His sources are standard works, mainly dating before 1970; his chapter on Lewis and Clark would have benefited from the use of their *Letters* (1962), and the chapter on Astoria should not have been written without some reference to Kenneth W. Porter's essential *John Jacob Astor: Business Man* (1931).

Wayne Franklin has written an extended essay on the theme that "from the start language and event in America have been linked almost preternaturally to each other" (p. xi). He divides early travel books into three categories—discovery, exploration, and settlement—and confines his observations to a few representative but outstanding writers. Thomas Harriot's *A Briefe and True Report* (1588) is at once an account of past events and an assumption about the future of British America, a wide-angle view with a strong emphasis on anthropology. Antoine de la Mothe Cadillac, founder of Detroit, writes to his sponsors (1713) of his apprehensions about the infant colony of Louisiana. John and William Bartram travel the new land and publish their arresting observations on natural history (1751, 1791). Franklin suggests, with Crèvecoeur, that John Bartram's botanizing "began with a reaction against his own destructiveness—a guilt over white plantation" (p. 51).

Thomas Jefferson's *Notes on the State of Virginia* (1785) is properly identified as a work extolling the "regenerative powers of the New World scene" (p. 25), in refutation of the old canards of Buffon and other Europeans who saw nothing in the New World but degeneration or stasis.

Franklin is concerned with the preconceptions of New World travelers and the occasions when, not always inadvertently, they penetrated through some of the notions with which they came. "Like any other part of Old World order, language and its received forms—forms of literary art, but forms of discourse and syntax as well—were affected deeply by their transportation to America. Had Europeans

been almost wholly closed of mind, firmly set against any significant adaptations, it seems likely that the colonization of the West never would have succeeded" (p. 17).

A set of thirty-six plates illustrates Franklin's thesis that early artists controlled visual space in a way comparable to the control of verbal space by New World reporters.

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LELAND M. ROTH. *A Concise History of American Architecture*. New York: Harper and Row. 1979. Pp. xxvi, 400. \$25.00.

In 359 pages of concise text with 304 apt and generally sharp illustrations, Leland M. Roth of the University of Oregon at Eugene offers a balanced and perceptive history of American architecture. A model textbook for students, it is also an amateur's delight, for the writer's style flows enjoyably, his comments are informative, his examples are telling and often fresh, and his illustrations appear on the pages where the illustrated buildings are discussed. Even where one senses that the format and text evolved from a semester's lectures with slides, the reader also respects the seasoned scholarship and long gestation of themes evident throughout this well-constructed and well-documented history.

Roth starts his story with the American landscape and native builders, then devotes the second chapter to colonial transplantations and his third to post-Revolutionary architecture ending in 1820; he describes the lure of the past and promises of the future in 1820-65 and, remarkably and rightly, carries the period 1865-85 in a separate chapter, "Age of Enterprise." He discusses urbanism and the varied architectural responses in his sixth chapter, 1885-1915, and offers fine insight into "Dichotomy: Tradition and Avant Garde" in his chapter on 1915-40. The eighth chapter, 1940-70, and the epilogue, "pluralism in the 1970s," will attract the attention of scholars already familiar with the earlier history.

The fine exposition and sure judgment throughout this book far outweigh its few instances of superficial ideas or aberrant criticism. I doubt that urban plazas are "anomalies in American cities" because they embarrass "the Protestant work ethic" (p. 324). I doubt that the architectural story of 1960-80 can be told without considerable attention to the giant corporate real estate financier developers, including Zeckendorf. Surely it is not informative to conclude the chapter, 1940-70, with a sentence that purports to explain "the enlargement of scope of the architect's domain" (p. 332) but does not and, in fact, refers merely to a greater "com-

plexity" and "playfulness." More disserving still is the concluding epilogue where, after flitting about the styles of Rudolph, Roche and Dinkeloo, Johnson, Stubbins, Eisenman, Meier, Venturi, Portman, Pei, and many other fashionable architects—omitting Thompson, Netsch, Jacobsen, and others who are less heralded now but whom history may celebrate—Roth refers approvingly to the "whimsical classicism" of the 1978 model for the AT&T headquarters and quotes Philip Johnson's remark about "a new uncertain, unclear, but clearly fascinating future" (p. 359).

Such dalliance with style distracts Roth from the very substantial narrative he had been telling for eight chapters, including the epilogue: the story of American efforts to build cities and towns. In fact, his epilogue is especially insightful in discussing recent contributions to public space and urban contexts, a theme he had introduced deftly in earlier chapters. To allow the posturings of mannerist scene makers to distract him from the great urban contributions of contemporary architects is out of character with Roth's earlier chapters.

Errors in name, spelling, or fact on pages 39, 88, 127, 274, 279, and 348 are readily corrected by the reader without diminishing his respect for a fine, enjoyable, and concise history of American architecture.

ALBERT BUSH-BROWN
Long Island University

JAN COHN. *The Palace or the Poorhouse: The American House as a Cultural Symbol*. East Lansing: Michigan State University Press. 1979. Pp. xii, 267. \$15.00.

Increasingly nowadays the symbolic and metaphorical content of architecture, neglected for several generations in favor of formalistic concerns, is being rediscovered. An important milestone in this process is the appearance of Jan Cohn's *The Palace or the Poorhouse: The American House as Cultural Symbol*. Cohn's fundamental viewpoint is literary; the first part of this study examines how houses have functioned as symbols in literature. A mass of examples from the very earliest settlements in Massachusetts, Virginia, and Georgia down through nineteenth- and into twentieth-century novels conclusively shows that, no matter how compulsively leaders of the reigning avant-garde Establishment dictating architectural education insisted that no other concern than functional should determine building forms, in fact the mass of the population continued to perceive a symbolic element in architecture, to value architecture for metaphor and symbolism above all other considerations. In the process a wealth of new historical insight has been revealed. No effective study of American culture can be

made, henceforth, without reference to the kind of evidence Cohn shows to be inherent in architecture. Even more significantly, however, Cohn (who has long been prominent in the Popular Culture Association) has demonstrated how this traditional perception of how architecture works in and for society still lives on at the popular and commercial level. Her thesis builds to a climax, so that in some respects the last few pages constitute the heart of her argument. Talking about the architecture of Levittown, she observes that, "Like the houses inspired by the Colonial Revival of the 1870s, the Cape Cods and Colonials of Levittown have an aura of tradition and association. Although a family may buy such a house planning to inhabit it for as few as five years, the historical styling politely ignores their transience and provides an architectural symbolism that speaks of stability and permanence."

This, ultimately, is what house-building has always been about. It is the most visible symbol of having arrived at a fixed place in society, and so functions today on the popular and commercial level: "In many ways the American house has become more symbol than reality. The individual house is often no more than one in a series of houses; yet it assumes to itself the values once accorded only the ancestral house, establishing itself as the temporary representation of the ideal permanent home. These houses, furthermore, stand for property, despite the fact that most are held by banks to whom the residents pay mortgages. These serially-owned, bank-held houses are, with increasing frequency, the unindividualized results of mass-production and design. Yet, a man's house is still considered the expression of his own taste and character, and men and women find in their homes the greatest opportunity to express their personal taste. The activities of the house, as well, have changed. Home industries have long since vanished and home functions of child-rearing, education, and care for the sick and elderly have passed to public institutions. Only laundry and food preparation remain, and should they disappear as Gilman suggested over seventy years ago, the home would survive their departure. In the last century and a half many changes have occurred in the physical house. Once fashionable styles are now quaint survivals, and technology has altered techniques of construction. But the symbolic house in America remains virtually unchanged. The aspirations and satisfactions experienced by the man who lived in one of A. J. Downing's Gothic cottages are the aspirations and satisfactions of today's suburban house-holder, whose house remains the evidence of property and the emblem of home." Why try to paraphrase a summary like this? It should be written out and prominently displayed in architecture schools all over the country. Maybe if more architects began

thinking in terms of what a house is about, what houses are meant to *do* in and for society, rather than simply about their composition and plumbing, there would be a lot fewer unemployed ones than there currently are. Better still, make the book assigned reading in architectural design as well as architectural history courses.

ALAN GOWANS
University of Victoria

JOSHUA C. TAYLOR. *The Fine Arts in America*. (The Chicago History of American Civilization.) Chicago: University of Chicago Press. 1979. Pp. xvi, 264. \$17.50.

Joshua C. Taylor has written a useful, provocative short history of the arts—mainly painting—in America. The author is director of the National Collection of Fine Arts in Washington, a Smithsonian branch specializing in American painting and sculpture, which he has made into a fine scholars' museum. His book, as one would expect, is readable and literate. It will be read profitably by a wide audience, from beginning students to specialists, as it functions both as a quick survey of the field and as a thoughtful, iconoclastic summary of the current state of scholarship in the American field.

Beginning in 1670 and coming up to the present day, Taylor weaves a fluent discussion of major artists, patrons, institutions, cultural events, and stylistic developments without resorting to the traditional clichés, to "isms," or to art-historical jargon. American art is seen simply as one aspect of our history, and the author's clarity of thought and language properly signifies the extraordinary creativity of Americans over the years. At the very beginning, he rejects the popular question, "What is American in American art?" He comments that "for the nationally oriented mind,"—and, one might add, for most earlier historians in the field—"European influence becomes a subtle opponent, a force luring the artist away from his true national character." He thus rejects the traditional, now meaningless, groupings: Homer, Eakins, and Ryder as the great "native" American painters or Whistler, Sargent, and Mary Cassatt as the balancing triumverate of expatriates. Rather, Homer is here discussed in a short section entitled "Perception and Truth," which introduces Chevreul's theories and Corot's style before discussing the work of Eastman Johnson, then Homer, and finally bringing in Émile Zola! Eakins also is given a new place, without any diminution in his reputation; Taylor finds in his art "a taut astringency very nearly unique in America," and he concludes in a typical laconic, telling way: "Later critics have liked to cite Eakins's art as char-

acteristically American; few of Eakins's contemporaries would have agreed."

Taylor's initial chapter takes on the first century of our art, 1670 to 1776, and in a few pages makes sense of our earliest styles in painting, furniture, and architecture. Here as elsewhere the tiny marginal illustrations in black and white may serve as references for those already familiar with the objects, but I should think a beginner would have to use them in conjunction with better reproductions or the originals. This chapter gives promise of bringing to bear a knowledge of all the arts, but the subsequent ones develop an increasingly narrow focus on painting and sculpture. Architecture, furniture, silver, prints, even drawings and watercolors are given short shrift; presumably they fall outside Taylor's definition of "fine arts."

The second and third chapters take us from 1776 to 1900 and form the richest part of the essay—understandably, as this period is the author's specialty. The development of genre, landscape, and still life is nicely tied to changing American attitudes and to the major fairs of 1876 and 1893. History painting is given its rightful place, for a change, as is popular art (the prints of Currier and Ives, the plaster Rogers groups, and so on). This makes the total omission of "folk" or rural art all the more difficult to explain. Most American art in a sense represents a more or less naïve reflection of high style art in England, France, or Germany; Copley relates to Reynolds as Ralph Earl or Winthrop Chandler relate to Copley. Failure to mention these "Connecticut school" followers of Copley, or to give any indication at all of our immensely rich, widespread folk tradition in the half-century before the Civil War, seems nearly inexplicable.

It is hard to argue with such omissions, however, as the author has promised us not a survey or a list but a suggestive essay; indeed both the book's strengths and its weaknesses lie in its idiosyncrasies. Thus, one can forgive inclusion of an eighteen-page chronology ("Events in Art," "Other Events"); though I cannot see its purpose, it may be useful to some. Less acceptable are a number of aggravating typographical and minor errors. It is even harder to justify having two illustrations of Catlin's work, one of George Loring Brown and James Hamilton (these four, like too many of the illustrations, are taken—apparently chauvinistically—from the National Collection of Fine Arts collection), while no mention at all is made of M. J. Heade, Fitz Hugh Lane, or Sanford Gifford. Taylor may believe that the term "luminism" has been misused or overused, but this is no excuse simply to avoid three of its major practitioners or to illustrate such an atypical, nonluminous example by a fourth, J. F. Kensett.

Two final chapters deal competently with the periods "1900–1945" and "Since 1945." Taylor dis-

cusses the concept of modernism, and, as before, gives emphasis to style, patronage, and popular taste. He avoids the old notion of art history as biography and in doing so tends to stress cultural pattern rather than individual genius. This is all to the good. And equally welcome is the author's constant stress on the variety of artistic inspiration in America: for example, the teens are seen both as a time of abstract experimentation and as one that saw the academicians retain their dominance. On the debit side, a continuing Washington bias must be noted; a greater problem is an increasing sense of fragmentation, as the author jumps from one artist to the next, as he tries in a short space to mention each of the major figures of our own incredibly busy century.

Finally, one might note that Taylor's bibliography is just two pages long and includes only one book on painting since Richardson's still nearly standard survey of 1956—that is, Barbara Novak's *American Painting of the Nineteenth Century* (1969). This is an intriguing citation, for no two recent books disagree more—both methodologically or critically—than Novak's and Taylor's: in fact, Taylor's current essay might almost have been written as an antidote to Novak's thesis, which selectively traces a mainstream American style that is by definition luminist, objective, and linear. Novak's book represents perhaps the last, best effort to isolate the qualities that make American art American; Taylor's may be the first to attempt a different, non-judgmental, historically-oriented synthesis. The two books, read together, provide as useful a survey of the fine arts in America as one can now obtain.

THEODORE E. STEBBINS, JR.
Museum of Fine Arts,
Boston

G. THOMAS COUSER. *American Autobiography: The Prophetic Mode*. Amherst: University of Massachusetts Press. 1979. Pp. vi, 222. \$15.00.

This book builds on a group of separable but related assumptions. (1) There is an important tradition of American autobiography, beginning with the colonial Puritans and Quakers, in which the self is conceived as representing the community at large. (2) Characteristically, the mode of representation is prophetic—the self embodies a vision of the community not as it is but as it ought to be. (3) This "prophetic mode" makes that very process of self-portrayal a means of denouncing what is by reference to the will of God. (4) The will of God, so defined, magnifies the autobiographical self to virtually cosmic proportions; the individual becomes at once the keeper of the higher spiritual laws and harbingers of American destiny.

None of these assumptions is original with G. Thomas Couser. As he acknowledges in his introduction, he culled them from various scholars in the field. What might be considered his own contribution is to trace the pattern through a series of personal writings spanning more than three centuries, from the Puritan "spiritual autobiographies" through the works of Woolman and Franklin to the predictable nineteenth- and twentieth-century figures: Thoreau, Whitman, Frederick Douglass, Henry Adams, Louis Sullivan, Frank Lloyd Wright, Gertrude Stein, Malcolm X, and Norman Mailer. The result is a sweeping and superficial overview of "American culture." The book reads like one of those all-too-familiar inter/multi/non-disciplinary courses in American Studies, a mishmash of personalities, ideas, and approaches, held together by a simplistic theme more or less chronologically developed.

To be sure, Couser tries hard to vary the theme. Following Daniel Shea, he distinguishes Quaker from Puritan concepts of the self. Following James Cox and William Spengemann, he shows how Franklin blended and secularized those concepts in making himself the exemplum of a rising nation. Following other critics and scholars (not all of them adequately acknowledged), he contrasts the uses of American selfhood in Thoreau, Douglass, and Sullivan and outlines the continuities from Whitman to Wright, Adams to Mailer. The "prophetic mode" lends itself to a variety of extremes: euphoria and despair, celebration and denunciation, self-vaunting and self-effacement. Couser notes the interplay between extremes, and describes how different autobiographers move toward one extreme or another. All this is worth reading as a summary of previous scholarship on the subject. But in its own right this book tells us virtually nothing new about "autobiography" as a genre or about "the prophetic mode" as a form of rhetoric and thought or, for that matter, about the anxiety of being American in any concrete historical or psychological sense. A common pattern is *there*, evidently, but that should provide the beginning for the inquiry, not its rationale and end.

Couser is at his best when the pattern he adopts leads him into controversial areas (for example, the ironic or ambiguous ending of Adams's *Education*). He is at his worst when he ventures into areas he apparently does not understand (Puritan rhetoric, for example). But by and large his tone has the blandness of extended synthesis and recapitulation. And therein may lie the book's chief value. Any work of summary has at least potential heuristic value; and by indirection, as it were, Couser may alert students of American autobiography to problems he himself fails to consider—for example, the social function of "the prophetic mode," the ide-

logical dimension of our "ongoing and almost obsessive inquiry into what it means to be an American" (p. 9), the formal and conceptual dynamics of a genre that thrives on "perpetual crisis" (p. 200), the tensions inherent in asserting oneself, verbally and figurally, on a "representative" scale, and most broadly, the psychic, imaginative, and cultural implications of transforming "American" from a civic into a "prophetic" identity, and of investing "America" (as the source and object of prophecy) with attributes of the divine.

SACVAN BERCOVITCH
Columbia University

BARNET BASKERVILLE. *The People's Voice: The Orator in American Society*. Lexington: University Press of Kentucky. 1979. Pp. 259. \$14.50.

The People's Voice essays what amounts to a social history of American oratory. Discussing famous speeches and speakers from the 1760s to the present, Barnet Baskerville describes changing modes of rhetoric and changing attitudes toward oratory as reflections of the changing character of American society and politics. He is widely read in his subject—he has devoted his career to the analysis of American rhetoric—and his book touches gracefully and often perceptively on a host of familiar phenomena not often approached from his point of departure. In all these respects his volume makes a useful study, and it culminates in a deeply felt appeal to the present generation to restore some form of eloquence to public deliberations.

Nevertheless the book promises more than it delivers, and it falls far short of exhausting the possibilities of cultural or social history. (This is, perhaps, only to be expected of someone who is not a professional historian; but we must review the book as a contribution to historical inquiry.) On the most elementary level the history to which Baskerville relates oratorical developments is at best conventional, at worst superficial. In dealing with the period after the Civil War, for example, he invokes Morrison and Commager but makes no use of Kleppner and Jensen, whose work might better have underlain his analysis. More generally, his account often resembles an old-fashioned political history of the United States written with special reference to classic performances like Patrick Henry's orations, Daniel Webster's second reply to Hayne, and the Lincoln-Douglas debates but extended by reflections on such matters as Theodore Roosevelt's presidency and the Kennedy-Nixon campaign of 1960. (Interestingly, it mentions Woodrow Wilson only in passing.) The reader may indeed gain some new understanding of the rhetorical strategies em-

played on these occasions, but the rhetorical analysis and the historical analysis do not significantly enhance each other.

Neither can the reader develop any sure perception of the roles that oratory may have played in American society. Baskerville holds that the quality of public rhetoric coincided with the importance each generation attached to political events, but the proposition does not carry him far. On his own evidence, there were questions to be asked about how the evangelical tradition may have shaped American discourse, how certain fundamental characteristics of American society may have influenced its reception, and what literary criticism, social psychology, or cultural anthropology may tell us about the early republic's preoccupation with eloquence. Answers to questions like these are admittedly all too likely to be glib or formulaic, but it is difficult to see how else one can truly illuminate the role of orators and of oratory in American life.

RUSH WELTER
Bennington College

W. ANDREW ACHENBAUM. *Old Age in the New Land: The American Experience since 1790*. Baltimore: Johns Hopkins University Press. 1978. Pp. xii, 237. \$13.95.

In recent years, following quickly on the heels of the previous decade's intense interest in the history of family life, there has emerged a small but growing body of work on past attitudes to the various stages of life. First it was childhood, then death; more recently, adolescence and old age. Slowly, perhaps warily, we are closing in on the middle. In each case initial work has been bold and speculative—followed by deeper, more cautious, more detailed analyses. As for the study of old age in America, the former approach was best represented by David Hackett Fischer's *Growing Old in America* (1977); for the latter we now have W. Andrew Achenbaum's *Old Age in the New Land*. Mention of Fischer's seminal work, at least in passing, is imperative here—if unfortunately so—since Achenbaum does not discuss it at any length and even fails to include it in his bibliography (while dutifully listing James Fenimore Cooper's *The Prairie* and the stories of Sarah Orne Jewett), despite the fact that Fischer's book is the only other full-scale study of the subject and that Achenbaum read that work in manuscript and reviewed it (quite critically) well before his own book was published.

Now, having said that, let me quickly go on: *Old Age in the New Land* is an insightful, carefully constructed, well-argued, complex, and important book. Without falling into the common trap of exalting the past as a golden age of invariable domestic warmth and intergenerational cooperation, sup-

port, and respect (from which we, of course, have sadly fallen), Achenbaum does carefully trace the steady degradation of the aged in the post-Civil War years—with, he suggests, an abatement of the worst of those conditions during the past several decades. Eschewing the temptation to interpret this general decline in status as directly consonant with the familiar shift, in so-called high culture, from romanticism to realism (indeed, one of the particularly insightful aspects of Achenbaum's entire study is its careful delineation of the differences between the nature and chronology of intellectual and social change relating to the aged during the course of American history), he weaves an intricate web of explanation involving the overlapping of scientific, bureaucratic, and popular thought with the emergence of major changes in medical and economic realities.

This is an ambitious book, doubly so because it attempts to cover so much in so little space: 190 years of history in 171 pages of actual text. Thus, while it is remarkably dense and complex for a work its size, it is inevitable that this limitation be evident somewhere. That somewhere can be seen most clearly in the heavy burden a relatively frail number of sources must carry; this is especially true when the author is attempting to elucidate the attitudinal aspects of his argument—that is, when he is working the side of the street generally given over to the cultural and intellectual historian. Much of the argument late in the book concerning relative states of well-being among the aged at different times in recent history, for instance, relies disproportionately on government economic data. Although he does not explicitly engage recent controversies concerning modernization theories in his historical narrative, Achenbaum does in the end construct an argument that is in large measure consistent with such theory.

Old Age in the New Land is, finally, a work of sophistication and compassion—a work that, despite its disagreements, is complementary to Fischer's pioneering study and a work that should stand for some time as a landmark in the analysis of the historical meanings of old age in America.

DAVID E. STANNARD
University of Hawaii

RICHARD L. BUSHMAN *et al.*, editors. *Uprooted Americans: Essays to Honor Oscar Handlin*. Boston: Little, Brown. 1979. Pp. xvii, 366. \$15.00.

Few historians of recent decades have established a larger presence within the discipline of history than has Oscar Handlin. Scholar, teacher, academic entrepreneur, critic of the discipline and the profession—Handlin has expended his restless energies in

a hundred distinctive and consequential ways. Students, both graduate and undergraduate, scholars enjoying the amenities of the Charles Warren Center, investigators of American social history, colleagues who have learned from and argued with him about the study of the past—the list of persons who have been influenced by him is large. How appropriate, then, that some of his former students should gather together a group of their own essays to honor him.

If a teacher's stature is measured in some part by the students who claim his or her tutelage, then the roll call of contributors to this volume offers impressive evidence of Handlin's standing. Not many of us can claim such distinguished lineage. Nor are there many whose students have ranged so widely or so effectively through American social and cultural history. Certainly it is true that the diversity of topics included in this volume reflects primarily the developing interests of the authors themselves. And yet, as Richard L. Bushman points out in his introductory statement, Handlin's own eclecticism—he posed “no formal method beyond the forceful and intelligent use of fact and reason”; he transmitted “no sense of forbidden or preferred paths” (p. x)—encouraged his students to strike out on their own. Even Handlin's increasingly vociferous “aversion to formalized hypothesizing” and “overwrought social-scientific methodology” (p. x) has not stood in the way of his students' engagement with the social sciences. Richard L. Bushman and John Demos give evidence of that.

The organizing theme of Handlin's scholarship, Bushman reminds us, has been the “systematic study of plain people” (p. x). In one way or another, each of the ten essays presented here bears witness to that concern. Every reader, depending upon taste and inclination, will find his or her favorites among them. My own include Demos's marvelously evocative account of witchcraft in the historical experience and memory of a New Hampshire community; Bushman's sensitive discussion of the revolutionary generation's rejection of dependent patronage assumptions in political and social relationships; and Neil Harris's imaginative analysis of the “consciousness revolutions” reflected in American utopian fiction of the late nineteenth and early twentieth centuries. But there are others—by Anne Firor Scott, Roland Berthoff, William McGloughlin, Paul Goodman, William Taylor, Arthur Mann, and Moses Rischin—that merit attention as literate and informed essays in historical exposition. Barbara Solomon offers a brief word portrait of Handlin at the beginning, and Robert Mirak provides a selected bibliography of Handlin's published works at the end.

Oscar Handlin has in recent years lamented his sense of the collapse of scholarly community among

historians. He reports that he has felt himself standing alone, reading and writing each book, he has said, as if it followed and preceded nothing else, as if the scholarly enterprise now lacked both context and connection. Surely these essays offer evidence that important connections still exist and that scholarly community, though dramatically more diverse than it used to be, remains a reality.

JOHN HOWE
University of Minnesota,
Twin Cities

W. STITT ROBINSON. *The Southern Colonial Frontier, 1607–1763*. (Histories of the American Frontier.) Albuquerque: University of New Mexico Press. 1979. Pp. xvii, 293. Cloth \$12.50, paper \$6.50.

When Frederick Jackson Turner in 1893 told us about the significance of the American frontier he made such an impact upon American historical writing that the reverberations echoed far into the next century and continue even today. W. Stitt Robinson's narration of the expansion of the southern frontier in the era, 1607–1763 is the latest addition to the projected fifteen-volume series of the American frontier edited by Ray A. Billington and Howard Lamar that is designed to round out the frontier experience as originally suggested by Turner. Some of these volumes, published and scheduled for print, are topical dealing with the fur trade and law on the frontier; others like Robinson's volume are chronological segments of frontier history tracing a steady advance from earliest times through the end of the nineteenth century. Very probably the volumes of chronological narratives will end about 1893, the year that Turner saw by census data that the frontier had passed. Since much of Turner's analysis had come from colonial and early frontier developments east of the Mississippi, it is appropriate that Robinson's study fill in gaps left by Douglas Leach's *Northern Colonial Frontier*, Jack M. Sosin's *The Revolutionary Frontier*, and Reginald Horsman's *Frontier in the Formative Years, 1783–1815*. These and other volumes in the series generally set forth themes of progress under Turner's theoretical scaffolding of ideas placing emphasis upon an “Americanization” process; a frontier influence upon the formation of the American character and the growth of political rivalries; a traditionalist environmentalism (emphasizing the impact of the wilderness upon pioneers rather than their impact upon the land and Indian people); and the emergence of sections and sectional conflict. Robinson, for example, writes about a southern frontier as opposed to northern and middle colony frontiers and recognizes the beginnings of sectional rivalry between the tidewater settlers and the inte-

rior frontier society. And, like Turner, he argues against a "monocausationist" approach to frontier history and yet at the same time says that the frontier was "one of the strongest factors" in shaping the growth of a predominately agricultural society. Robinson's book follows Turner's lead in telling a story of Anglo-European settlers' expansion into the wilderness. There is a lot about Indians in this book but not much about Indian people in what is now called ethnohistory. Nor are blacks recognized as having any real force in early frontier history.

Robinson, a seasoned scholar, gives us a thoroughly documented series of chapters based upon wide investigation of both sources and secondary works. He begins his book with the first settlement of Virginia and early experiments in agriculture and manufacturing at Jamestown. His descriptions of Indian attacks are given to us as "concerted action" (rather than as conspiracies). Maryland's manorial society, prospering on a tobacco economy, is closely linked to Virginia. Bacon's rebellion is explained in its multiple causes, and Bacon himself is rejected as "a torchbearer of the Revolution." Thereafter follows a narrative of the emergence of a Carolina coastal frontier and a fur and skin trade penetrating hundreds of miles into the southern wilderness.

Although the Indian slave trade is mentioned, we might have had more of an analysis of its complexity and its relation to black slavery and Indian politics coming out of frontier violence and the Yamasee War. Nevertheless, Robinson is to be congratulated for weaving a large mass of detail into a readable narrative based upon the origin and growth of the southern colonies and their common frontier up to the time of "the War for Empire." And throughout the book there is the essential thread of frontier expansionism. Here, then, is a volume in the tradition of Turner and another predecessor many of us admire, Verner Crane.

WILBUR R. JACOBS
University of California,
Santa Barbara

DAVID THOMAS KONIG. *Law and Society in Puritan Massachusetts: Essex County, 1629-1692*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1979. Pp. xxi, 215. \$21.00.

David Thomas Konig has introduced a significant dimension to the debate on Puritan Massachusetts's transition from a communal to a more modern, impersonal society in this closely reasoned study of the relation between the development of legal institutions and social change in Essex County. It is his contention that the colony's relative stability in the

midst of profound change was due largely to the magistracy's ability to reconcile conflicts and maintain social control. Unlike most recent historians, he regards the remarkable litigiousness of the settlers after 1660 as a method of integrating the society rather than as a symptom of "declension." Given the floundering state of Puritan studies, this is a most helpful hypothesis.

The author also asserts, apparently as a corollary, that the towns and churches played relatively insignificant roles in forging and maintaining order. This provocative suggestion is more dubious. If correct, it would force a substantial reinterpretation of American Puritanism. Unfortunately, Konig's sketchy portrait of town and church government is static. He dismisses too readily the possibility that these institutions developed in a manner roughly parallel to the court system. The Scotch verdict of "not proven" seems just on this portion of his argument.

Yet however exaggerated his notion of the rigidity of all local institutions except the court, Konig has demonstrated that a lively picture of socioeconomic change can be gleaned from the court record. No scholar has read those voluminous manuscript records more sensitively. His skillful melding of social and legal history is of great value and makes fascinating reading. Of particular interest, especially to nonspecialists, is his account of the struggle between those who favored and those who opposed a legalistic approach to order and the impact of that struggle on the witchcraft outbreak and subsequent trials in 1692. Historians have long held that the timing of the crisis was related to the great uncertainties of the Dominion era. Yet few have been able to find a credible link between high politics and local hysteria. Konig has found one in the conflict over the authority of local legal institutions whose legitimacy had been compromised by the revolutionary circumstances of the period.

Recent colonial historians have been battering at the dominant town-centered approach to New England. This book, rich in ideas and astute in the use of evidence too often ignored, contributes markedly to the trend. By shifting the focus to the courts and by substituting concepts from anthropology for ones derived from demography, it offers an alternative method as well as criticism. The book's merits are considerable and its arguments ought to be pondered.

RICHARD P. GILDRIE
Austin Peay State University

BRUCE C. DANIELS. *The Connecticut Town: Growth and Development, 1635-1790*. Middletown, Conn.: Wesleyan

University Press; distributed by Columbia University Press, Irvington, N.Y. 1979. Pp. xii, 249. \$16.00.

Bruce C. Daniels welcomes the findings yielded by the case-study approach to an analysis of New England's early towns. He believes, however, that these findings—series of microscopic slides, as he terms them—must be merged, along with other materials, so as to produce colony-wide or regional pictures. In requiring a wider angle of vision, he follows the lead of Michael Zuckerman's *Peaceable Kingdoms* (1970) and Edward M. Cook's *Fathers of the Towns* (1976). In seeking useful models among the social sciences, he conforms to the current colonialist norm. Treating the evolutionary aspects of town settlement and formation, of population and its distribution, of town-meeting government, of ecclesiastical societies and other proliferating local institutions, and of central places, he argues that institutional elaboration and economic interconnection led Connecticut's towns to the threshold of urbanization.

Daniels's study—easily the most ambitious examination of Connecticut communities to appear to date—is a most useful account. Yet it is marred by numerous flaws. At times data are mishandled. A failure to take account of shifting county boundaries, for example, produces important errors in the detailing of population growth. Again, the misreading of shipping statistics underlies a totally inaccurate account of the distribution by town of Connecticut-registered vessels. Borrowing from the social sciences is done with unequal success. Daniels skillfully employs geographical techniques, integrating them with his data and providing a series of first-rate maps. In other cases, however, the meshing of evidence and contributing discipline is awkward and incomplete, models being jarringly appended to what is, essentially, traditional institutional history. The study suffers from a further lack of integration in its concentration on individual structures of town life without adequately relating them to the whole. To pick a superficial illustration, the decline in the number of town meetings, especially noticeable between 1721 and 1765 and here accorded major importance, obviously is related to the appearance of proprietors' and society meetings, their business having formerly been transacted in the town forum. Yet no connection is drawn. Finally, Daniels is handicapped by the uneven quality and paucity of specialized studies relating to Connecticut's towns. Reliance on this work causes him to emphasize a nonexistent shift from an economy geared primarily to agricultural production for local consumption to one heavily oriented to export markets. And the literature's lacunae account for a picture of the maldistribution of wealth in urban centers that depends far more on Hecretta's and

Kulikoff's studies of Boston than on any close look at the towns in question. Such problems suggest that Daniels's attempt to synthesize is premature.

BRUCE E. STEINER
Ohio University

EDWIN S. GAUSTAD. *George Berkeley in America*. New Haven: Yale University Press. 1979. Pp. xi, 225. \$15.00.

That George Berkeley, one of the most brilliant of British philosophers and ultimately a bishop in the Anglican church, a friend of Addison, Swift, Pope, and Steele, should have spent nearly three years of his mature life in Rhode Island is perhaps the most amazing improbability of our cultural history of the early eighteenth century. The rudimentary facts of Berkeley's mission here—and only a religious or moral mission could have brought him—have long been known, but with the publication of this useful book by Edwin S. Gaustad, who is an authority on colonial religion, we have the whole odd story for the first time pulled together in one place and clearly explained.

Berkeley arrived in Rhode Island in January 1729 with the whiggish intention of rescuing Britain and the empire from corruption and decay by establishing in the New World a seminary of religion and virtue, specifically an Anglican college from which would pour forth men of good morals and learning. He also was possessed of the dream already a century or two old of using such an institution to train Indian converts who would themselves become missionaries to their kindred. The philosopher's original site for this fountain of world redemption was Bermuda, but, as Gaustad shows, in the face of criticism and even ridicule, Berkeley had probably already decided on a mainland location before he sailed from England.

The execution of the plan was contingent on the allocation of £20,000 from Parliament, which Berkeley, not unrealistically, felt assured of getting. He already had a royal charter. The whole scheme collapsed, however, when Parliament failed to deliver on its promise, one cause being objections raised in Virginia, where William Byrd II, James Blair, and the royal governor, William Gooch, all wrote home opposing the scheme. Ironically, they learned of Berkeley's plans when the ship that brought him over to Rhode Island was forced to land first, after four months at sea, in Virginia in order to reprovision. Berkeley then spent ten days in Williamsburg. It remains a mystery why he had not years earlier recognized the College of William and Mary as an already existing Anglican foundation in America and instead tied his hopes to the Bermuda

location. He may have felt that only an entirely new institution, developed under his direct guidance, would suffice.

Yet despite the total failure of the college idea, the colonies benefited incalculably from the presence of the Irish universal genius. The stimulus Berkeley gave to American philosophy through Samuel Johnson, who later became the first president of King's College (Columbia), is well known. But Berkeley also endowed the first graduate fellowships in America (at Yale), gave unparalleled gifts of books to both Harvard and Yale, purchased a pipe organ for his church in Newport (one of the first of these instruments in the English colonies), and brought with him in his coterie of future "faculty" the painter John Smibert, usually considered the best portraitist to come to America in the eighteenth century (Smibert settled permanently in Boston and influenced Copley and others). In sum, everywhere Berkeley turned he was enriching, not least when he preached regularly on Sundays and functioned diversely as a churchman. As dean of Derry, Berkeley was the highest ranking Anglican to set foot in the American colonies, and Gaustad rightly emphasizes that those services to America that Berkeley himself most valued were spiritual.

NORMAN FIERING

Institute of Early American History and Culture

JEROME H. WOOD, JR. *Conestoga Crossroads: Lancaster, Pennsylvania, 1730-1790*. Harrisburg: Pennsylvania Historical and Museum Commission. 1979. Pp. xi, 305. \$8.50.

Conestoga Crossroads recounts social, political, and economic developments in Lancaster, Pennsylvania, between 1730 and 1790. In this pleasantly written and handsome volume, Jerome H. Wood, Jr., offers a comprehensive survey of life in this largest inland town in British North America. Following a brief description of the town's origins, the volume divides into three topical parts covering the years 1742 to 1790. The first part portrays the town's political history; the second, economic structure and the vital role of inland commerce; the third, society and culture, with special emphasis on religious life. Wood's book is traditional history in the best sense of that maligned term; he has culled the documentary record, and his account is spiced by apt quotations; yet the author also includes some of the newer counting techniques in his examination of social and economic structure.

Colonial historians will find Wood's volume useful as an urban complement to James T. Lemon's regional history of southeastern Pennsylvania, *The Best Poor Man's Country* (1972). Wood necessarily pursues many of the same themes developed by

Lemon—multi-ethnicity and cultural pluralism, privatism in the economic sphere, accelerating social and economic stratification, denominationalism in religion, and limited political involvement despite the existence of democratic institutions. Wood has the advantage over Lemon on the matter of scale as his study of a single town permits him to probe more deeply. The inland trade, for instance, is carefully described, and his account of this engine of growth is nicely punctuated by abundant quotations drawn from Lancaster and Philadelphia merchants. The reader might have been given a bit more quantitative evidence on the trade areas carved out by Lancaster, particularly since the author implies that Lancaster eventually lost its grasp on the inland wholesaling trade. Indeed, some of the author's own tables suggest that the town's growth slowed somewhat in the quarter century before the Revolution, but these data are inadequately tied to shifting trade patterns. Equally revealing are the chapters on ethnicity and religious life. Clearly this was a diverse town, with a majority of Germans and an abundance of English, and these nationalities were in turn divided among a variety of religious denominations. Wood's engaging discussion of the Great Awakening in Lancaster exposes the tense doctrinal differences that served to weaken community and, at the same time, to strengthen privatism.

One comes away from this volume having learned a great deal but with the worry that somehow the expertly crafted essays on Lancaster's politics, economy, and society fit together uneasily. Lancaster has been described as a volatile mixture of privatism, denominationalism, cultural pluralism, political apathy, and social stratification—and we must wonder at the remarkable and largely unknown forces of order that kept the town from exploding. Had Wood taken us this last step, his volume would qualify in my judgment as a great book instead of the very good one that it is.

CARVILLE EARLE

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W. ROBERT HIGGINS, editor. *The Revolutionary War in the South: Power, Conflict, and Leadership. Essays in Honor of John Richard Alden*. Durham, N.C.: Duke University Press. 1979. Pp. xxii, 291. \$17.75.

Between 1956 and 1971 the noted military historian John Richard Alden guided nearly a score of students through the doctoral program at Duke University. *The Revolutionary War in the South* is a tribute by eleven of those students to Alden's skill and dedication as a teacher and as a scholar.

In form and subject matter, this book is an ap-

propriate tribute. Following the editor's brief preface explaining the genesis of the work, W. W. Abbott skillfully surveys and evaluates the corpus of Alden's scholarship. The collection begins with four essays on "The Context of the Revolution in the South"; by Jack P. Greene, who offers a study of three leaders of Revolutionary Virginia, Richard Henry Lee, Edmund Pendleton, and George Mason; by W. Robert Higgins, who discusses the impact of the Revolution on the South's slaves; by James H. O'Donnell, on the impact of the Revolution on Southern Indians; and by R. Don Higginbotham, on James Iredell and North Carolina politics. The second section of the book deals with "The Role of the Individual" and consists of essays by John C. Cavanagh on Benjamin Lincoln, and Paul David Nelson on Horatio Gates, both focusing on their Southern campaigns, and pieces by Louis W. Potts on Arthur Lee as a diplomat, and Richard J. Hargrove on John Laurens. The collection closes with three pieces on "The Framework of Military Conflict in the South" by Ira D. Gruber on Britain's Southern strategy; Clyde R. Ferguson on the partisan-militia in South Carolina; and E. don Jones on the withdrawal of British forces from the South at the end of the war.

If this recitation of the collection's contents seems rather flat, it should at least give the reader an accurate foretaste of the intellectual stimulation to be derived from most of these essays. Only two of the eleven pieces are of real quality. Jack Greene's portrait of three Virginia leaders, although prefaced by an unnecessarily complicated analytical apparatus, is a sensitive and penetrating study. And it makes at least one arresting point: George Mason was uniquely effective as a constitution-maker in Revolutionary Virginia in part because he truly manifested the patrician disdain for political life that other gentry leaders thought they ought to feel. Thus when Mason came out of retirement in response to a real political crisis, his countrymen were deeply impressed by his sacrifice. The second important piece is Clyde Ferguson's creative integration of old and new interpretations of the military history of the Revolution as espoused, for example, by John Richard Alden and John Shy. Ferguson argues persuasively that South Carolina's partisan guerrilla forces were often militia, and a more seasoned militia than Shy and others have realized.

Several other essays merit brief comment. Higginbotham's reinterpretation of Revolutionary politics in North Carolina is sensible and perhaps important but not especially exciting. Pott's probing study of Arthur Lee, while probably less important, is interesting and well written. Hargrove's more speculative—and factually sloppier—psychobiographical portrait of John Laurens is less per-

suasive; Nelson's strained apology for Horatio Gates is less stimulating.

The remaining essays deserve little attention. Higgins shows refreshing candor, in a fairly traditional Southern historian of the South, by admitting that the Revolution did nothing for the South's slaves but he has nothing new to say. Cavanagh's defense of the ineffectual Benjamin Lincoln is muddled and tedious; Gruber's insipid analysis of Britain's Southern strategy is, coming from a prominent military historian, a keen disappointment. Finally, O'Donnell's bare summary of the wars between Southern whites and Indians and Jones's recitation of the logistics of the British withdrawal are simply pointless.

RICHARD ALAN RYERSON

Papers of William Penn,

Historical Society of Pennsylvania

JACK N. RAKOVE. *The Beginnings of National Politics: An Interpretive History of the Continental Congress.* New York: Alfred A. Knopf. 1979. Pp. xvii, 484. \$15.95.

This volume might best be described as an oddly conceived book, reflecting the author's latitudinarian attempt to accomplish too much without proper grounding in the vast array of primary source materials available to students of the Continental Congress and the Revolutionary era. Then, again, the problem may be that Jack N. Rakove could not make up his mind about what was the subject of his book. As a result, readers will inevitably become confused. Is this a history of the Continental Congress? Or the evolving doctrine of American federalism? Or the individuals (both strong and weak, important and secondary) who struggled to direct the war effort and the complex process of nation-making? The author undoubtedly would answer in the affirmative on all counts. Unfortunately, the integration of topics is so haphazard that only knowledgeable specialists will be able to make much sense or glean new findings from what may be characterized as a discursive, if not impenetrable, volume.

It would be appropriate to reflect on each of Rakove's concerns. In his history of the Congress, the author has sought to overcome what he repeatedly labels as the "simplistic" interpretations of Merrill Jensen, H. James Henderson, and E. James Ferguson. Rakove, it seems, has discovered that factions were not very paramount to explaining the decision making of Congress, at least until 1779. Terms like "radical" and "conservative" (except when Rakove employs them) are misleading, as are notions of voting blocs in Congress. It thus appears that men as different in temperament and thinking as Samuel Adams and John Dickinson were really just seeking

solutions to an unending list of vexing problems. In fact, it is possible to speculate that Rakove got himself trapped into analogizing from a modern corporate model of decision making: individuals taking up issues, debating the policy alternatives, accepting group consensus, and burying any record of formative disagreement for the good of the corporate image. This type of explanatory model simply is out of place in the turbulent Revolutionary political arena, where personal, local, regional, and sectional differences made decision making, especially in Congress, a nightmarish if not paralyzing task.

But Rakove refuses to admit that the paralysis of Congress, surely setting in by 1777, might have affected the formulation of policy (or its relative absence) and, even more important, questions about the distribution of powers between the states and central government. It is in this context that the author asks: Why did so many men avoid extensive service in Congress? His conclusion: service was not a matter of seeking power but of reluctantly accepting the burdens of republican self-sacrifice. (Another plausible answer might have been that patriot leaders sought power where it was, which was not in Congress.) Rakove's assertion of republican self-sacrifice on the part of the delegates is curious, moreover, since he claims that Revolutionary ideology had little impact on their actions or on the postwar debate over the concept of federalism (pressing issues of the moment were far more formative). Yet, why did ideology only affect behavior but not thought? If he had admitted both, then Rakove would have had a real dilemma, since he refuses to admit that very real differences over the fundamental issue of national authority—both in and out of Congress—reflected deep-seated ideological differences between localist and nationalist leaders.

A further problem has to do with Rakove's observations on the emergence of the doctrine of federalism. So much of the thrust in nationalist thinking arose from powerful minds in Washington's officer corps. Rakove, however, fails to explore that dimension, presumably because it was beyond Congress. Indeed, except for the nationalist attempt to infuse Congress with greater authority in the 1781–83 period, the author's presentation will do little to enlighten historians about the background of the nationalist upsurge that resulted in the Constitution of 1787. Rakove may have even further confused the issue by placing so much emphasis on James Madison.

This book does have some strong points. The discussion of the various drafts of the Articles of Confederation is fresh, even if it does not affect the interpretive substance of what already is well-known. His characterization of irascible Thomas Burke of North Carolina is revealing, adding to knowledge about that much-neglected personality. The role of

the Deane-Lee controversy represents the strongest material in the book, even if the author misses its full implications (such as its impact upon the creation of the nationalist bloc before 1781). On the other hand, the portrait of Robert Morris suggests selective sampling of the financier's correspondence. Few historians will be satisfied with a portrait of Morris as a man being tossed to-and-fro by others.

In its various aspects, this book leaves a great deal to be desired, except for those who have already decided that the interplay of factional alignments and political ideology had little impact on the experience of the Revolution—and its outcomes. At best, Rakove's work will supplement important studies by Jensen, Henderson, Ferguson, and Jackson Turner Main (on state politics). Despite dust jacket hyperbole, Rakove has not offered a fresh alternative for interpreting the Congress and the American Revolution.

JAMES KIRBY MARTIN
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STEVEN R. BOYD. *The Politics of Opposition: Antifederalists and the Acceptance of the Constitution*. (KTO Studies in American History.) Millwood, N.Y.: KTO Press. 1979. Pp. xiii, 178. \$18.95.

This slender volume contains a large body of information about the politics of the ratification of the Constitution. From the discussion of the document in Congress to the state legislative debates, the election of convention delegates, and the first national elections, the process is meticulously catalogued. Few errors mar the narration, several errors or misconceptions of previous chroniclers are corrected, and many bits of fresh data are supplied. The author does occasionally lapse into ill-considered generalizations. "Maryland, like the other middle states, was solid in its support for the Constitution" (p. 32); every state had a history of "bi-partisan" politics (p. 41); Stockbridge and Great Barrington, Massachusetts, were "urban centers" (p. 61); the efforts of Pennsylvania Antifederalists "led to" the Antifederalist victory in North Carolina and the "virtual draw" in Virginia (p. 95). Despite such slips, this is as concise and accurate a summary account as one is likely to encounter.

Unfortunately, Steven R. Boyd has also chosen to argue a thesis, and in that effort he is less successful. Since every attempt to amend the Articles of Confederation had failed, and Antifederalists constituted a majority in 1787–88, he asks, how does one explain the Federalists' triumph and the Antifederalists' acquiescence in their defeat? The usual explanation is that the Federalists were well organized and the Antifederalists were not. To the contrary, Boyd insists, the antis began organizing early

and had a vigorous campaign in operation throughout the fall of 1787 and the winter and spring of 1788. Their failure, rather, and the ultimate acceptance of the Constitution by all parties, derived from a tactical error: by agreeing to do battle on the issue "within the framework of the proposed Constitution," the Antifederalists unwittingly functioned as a loyal opposition and thus "contributed to the ultimate legitimization of the very Constitution they opposed" (p. 67).

The thesis is flawed in several respects. The antis were actually well organized only in Pennsylvania and New York, and they never managed to agree either on tactics or on basic aims. The absence of such agreement is the absence of "organization." More importantly, Boyd's question rests upon an unsound premise. Why did the Constitution meet with a different reception from that of proposals to amend the Articles? The answer is that it did not. Only one or two states, in each instance, had voted to reject the earlier amendments, and only two states voted to reject the Constitution. The difference was that the rules were changed: unanimous approval was required to amend the Articles, and only nine states were required to approve the Constitution.

In the end, then, the question is why did the Antifederalists go along with the changing of the rules? The alternatives Boyd suggests—forcible resistance or refusal to participate—were not options at all. The fact is that the antis were outwitted: superior intelligence, determination, and organization won out over numbers. For once, the good guys triumphed. As I have suggested elsewhere, that was in the order of the miraculous.

FORREST MCDONALD
University of Alabama

WHITMAN H. RIDGWAY. *Community Leadership in Maryland, 1790–1840: A Comparative Analysis of Power in Society*. Chapel Hill: University of North Carolina Press. 1979. Pp. xxi, 414. \$19.50.

The expansion of the suffrage and the introduction of the spoils system have led most historians to conclude that the American political process was more democratic by the 1840s than it had been fifty years earlier. Recently, however, as part of his attack on the "egalitarian myth," Edward Pessen has disputed the real importance of these changes for the way that power was wielded in antebellum society. The distribution of wealth was becoming more lopsided and so seemingly was the distribution of power. In *Community Leadership in Maryland*, Whitman H. Ridgway presents a modified version of the traditional view and concludes, in direct contrast to Pessen that, "the structure of authority in Mary-

land's political institutions conformed to Tocqueville's general description of democratic government. The ultimate power behind government rested with the people, while government responsibility was divided through various divisions and balances" (p. 189).

Arguing that crucial questions about the "evolution of American democracy" must be examined at the community level, Ridgway focuses upon the local context of four economically diverse counties: Baltimore City; rural, but socially heterogeneous, Frederick County in the central part of the state; slave-holding and tobacco-growing St. Mary's County south of Baltimore; and Talbot County on the Eastern Shore, primarily a grain-producing area. Ridgway analyzes changes in the nature of the decisional elite in each community between the first and second party eras and, by comparing the patterns of change in the different types of counties, asserts his basic thesis that growing economic and social diversity undermined oligarchic control of local communities during these years. While agreeing with the traditional view, he pictures the process as more gradual and complicated than have previous historians. Wealthy and prominent men continued to dominate the power structure well into the second party era, and change proceeded at different rates in different areas as a "function of a community's underlying social and economic heterogeneity."

Ridgway has struggled to specify his concepts and relate his study to relevant work in the social sciences. He challenges Pessen's use of "democracy," "egalitarianism," and "elitism" and attempts to define each more precisely. Rather than simply examining the social position of office holders, Ridgway distinguishes between the decisional elite (those participating in and significantly influencing key community decisions) and the commercial, propertied, and positional elites ("men with a certain resource"), and he carefully analyses the ways these groups overlapped. In this, as in his emphasis on the relationship between pluralism and democracy in America, Ridgway follows the general position set out by Robert Dahl in *Who Governs?*

Although Ridgway makes a plausible case for his main thesis, it seems unlikely that reading this book will convince those who have continued to resist Dahl's original formulation of the pluralist model. The author compounds this skepticism by failing to operationalize key concepts once he defines them and by introducing in his conclusion a number of slippery new concepts with little attempt at clear definition. Yet, given these problems, Ridgway has written a readable and quietly intelligent book that presents the most reasonable discussion available of the democratization of community leadership during these years. It is thoroughly researched and de-

serves to be read by every serious student of American political development.

WILLIAM G. SHADE
Lehigh University

JOHN L. HAMMOND. *The Politics of Benevolence: Revival Religion and American Voting Behavior*. (Modern Sociology Series.) Norwood, N.J.: Ablex Publishing. 1979. Pp. xi, 243. \$16.50.

Increased understanding of the influence of religious values on politics has benefited political history. This book, whose useful points might have found adequate exposure in an article or two, shows that a good thing can be carried too far. John L. Hammond argues that the revivals of the 1820s and 1830s initiated by Charles G. Finney created a "revivalist political ethos" in New York and Ohio leading certain communities to support abolition, antislavery, temperance, and other moral causes. This notion is not new, but Hammond pushes it to extremes both in making sweeping claims about its impact before the Civil War and in extending his thesis into the twentieth century down to the 1960s. The writing makes the book difficult reading, but more importantly Hammond did not do the primary research needed to support his characterization of the revivals.

Hammond's key chapter, "The Revival Movement," relies on secondary sources and Finney's published works. The author simply assumes that the content of thousands of sermons preached for over a decade in hundreds of churches throughout New York and Ohio closely resembled Finney's preaching. He also assumes that revivalism led communities uniformly toward moral reform. These assumptions are not warranted either by the standard historical literature or by recent inquiries into revivals by younger scholars such as Paul Johnson and Linda Pritchard. Further, researchers at the Center for the Study of Early Religious Life at Ithaca College have found that revivals functioned often to integrate communities rather than raise divisive issues.

Hammond measured revivalism from 1825 to 1835 primarily by counting the revivals in each county reported in ten religious periodicals. His methods made for an encounter with only the tip of an iceberg. His honest admission, in an appendix, that "the most important variable could be measured with only moderate precision" (p. 216) is an understatement. Now moderate precision in a historian is no sin, but Hammond did not conduct sufficient research in local sources to get some sense of the enormously diverse and complex religious life of the period.

Hammond emphasizes most the causal link be-

tween revivalism and "political abolitionism," asserting that "those who experienced revivals also voted abolitionist" (p. 69). To make this claim, Hammond must bury a central fact that has emerged from recent writings on nineteenth-century politics and religion, namely that very few of the great mass who participated in revivals went on to become abolitionist or vote for the Liberty Party.

The backbone of the book is statistical analysis of revival measures with other county-level political and demographic data, not only for the years from 1830 to 1850 but also for 1856, 1860, 1870, and beyond. One might have thought 1850 the outer limit for taking data of uncertain reliability from 1825 to 1835 and stuffing it into a multiple regression formula. But Hammond presents tables combining data from 1825-35 down to 1940, and even generalizes about "revivalist areas" in New York in the 1960s. The further in time he goes, of course, the less his data have to do with the populations discussed. One does not need to be a statistician to realize that at some point the limits of absurdity are reached and crossed. The analysis of county-level data can be moderately useful, but should have constituted the *beginning* of description rooted in historical context, not, as in this case, the *end* of description, and much less of explanation.

RONALD P. FORMISANO
Clark University

RUSSELL E. MILLER. *The Larger Hope: The First Century of the Universalist Church in America, 1770-1870*. Boston: Unitarian Universalist Association. 1979. Pp. xxviii, 1,009. \$25.00.

The Larger Hope, by Russell E. Miller of Tufts University, is a massive, largely institutional study of the first century of the Universalist Church in America. The author, not himself a member of the denomination about which he writes (it merged with Unitarianism in 1961), has pored over voluminous records, minutes, periodical files, biographies, and other primary and secondary literature dealing with Universalism. The work is organized topically in 8 sections and 29 chapters, with 99 pages of backnotes, 49 illustrations, a selected bibliography, and an index. Miller has told much of his story through attention to lay and clerical leaders who rose to prominence; their names stud these pages.

The Universalist Church—not so named formally until 1870—traced its origins to the arrival of Englishman John Murray (1741-1815) in America in 1770. Its first church was organized nine years later, and what became the Universalist General Convention was formed in 1793. Thereafter, the denomination grew swiftly; though reliable statistics are lacking, it was probably fifth or sixth largest in

the country in the 1830s and was at the peak of its national influence (though declining relatively in numerical rank) in the 1840s and 1850s. Universalism appealed to many Americans fleeing from strict Calvinism, for its theological basis stressed the love of God and the salvation of all (it was turned in a unitarian direction by its most prominent theologian, Hosea Ballou [1771–1852]). The church also suited the needs of a culture in which individualistic and localist trends were strong: “Universalism as a separate denomination developed in a casual, uncoordinated, and almost accidental way, reflecting the individualism and suspicion of centralized organization shared by most Universalists who felt sufficiently strong in their religious convictions to join a distinctive group” (p. 56). Its relationship to Unitarianism was strained during the first half of the last century; the theological emphases of these two liberal bodies were not identical (Universalism saw itself theologically between Unitarianism and the Evangelical churches), and Universalism was more a church of the people, while Unitarianism was strong among the upper classes. But—and here Miller challenges a long-standing generalization—Universalism was not primarily a rural movement in its early years. It was “confined at first largely to urban centers, and did not spread to the interior until after 1820 as part of general westward migration” (p. 161).

Many other facets of the institutional, educational, publication, and reform history of Universalism are traced in leisurely detail. For example, the church played a pioneering role in the ministry of women: “There seems to have been an unusually large number of individuals, both men and women, who became champions of the rights of women and/or made significant social contributions” (p. 535). Clara Barton and Mary A. Livermore became widely known, while Olympia Brown (1835–1926) was the first woman to receive fully recognized ordination in America (1863). Stronger on institutional than on theological history, this book makes an important contribution to liberal denominational history.

ROBERT T. HANDY
*Union Theological Seminary,
 New York*

MASAO MIYOSHI. *As We Saw Them: The First Japanese Embassy to the United States (1860)*. Berkeley and Los Angeles: University of California Press, for the Center for Japanese and Korean Studies, University of California, Berkeley. 1979. Pp. xi, 232. \$14.95.

In 1860, the Edo Bakufu dispatched a group of over one hundred seventy men to the United States for exchange of the ratifications of the American-

Japanese Treaty of 1858. In this monograph, Masao Miyoshi, professor of English at Berkeley, narrates the engaging activities of these men who constituted the first Japanese embassy to the United States, analyzes their “minds,” and concludes that “America and Japan . . . lie as far apart now as they did a century ago.”

From the standpoint of historiographical analysis, the contents of the monograph may be divided into three parts: (1) well-known events of the Bakumatsu and Meiji eras that are skillfully presented to provide appropriate historical contexts in which the activities of the embassy and subsequent incidents may meaningfully be described, analyzed, or appreciated; (2) a reconstruction of the men’s perceptions of America and Japan; and (3) an account of the lives of the men after their return from the trip.

Of the three parts, the second is the most valuable. Here one finds a richly detailed and exquisitely sensitive treatment of several timeless patterns of Japanese behavior and thought: the concept of meticulously formulated hierarchy, the Japanese group orientation, the lack of self-assertion in the diaries of the embassy, and the uniformity in the content and style of Japanese writing. With great sensitivity and understanding as well as detachment, the author has elucidated these patterns as underlying the Japanese perceptions of America. It is rather sobering to realize that in general these patterns still help shape the Japanese image of the West.

Those portions of the book, especially the first chapter, that make up the first of the three parts are lucidly written but contain a number of assertions that today’s historians would have difficulty accepting (pp. 13, 15, 16, 17, 21, 22, and 101). As examples, two will be cited. First, the definitions of *judai* and *tozama* daimyo as given in this book are like those given by the late James Murdoch (pp. 15f). Second, the author asserts that the *sankin kōtai* system required the daimyo to reside in Edo “every half-year” (p. 101). It is well known that a small number of daimyo did so, but the bulk of them resided there every other year.

The last of the three parts, taking up as it does 42 of the total 186 pages of the text, adds little to the content of the book. To begin with, the biographical sketches of many lives are too short to be meaningful, while the fairly lengthy accounts of two lives, Katsu Kaishū and Fukuzawa Yukichi, just present well-known facts. Beyond this, by his own admission, the author merely has presented “a pastiche of some of the careers . . . ignoring the explicit cause-effect readings of the voyage and the later lives” (p. 154).

The monograph is well worth reading on account of the second part alone. In both style and content the book has great appeal to scholars and laymen

alike. It could have easily been brought out by such publishers as Kōdansha as well as by any university press.

RICHARD T. CHANG
University of Florida

DAVID J. WISHART. *The Fur Trade of the American West, 1807-1840: A Geographical Synthesis*. Lincoln: University of Nebraska Press. 1979. Pp. 237. \$15.00.

In the considerable body of literature that exists on the American fur trade of the trans-Missouri West, the traditional focus has tended to be romantic and personalistic. Emphasis on the heroic confrontation between nineteenth-century man and the natural environment has dominated this literature; there has been confusion between real and apocryphal, personality has overshadowed place, and the real or perceived brilliance of individual exploits have hidden the system within which these individuals operated. There are notable exceptions to these criticisms of the genre in the work of L. R. Hafen, H. M. Chittenden, P. C. Phillips, and J. E. Sunder. To this list must now be added the name of David J. Wishart whose *Fur Trade of the American West, 1807-1840: A Geographical Synthesis* provides a new interpretation of the fur trade, viewing it as a system operating in space and time. In this most interesting work, written by a historical geographer, the guiding ideas were what D. W. Meinig, also a historical geographer, has referred to as "strategy and ecology—man's organization of area and man's relationships with the other parts of the ecosystem."

Wishart prefaces his study with an analysis of the geographical setting of the fur trade, a setting that consists of the geostrategical context of the trans-Missouri region as well as the physical and biological characteristics of the landscape. Then, following the guiding principles of his work, Wishart analyzes the "strategy" of the Upper Missouri fur trade in bison robes in terms of human organization of the area of the Upper Missouri basin and discusses the "ecology" of the trade by describing the relationships between the traders and the other elements in the ecosystem (including native Americans) in terms of the annual cycle of operations. Similar discussions of strategy and ecology are implemented for the more familiar Rocky Mountain fur trade devoted largely to beaver pelts. The author then concludes his analysis by assessing, in ecological terms, both the Upper Missouri and Rocky Mountain components of the system as parts of a greater whole, the totality of the trans-Missouri fur trade. The result of this assessment is a decidedly anti-romantic image of the fur trade as ecologically destructive to the physical environment and the native inhabitants of the area alike.

Like many innovative works, the book suffers from occasional inconclusiveness. But the research work has been done carefully and Wishart's conclusions on the fur trade as "an early stage in the progressive dissipation of the American environment" are certainly thought provoking in light of the emerging interest in environmental history. But whether it is better (as Wishart claims) to view the fur trade through the "dark lens" of environmental destruction or through the "rosy lens of frontier romanticism" remains moot. Perhaps viewing through any single lens exclusively is dangerous, and, although Wishart's bias does not seem to be an illogical or unsubstantiated one, those engaged in the fur trade were figures of romance in a romantic period. To ignore that fact altogether in favor of a completely "systematic" argument does not seem totally appropriate.

JOHN L. ALLEN
University of Connecticut,
Storrs

MARTHA ROYCE BLAINE. *The Ioway Indians*. (Civilization of the American Indian Series, number 151.) Norman: University of Oklahoma Press. 1979. Pp. xiv, 364. \$24.95.

Martha Royce Blaine's work is an ambitious ethnohistory of the Ioways. She traces the tribe chronologically from earliest prehistory in northeastern Iowa to final allotment in Oklahoma during the last quarter of the nineteenth century. Between those times, Blaine states, the Ioways were a force in the region during the seventeenth and eighteenth centuries. Information detailing the tribe's activity during this period is sparse and vague, despite apparently frequent contacts with French, Spanish, and English fur traders. The crossroads location of the Ioways' territory between the Missouri and Mississippi Rivers not only affected their role in the fur trade but also brought them into sustained contact with other tribes inhabiting the region. Eventually that same pivotal position led to intercourse with the Americans, particularly after 1800. A predictable course of events—loss of territory and tribal sovereignty, economic dependence, and cultural devastation—followed. Blaine contends that the Ioways exerted a regional influence far greater than their modest numbers would suggest. The tribe exercised this influence due primarily to the fortuitous location of their homeland—a location that fostered a significant measure of "middleman" activity in the fur trade, thus ensuring the tribe's position as a regional force well into the nineteenth century.

Blaine's analysis, however, is not completely supported by the evidence she presents. The tribe's im-

portance during maneuverings by the European colonial powers is outlined, although the Ioways appear only tangentially important in regional affairs until the American influence began to be felt around 1800. The sketchy nature of the information known about the Ioways during the pre-1800 period is stretched precariously thin to establish the tribe's importance during this period. As such, it seems an inadequate basis upon which to draw such significant conclusions. Moreover, it is questionable whether the Ioways in the post-1800 period, fragmented as they were into at least two opposing factions whose total population never exceeded 1,500 and buffeted by a host of other Indian tribes, would have been in any position to exert a dominating influence in the region or the fur trade.

The main contribution of Blaine's work rests with the straightforward, factual ethnohistory she presents of the Ioways. Drawing upon a wide variety of primary and secondary sources, Blaine has brought this material together into a useful narrative. In several instances, however, the narrative is fragmented and plodding due to the scarcity of source material or the inclusion of irrelevant information that is a distraction to the reader. Nevertheless, this work will be a useful addition to the study of Native American ethnohistory.

RONALD RAYMAN
Western Illinois University

DANIEL F. LITTLEFIELD, JR. *Africans and Creeks: From the Colonial Period to the Civil War*. (Contributions in Afro-American and African Studies, number 47.) Westport, Conn.: Greenwood Press. 1979. Pp. xiii, 286. \$22.50.

It is impossible to understand the history of the Five Civilized Tribes without attention to the black men, as slaves or as freedmen, in their midst. In recent years, thanks to the studies of Daniel F. Littlefield, Jr., and others, that story is becoming better known. Littlefield began his work with *Africans and Seminoles: From Removal to Emancipation* (1977), continued it with *The Cherokee Freedmen: From Emancipation to American Citizenship* (1978), and now in the present work considers the presence of blacks among the Creek Indians.

The Creeks, like others of the southern tribes, had no well-developed slavery system of their own, although there were occasions when other Indians were enslaved. But as their contacts with whites multiplied and as increasing numbers of black slaves fled into the Creek Nation, the Creeks adopted the white practice of black slavery. From the Revolutionary War on, blacks became an important element in Creek history. They affected the economic development of the Creeks, for many

Creeks (especially mixed bloods) adopted a plantation economy, and they seriously complicated the relations of the Creek Nation with the whites and with neighboring tribes. Although Littlefield supplies a detailed section on the nature of Creek slavery and the slave codes that enforced it, he is more concerned with recounting the presence of the blacks in the general unfolding of Creek history.

Creek relations with blacks were intertwined with Creek-Seminole relations, for slaves of the Creeks as well as of white planters fled to Seminole country, where their bondage was generally light. Many blacks among the Seminoles led almost independent lives and some, because of their knowledge of English, served as interpreters and often had great influence on Seminole affairs. When the Creeks and Seminoles were closely associated in Indian Territory in the West, the problem of the free or nearly free blacks among the Seminoles caused innumerable conflicts, in which officers of the United States government often intervened. The book makes constant reference to the Seminoles, and substantial sections are as much about them as about the Creeks. In fact, many pages repeat almost verbatim long passages from *Africans and Seminoles*.

The story told is an important one, and the book reflects extensive research in archival documents. The presentation, however, is faulty, for the book seems to have been written in haste without adequate digestion of the sources. There is a great deal of anecdotal reporting—great detail of names and places and minor events that illustrate but do not advance the action—and an annoying technique of supplying long lists of names of slaves both in the text and in the notes. The reader who can ignore such irritations, however, will begin to get an appreciation of how significantly blacks fitted into Creek and Seminole history in the decades before the Civil War.

FRANCIS PAUL PRUCHA
Marquette University

RONALD L. LEWIS. *Coal, Iron, and Slaves: Industrial Slavery in Maryland and Virginia, 1715-1865*. (Contributions in Labor History, number 6.) Westport, Conn.: Greenwood Press. 1979. Pp. 283. \$22.95.

Joining the growing rivulet of scholarship directed in recent years toward the industrial employment of slaves in the plantation economy, this concise, detailed work, a revised and updated dissertation, demonstrates the worth of such investigations in comprehensively understanding the peculiar institution's operation in the Old South. This study focuses upon the labor employed from the colonial period through the Civil War in the eastern Virginia bituminous coal field and in the various iron-

works in the Baltimore, Maryland, vicinity, Richmond, and the upper Valley of Virginia. At various times, approximately 4,500 to 7,000 bondsmen labored at the numerous Maryland-Virginia ironworks and between 500 to 1,000 toiled in the cyclical Virginia coal industry.

Recognizing the slaves' substantial influence on whites to adjust labor policy, the author finds the relationships between slaves and masters in the ironworks and coal mines to be based on forced compromise rather than on absolute authority. What emerged was a three-way relationship: (1) the slaves pushed for advantages within certain parameters; (2) the employers yielded without losing ultimate control; and, (3) the slaveowners attempted to protect and profit from their property. These industrial employers of slaves pursued a policy of compromise towards bondsmen, not because of humanitarian considerations but for more pragmatic reasons. The owners and managers used negative and positive incentives to motivate their work force. Negative incentives included verbal chastisement, placement in more difficult tasks, whipping, and threats to sell, whereas positive stimulation arose from granting of free time, manipulation of food and clothing allotments, monetary remuneration, and the overwork system. Despite all motivational techniques, many slaves in both industries never accepted their plight and combated it by excessive drinking, theft, escape, and temporary disappearance.

By suggesting that profitability in the coal and iron industries might be more dependent upon managerial considerations than upon the labor system, Lewis deftly avoids the final answer to the question because of the lack of data. Other factors such as marketing, location, adaptation to technological innovation, transportation costs, and the risky nature of coal mining played more deterministic roles in ultimate success of the enterprises. It is certain that the entrepreneurs concerned preferred slave labor and thought it to be superior to white labor. Throughout the period, these iron masters and coal operators, like their plantation brethren, opposed a social, political, and economic system that benefited industrialization. Although temporary differences arose over the issues of tariff and industrial self-reliance, they, being social conservatives, preferred the plantation society based upon a predictable and stable labor system and upon a common understanding of communal responsibility in continuance of a slave system.

The major strength of this study is its employment of thorough research to document the day-to-day workings of industrial slavery and the hire-system in major Old South enterprises without resorting to questionable theorizing and dubious generalizations. Lewis reaches temperate conclusions,

buttresses his presentation with numerous tables, realistically employs his evidence in relating the contemporary operational environment of these dynamic industries, and successfully marries his findings with the most recent scholarship in the field. More than a labor study, the volume traces the rise of the respective enterprises, accounts for their importance, and places their development into a national economic context.

JOHN E. STEALEY III
Shepherd College

MARCUS CUNLIFFE. *Chattel Slavery and Wage Slavery: The Anglo-American Context, 1830-1860*. (Mercer University Lamar Memorial Lectures, number 22.) Athens: University of Georgia Press. 1979. Pp. xix, 128. \$9.00.

Marcus Cunliffe delivered the three lectures that compose this book as the Lamar Memorial Lectures at Mercer University in 1978. Since the lectures' purpose is to explain and to preserve Southern life and culture, the selection of Cunliffe, the first non-American and nonspecialist in Southern history, seems, at first glance, surprising.

Cunliffe justifies the faith of those selecting him, however, choosing to lecture upon the history of a controversy over the relative merits of wage slavery in England when compared with chattel slavery in the United States. Beginning with George Fitzhugh, whom he calls the pioneer in the comparison of the two institutions and describes as "wiser than either the abolitionists or the complete defenders of the status quo," Cunliffe ends with a discussion of the ideas of Charles Edward Lester, an obscure Northern clergyman-abolitionist who migrated to England in 1840, as primarily expressed in *The Glory and Shame of England*. The book, published in 1841, reflected Lester's experiences in the North and in England and commented caustically upon wage slavery in England. In the intervening lecture, Cunliffe describes the mutual criticism aimed at each other's societies by intellectuals on either side of the Atlantic. Americans, convinced that English social organization exploited the workers, believed the English criticized America's peculiar institution in order to distract attention from that organization and to "undermine" the American Union. The English, on the other hand, regarded American society as vulgar, corrupt, dishonest, and, most particularly, hypocritical in its profession of freedom while practicing slavery.

The exposition is a graceful one displaying a certain familiarity with what Cunliffe admits must be considered a minor strain in Anglo-American thought. Although he asserts that the attacks on wage slavery made by Lester and Fitzhugh repre-

sented opinions more commonly held than has previously been supposed, he fails to substantiate fully that assertion in this microcosmic study. Nor does his conclusion that our generation has accepted the view that chattel slavery was "more economically viable and more robustly absorbed by its black force" than its opponents realized seem justified when the reaction to that view has been as heated as it has.

While containing little new for students of slavery, the book does show how well Cunliffe met the purpose of the series, to provide lectures of the highest quality to the public.

DWIGHT HOOVER
Ball State University

DICKSON D. BRUCE, JR. *Violence and Culture in the Antebellum South*. Austin: University of Texas Press. 1979. Pp. x, 322. \$16.95.

In the antebellum South, "it was remarkable, and worrisome, how many gentlemen were prepared at all times to turn a minor altercation into a killing" (p. 72). Within the pre-Civil War South there was overwhelming acceptance of violence as being unavoidable. Yet, Southerners strongly cherished order; for them discipline was fundamentally important, so much so that in their child-rearing practices they attempted to eliminate selfishness from their children.

Southerners viewed the world as one of violence, a theme Dickson D. Bruce, Jr., explores by focusing on the meaning of violence, its sources, and its implications. Following an excellent introduction, he devotes chapters to the Southern duel, child rearing, views of the planter-class toward violence, plain-folk society, violence from both the slaver's and slave's point of view, militarism, fiction, and a conclusion centered around the relevance of Edgar Allan Poe to a discussion on Southern violence. "Poe," Bruce writes, "expressed the logical outcome of many Southerners' world view in his tales" (p. 240), which portrayed human nature as perverse, often dominated by evil.

Bruce believes "the basis for Southern attitudes toward violence lay in pervasive patterns of culture that extend well back into Antebellum history" (p. 8). For Southerners the villain that gave rise to violence was passion. Viewing passion in negative and destructive terms, most Southerners distrusted human nature; at best, they could only hope to keep passion under control. With slavery in their midst, the concern Southerners had about the need to control violence was exacerbated. Given such an institution, built and maintained through violent force, fear of slave uprisings dominated their thought. The mere idea of abolition also evoked fear of vio-

lence: "abolitionists, or suspected abolitionists, were the most common targets of lynch law" (p. 130). "Violence," contended Frederick Douglass, "not morality, was the language of American race relations" (p. 159).

Craving order but convinced it was fickle and unlasting, Southerners viewed violence as the opposite of order. Paradoxically, while placing a premium on order, the most figurative language used by them was violent. The "source which provided the greatest stock of Southern orators' violent imagery was the American Revolution" (p. 184).

Leaving in place the general view that antebellum society was the most violent within the nation, Bruce theorizes that Southerners' fear of passion lay at the heart of the problem. Their obsession with the need to control passion interacted with their general pessimistic attitude to produce within them an overall violent temperament, one even conducive to civil war. Insecure within their racist and stratified sectionalism, with a national creed proclaiming freedom and equality, antebellum Southerners were frightened people. They saw or imagined threats and dangers everywhere, even within themselves. Bruce does not say it, but the legacy of that disposition haunts our own times.

E C FOSTER
Jackson State University

RAY MATHIS. *John Horry Dent: South Carolina Aristocrat on the Alabama Frontier*. University: University of Alabama Press, for the Historic Chattahoochee Commission. 1979. Pp. xiv, 267. \$17.50.

In 1837 when he was twenty-two years old, John Horry Dent left his economically depressed home state of South Carolina and established his young family in Barbour County on the Alabama frontier. Drawing on Dent's extensive collection of farm journals, diaries, financial records, and short essays, Ray Mathis has created a detailed and rich mosaic that illustrates his subject's rise as a prosperous planter and businessman. The result is a valuable microcosmic slice of Old South life that allows the testing of many of our large historical assumptions against individual experience.

The book's principal strength is also its main weakness. It is perhaps half-authored and half-edited, since Mathis quotes extensively from Dent's journals, allowing the subject most often to speak for himself. Although Dent was consistently detailed and competent, he was rarely eloquent; thus, the narrative sometimes takes on a tedious quality for the reader. Nonetheless, for the specialist willing to plow through these passages, there are glittering rewards: a full, clear account of the plantation year, the month-by-month agricultural operations in

which Dent engaged; a good exposition of this paternalist master's attitudes toward and treatment of both slaves and overseers; a compelling depiction of Dent's unflattering obsession with economic gain as a key to social status; and a thoughtful treatment of plantation adjustments with the advent of black emancipation.

Mathis's monograph deals only with Dent's life and occupation through the end of the Civil War, leaving for subsequent treatment his career as an agricultural reformer, farmer, and financier in north Georgia. Nor is the volume much concerned with local history or Alabama politics. Historians of the Old South, especially those interested in agriculture and economics, will find John Horry Dent an informative acquaintance.

HUGH B. HAMMETT
*Empire State College,
 State University of New York*

JOHN MCCARDELL. *The Idea of a Southern Nation: Southern Nationalists and Southern Nationalism, 1830-1860*. New York: W. W. Norton and Company. Pp. xi, 394. \$16.95.

This Allan Nevins Award winning work ambitiously seeks to explain the origins and development of the ideology of Southern nationalism. However, despite, or perhaps because of, its ambitious scope, John McCardell's study is disappointing in that it too often amounts to a bland synthesis of existing scholarship. What the reader searches for in vain are those "fresh conclusions about the nature of the secession movement" promised in the introduction (p. 8).

To be sure, the general reader and the undergraduate student in Southern history will find much of value in the narrative. In a series of topical and biographical chapters focusing on the period after 1830, McCardell touches base with that variety of strains—social, economic, and political—which comprise the familiar backdrop to the republic's intensifying sectionalism. Sound and generally insightful discussions, based on the latest scholarship and often on the imaginative use of manuscript sources, are presented in such areas as proslavery thought, literature, and expansionism. Still, in common with most works of synthesis, the text lacks depth and nuance. At times the very breadth of the material that McCardell feels compelled to cover results in superficiality, as in his treatment of late antebellum Southern religion that glosses over both the ethos of evangelicalism and its social appeal; in misleading generalizations, as evidenced in his conclusion that the "education of young children was hardly neglected" (p. 179); or in a rushed narrative, such as the final chapter on postnullification Southern and national politics.

Had McCardell adhered more closely to the conceptual formulations that he laid out in his introduction, *The Idea of a Southern Nation*, in my judgment, would have been a more successful work. For example, borrowing from Clifford Geertz, McCardell suggests that ideology be approached as an effort to cope with stress in the social structure and cultural belief system. Although the ensuing narrative, especially in its biographical portraits, often locates individual stresses and relates them to a personal commitment to Southern nationalism, McCardell stops short of linking tensions of individual personalities to Southern culture or society as a whole. Indeed, just when such a linkage appears about to be formed, McCardell moves on to another topic or retreats into irony. Thus, at the end of a fine chapter on nullification, he finds "ironic" that the most radical nullifiers, those committed to a Southern nation outside the Union rather than a protected Southern section within the Union, were "entrepreneurial, parvenu planters" (p. 48) who perceived the North as a threatening society of venal, money-grubbing shopkeepers. The irony dissolves when we consider that the emerging Southern nationalists, and putatively Southern culture as a whole, when confronted with unavoidably contradictory expectations of identity—gentility versus aggressiveness, chivalry versus materialism, order versus competitiveness—displaced onto the mythical Yankees those values and personality traits that they so hated in themselves and then in turn used that displacement as a justification for separating from the North.

Though one is left unsure as to the extent of Southern nationalism, even by 1860, I would recommend McCardell's study as a useful survey of the background to secession. As a significant and original contribution to our understanding of that movement, the book falls short.

WILLIAM L. BARNEY
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 Chapel Hill*

WALTER EHRLICH. *They Have No Rights: Dred Scott's Struggle for Freedom*. (Contributions in Legal Studies, number 9.) Westport, Conn.: Greenwood Press. 1979. Pp. xvi, 266. \$22.50.

Walter Ehrlich's *They Have No Rights* will doubtless be referred to as "the other" modern treatment of the *Dred Scott* case. Clearly overshadowed by Don Fehrenbacher's deep and wide-ranging analysis of the same general subject, this narrower study develops narrative legal history almost exclusively. Ehrlich, unlike Fehrenbacher, offers no sustained analysis of antebellum racial feeling, political ideologies, and social stresses or their impact upon the thinking of lawyers and judges. He also does not consider the

complicated consequences of the decision itself in the final years of the sectional conflict, from 1857 through 1861, or its broad impact on race and law in the post-Civil War era. Instead, Ehrlich pursues the more restricted but nevertheless worthwhile task of tracing Dred Scott's lawsuits from the mid-1840s onward in order to demonstrate how they evolved from straightforward pleas for an individual's emancipation into devices by which jurists and politicians addressed the deepening crisis over slavery's expansion. To this end, Ehrlich has carefully examined legal briefs, judicial pronouncements, and press accounts in order to set forth in his words "the detailed morphology of the case itself" (p. xiii). Historians of law and of the sectional conflict now possess a reliable, modern, and even absorbing brief account of how Dred Scott's quest for freedom slowly became a political *cause célèbre*.

Ehrlich's approach demands that he concentrate heavily on the early phases of Scott's life and litigation. Nearly half the book involves detailed examinations of Scott's situations and travels in the 1830s and 1840s, the career and personal histories of his various owners, and the motives of those who became his early advocates. It is a tangled story, complicated by conflicting interpretive possibilities and ambiguous evidence. But Ehrlich employs careful judgment and straightforward prose to establish very clearly that Scott's case took its initial form without reference to larger slavery issues. Litigants at first perceived no broader sectional implications, but as appeals went forward, lawyers and jurists began to expand the questions involved and make them even more controversial. The final case of *Dred Scott v. Sandford*, it is clear, was in no way the product of conspiracy. Yet Ehrlich's treatment certainly clarifies why it was so widely perceived as such; his "morphology" demonstrates how the people involved in *Dred Scott*'s development actively reformulated the case into a wide-ranging vehicle for sectional expression. By treating lawyers and jurists as responsible historical actors, while de-emphasizing the analysis of abstract legal opinion per se, Ehrlich succeeds in elaborating with useful conciseness and originality how and why the *Dred Scott* case took on the form that it finally did.

The balance of *They Have No Rights* treats the processes of argumentation, consultation, and opinion rendering by which the Taney court disposed of *Dred Scott*. Readers already acquainted with modern scholarship will find in these chapters a useful analysis of familiar themes, and will especially appreciate Ehrlich's explanation of newspapers' roles in shaping popular reaction to the Supreme Court's final decision and published opinions. Yet compared to Fehrenbacher's magisterial work, Ehrlich's brief treatment of *Dred Scott* may seem less substantial than it actually is. By restricting his scope and without advancing inflated claims, Ehrlich has suc-

ceeded in rendering a clear, developmental account of the *Dred Scott* case as it progressed through the courts to its final but disruptive adjudication.

JAMES BREWER STEWART
Macalester College

MECHAL SOBEL. *Trabelin' On: The Slave Journey to an Afro-Baptist Faith*. (Contributions in Afro-American and African Studies, number 36.) Westport, Conn.: Greenwood Press. 1979. Pp. xxv, 454. \$25.00.

Mechal Sobel, who teaches history at the University of Haifa in Israel, has contributed in *Trabelin' On* an important survey of the church life of black Baptists in the antebellum South. Unlike most treatments of slave religion, this book focuses on the "visible institution" of quasi-independent black churches, which, Sobel demonstrates, were more numerous and widespread than previously thought. Gathering data from church minutes and association records, she lists and briefly describes 205 black Baptist churches founded before emancipation. Of these, 130 existed in the South. Sobel also narrates the activities of black Baptist preachers and sketches the growth of black congregations over time.

Historians of slavery and of American religion will welcome Sobel's data on black churches and ministers. Her explanation of the transformation of slaves from Africans to Afro-Americans is likely to meet with disagreement, however. Although recognizing that there was no single cosmology for West Africans, she asserts that a distinctive "African Sacred Cosmos" did emerge in America. This African Sacred Cosmos began to change as African languages and pidgin English gave way to Creole, which in turn yielded to black English by the third generation of American-born slaves. Sobel's model for cultural change is linguistic: just as slaves moved from African languages and pidgin to Creole and finally to black English, so they moved from an African Sacred Cosmos to African Christianity and finally to an Afro-Christian world view. This model might have been more enlightening if she had probed more deeply the relationship between language and cosmology. As it stands, her analogy is more suggestive than illuminating.

Confronting the related issue of the transmission of African religions in the U.S., Sobel argues that slaves were influenced by African concepts of personality and spirit. In the African concept of person, the soul is a complex or multiple entity. Besides the self there is another inner self, a "little me within me." Experience of the inner self was a common constituent of many ex-slave conversion narratives, and, according to Sobel, the vision of a "little or another me" differentiates black conversion narratives from those of whites. Moreover, the African concept

that spirit originates from the Supreme God and returns to Him continued to inform Afro-Christian visions of traveling to heaven and encountering the Lord. African notions of spirit as animating force, enlivening gods, men, animals, and objects, formed the basis for slave conjuring. There is much that is valid in Sobel's analysis of African influence on slave belief and behavior, although she tends to overstate the case. Comparison with black religions in Latin America might have helped her to see the issue of African influence in broader perspective and might have led her to moderate her views.

On balance, *Trabelin' On* is a valuable source of information about the formation and growth of black churches from 1760 to 1865. And, as the author intends, it is a useful chart for further research into black ecclesiastical history.

ALBERT J. RABOTEAU
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DONALD G. NIEMAN. *To Set the Law in Motion: The Freedmen's Bureau and the Legal Rights of Blacks, 1865-1868*. (KTO Studies in American History.) Millwood, N.Y.: KTO Press. 1979. Pp. xvii, 250. \$19.95.

To be enlightened on the history of the Freedmen's Bureau, with its organization of assistant commissioners in the states and its agents in the counties and municipalities of the South, is to be enlightened on the essence of Reconstruction itself. For these men probably knew more about the practical everyday problems of Reconstruction and the prospects for a workable policy than any other group in the country. In a tightly and lucidly written book, based primarily on Freedmen's Bureau records in the National Archives, Donald G. Nieman is indeed enlightening. His book is an important contribution to the history of the Bureau and in turn of Reconstruction.

Although Nieman focuses on legal protection for blacks against white violence, in drawing up and implementing labor contracts, in the freedmen's struggle to retain possession of abandoned lands, and in civil rights, these matters were so central to the operation that the book is in essence a history of the Freedmen's Bureau. In important respects it supersedes George Bentley's work of twenty-five years ago.

Nieman's method is to let the evidence speak for itself, and it does speak, in indubitable tones. What the evidence reveals is a Bureau whose members are frequently—but not always—dedicated to the fulfillment of their charge, but with the odds overwhelmingly against them. This was so for several reasons: a prevailing adherence to states' rights, North and South, in the Reconstruction era that

vastly reduced the effectiveness of the Freedmen's Bureau Bill and the Civil Rights Bill; the absence of enough military to enforce the laws and the reluctance of military leaders to use what power they had; the insurmountable difficulties of operating in an alien region whose unchanging values and social mores underlay both overt and subtle resistance; the current fear of paternalism among Bureau officials of all ranks; and a president who unceasingly worked to undermine the agency. It should be said, however, that Nieman places Andrew Johnson in a proper perspective. While he does not minimize Johnson's role, he makes it clear that this was only one cause, among many, for the Bureau's frustrations and failures.

With originality and keenness of insight Nieman shows some of the ironies in the Bureau's operation: state codes were rewritten, for example, to make them conform to the Civil Rights Bill. But the new, apparently liberal, laws actually weakened the Bureau in dealing with civil rights because cases arising under them could be decided in state courts where probate judges and justices of the peace meted out traditional southern justice to blacks. The Bureau was most effective in enforcing civil rights in Kentucky because that border state did not permit Negro testimony in cases involving whites. It has long been known that the Reconstruction Acts of 1867 and 1868 did not revolutionize the South, but it is surprising to learn how little difference these laws made to the Bureau or the freedmen it was trying to protect.

Quietly and without polemics, Nieman's book suggests that the "retreat from reconstruction" was not merely a phenomenon of the 1870s but that it began very early and at the grassroots of southern society. "In the face of massive resistance from white southerners," he writes, "a temporary and poorly staffed agency which possessed only limited authority could not perform the herculean task of providing blacks with a firm basis for freedom" (p. 222).

To Set the Law in Motion is the first book of a promising young scholar. Let us hope that he will be encouraged by his success to continue his scholarly efforts.

PATRICK W. RIDDLEBERGER
Southern Illinois University,
Edwardsville

ERNEST WALLACE. *The Howling of the Coyotes: Reconstruction Efforts to Divide Texas*. College Station: Texas A&M University Press. 1979. Pp. xii, 217. \$15.75.

When Texas entered the Union, in 1845, it claimed an area that could have been divided into 312 states of the size of Rhode Island. According to the joint

resolution for annexation, Texas had the privilege of dividing itself, not into 312 states, but into as many as five. It gave up more than a fourth of its territorial claim in the Compromise of 1850, but it retained so vast and so diverse an expanse that, from time to time, a further cession or a division made sense to some of its citizens. During the next decade and a half they proposed various and conflicting plans for reducing its size or for cutting it up into two or more states.

Divisionists came the closest to success with the onset of Radical Reconstruction. The Radical Republicans, in Congress and in the Texas constitutional convention, were now the most enthusiastic advocates of partition. Thaddeus Stevens liked the idea, and the Joint Committee on Reconstruction approved the Beaman bill, which would have carved Texas into three parts, but which never got through Congress. The favorite scheme of Texas Radicals would have created a new state of West Texas and would have admitted it into the Union while leaving the rest of Texas unrepresented and under military occupation. In 1869, a draft of a constitution for the new state was published as a pamphlet (of which only a half-dozen copies are known to have survived). Even the proponents could not agree on the boundaries, however, and opponents derided the "State of Coyote" and "the howling of the coyotes" who wanted it. The controversy prolonged the convention and helped to account for the delay in the readmission of Texas. "Since 1869, the 'Coyotes' have been heard occasionally on the western horizon, but the noise has been from a lonely few baying at the moon rather than from a ravenous pack intent on tearing to pieces its helpless victim" (p. 137).

Though historians of Texas have made passing reference to divisionist movements, Ernest Wallace is the first to undertake an exhaustive treatment of the theme. His book is rather brief, with only about 144 pages devoted to the history of the subject and with 46 given over to an appendix reprinting the Constitution of the State of West Texas. Still, his account is authoritative and detailed enough to be called definitive. Nine maps show the outlines of West Texas and other proposed new states. The book will be of interest primarily to aficionados of Texas history, secondarily to students of Reconstruction in general.

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RONALD T. TAKAKI. *Iron Cages: Race and Culture in Nineteenth-Century America*. New York: Alfred A. Knopf. 1979. Pp. xviii, 361. \$15.95.

Over the past decades, historians have made numerous efforts to analyze the racial thinking of white Americans toward blacks, Indians, Chinese, and other "coloreds" of the world. With few exceptions, these efforts have tended to focus on one race at a time. Ronald T. Takaki has attempted to broaden our range of vision by seeking areas of commonality that encompass all of white America's race attitudes. He has recognized the multifaceted nature of this task—the need to examine racism against the background of broadly defined political, social, and economic forces—and has convincingly extracted the racial mood of white Americans from the ideologically murky waters of the nation's self-portrait. To be sure, this was no easy task as it required Takaki to stalk profanely through the very sanctum sanctorum of American beliefs. Utilizing a broad array of sources—from Benjamin Rush and Thomas Jefferson to Bret Harte, Henry George, Alfred T. Mahan, and others—he has examined those values, images, ideas, and assumptions held or shared by the "culture-makers" and has attempted to tie racial thought to a broader spectrum that included the Protestant ethic, principles of self-government, nationalism, and individualism. Borrowing from both Marx and Melville, he concludes that nineteenth-century white Americans "mutilated" the full potential of their ideals by allowing racial oppression to feed on the very forces that made America into a world power. Each phase of America's transformation from an agrarian-commercial economy to a technological and bureaucratic society has its own "iron cage" within which society defined, controlled, and projected its image. And within each cage, Americans played out a role that integrated both the demon and the divine, or, to borrow from D. H. Lawrence, "clever America lies on her muckheaps of gold, strangled in her own barbed wire of shalt-not ideals and shall-not moralisms."

What Takaki has undertaken is ambitious by any standard, although there is certainly nothing novel in the attempt. In recent years historians have taken great pains to dissect the dynamics of American politics and the paradox of its democratic ideology. Some of the most exemplary thinking among white Americans in their effort to assert democratic ideas is ironically wed to racial attitudes of the basest sort. The reality of this paradox brings one in full circle (Melville would have been proud), for the very term "freedom" acknowledges an irksome internalized conflict that casts shadows across its broadest interpretation. The "colored" races stood at arm's length from the body politic and had to be made safe before being made democratic.

Over and over again, one is impressed with the breadth of Takaki's analysis. Yet there is room for even greater breadth. His American character-

izations of the Chinese and Negroes are strikingly reminiscent of the depictions given by Carl von Linnaeus in his *Systema Naturae* (1735), and G. W. Cable's portrayal of blacks as a race under the influence of the instinctual life had a "scientific" origin that extended backward through the centuries among European savants. Had Takaki read more in the medical and scientific literature of the era, he probably would have had to expand his theme to encompass the self-images of Western culture as opposed to simply American society. Attitudes on women's inferiority that he attributes to American physicians was part of a much larger anthropological and medical background with origins deep in European thinking. The anthropometry of race and sex has a most fascinating history with the authorship of studies on the brain weight of women and the mental differences between the races and sexes bearing no one stamp of nationality. Despite these deficiencies, *Iron Cages* is a thoughtful and important study and deserves careful reading by historians and social scientists.

JOHN S. HALLER, JR.
Indiana University Northwest

ROBERT C. BANNISTER. *Social Darwinism: Science and Myth in Anglo-American Social Thought*. (American Civilization Series.) Philadelphia: Temple University Press. 1979. Pp. 292. \$17.50.

Robert C. Bannister reassesses themes Richard Hofstadter pioneered in *Social Darwinism in American Thought* (1944). Here Bannister is only concerned with examining the impact of those specifically "Darwinian concepts of struggle for existence, natural selection, and survival of the fittest" (p. 7), not with evolutionism more generally. Bannister insists that Hofstadter was wrong to argue that a powerful conservative Social Darwinist movement justified the status quo in the Gilded Age and sanctified racism, imperialism, and militarism after 1890. Indeed, Darwinism repelled conservatives because of its competitive and relativistic implications. Bannister's thesis is that reformers and liberal academics created a "myth" of conservative Social Darwinism for their ideological purposes. Bannister effectively shows that Gilded Age conservatives often labeled Social Darwinists did not fit his definition of the term. Such conservatives as William Graham Sumner, Herbert Spencer, and Andrew Carnegie wanted "stability, consensus, homogeneity, and peaceful change under a capitalist regime"; therefore, they found "little comfort in a cosmology that posited permanent struggle as the engine of progress" (p. 136). They retained their faith in classical economics, "Lockean liberalism," and a "Franklinesque" success mythology (p. 136). Bannister

concedes they were evolutionists, but Lamarckians, not Darwinians. Then Bannister traces the emergence of the myth of conservative Social Darwinism from the 1890s to the 1940s on two levels simultaneously: certain post-1890 intellectuals usually considered Social Darwinists—eugenicists, racists, Nietzsche's followers, certain literary naturalists, and foreign policy commentators—who were not Social Darwinists by his definition; and how reformers and liberal academics invented the myth of Social Darwinism as a weapon to use against their conservative opponents in public politics.

No brief review could treat Bannister's intelligent book adequately. His discussions of particular individuals appear generally accurate, and his thesis of the "myth" of conservative Social Darwinism as he defines it is a useful contribution. If his discussion of Gilded Age conservatives amply demonstrates that they were not "Social Darwinists," this is a valuable but not especially new characterization. Bannister seems unaware that Lamarckianism could be and was used by conservatives to justify the status quo. That Bannister found so few Darwinian social thinkers is less astonishing when it is understood that many American scientists resisted Darwinian selectionism until the 1920s. Bannister's grasp of science and its history is decidedly superior to Hofstadter's, but it can be criticized here and there, as, for example, when Bannister correctly summarizes the popular misconception of August Weismann's scientific ideas circa 1900 but unfortunately shares that misapprehension. And Bannister ignores the new scientific hereditarianism after 1900 (as, for example, mutationism and mental testing) that lent fresh scientific legitimacy to eugenics and immigration restriction and created racial mental testing—perhaps it was these socially divisive developments Hofstadter mislabeled "Social Darwinism." Finally, Bannister's style is often unnecessarily difficult, and his discussions of individuals and themes after 1890 are often too brief, as, for example, his treatment of the Social Darwinist myth after World War I, which is lamentably reduced to an abbreviated epilogue.

Yet on balance Bannister has added to the general understanding of "Social Darwinism."

HAMILTON CRAVENS
Iowa State University

SUSAN ESTABROOK KENNEDY. *If All We Did Was to Weep at Home: A History of White Working-Class Women in America*. (Minorities in Modern America.) Bloomington: Indiana University Press. 1979. Pp. xx, 331. \$17.50.

Another in a series of studies of minorities in modern America, this work presents a succinct portrait

of white working-class women from the colonial period to the 1970s. The colorful title is taken from the 1902 protest of Jewish women in New York's East Side, who organized a boycott to protest the rapidly rising price of kosher meat. The boycott turned into a riot, and the police arrested dozens of women. When Rebecca Ablowitz was asked by a judge why she had taken part in the attack on butcher shops, she told him: "We don't riot. But if all we did was to weep at home, nobody would notice it; so we have to do something to help ourselves."

This incident is related (p. 65) as part of a section dealing with immigrant women from several cultures. Much of the terrain covered up to this point in the book has been previously mapped by historians, and while Susan Estabrook Kennedy's treatment is informative, it adds little to what is already known. While it may be unfair to criticize the author for this failing in so short a study, one can still fault her for adding little, and sometimes less in terms of certain key episodes, such as rise and decline of the Female Labor Reform Associations of the 1840s. There is also a tendency in these early sections for too much facile generalization and too little depth of analysis.

The book picks up momentum during discussions of World War I and World War II as Kennedy clearly demonstrates that both of these conflicts held out only a glittering promise of economic and social advance for working women. These sections are a welcome addition to the literature of the period. They furnish valuable information rarely to be found in most standard histories of the United States.

I wish the author had maintained these standards in her discussion of the late 1920s and early 1930s. One would not know from this work that the period between 1926 and 1930 saw some of the fiercest battles involving white women workers. In 1926, a militant strike broke out in the silk mills of Passaic, New Jersey. Two years later, over twenty-six thousand cotton mill workers in New Bedford, Massachusetts, most of them women, struck. The most famous strike of the period took place in Gastonia, North Carolina in 1929-30. Nearly all of these hard-fought battles were lost. But though bitter defeats, they planted the seeds that were to flower in the 1930s. Again, these strikes were under Communist leadership, and this may be the reason for their neglect in the present work. In general, Kennedy either overlooks or dismisses left-wing contributions to improving conditions to working-class women.

Kennedy also fails to give sufficient credit to the CIO and its efforts to organize all workers and not just the skilled. While it is true that the CIO attached low priority to fighting sexually discriminatory employment and wage scales pervading indus-

try, to equate it with the AFL, as Kennedy does, is truly startling.

Despite these flaws, this is basically a solid and useful book and one that reflects at various points the most recent historiographical trends in the field. Its superb bibliography includes an excellent collection of manuscript sources, unpublished studies, and most of the important general works. There is detailed documentation for each chapter and a useful index. In the likely event of a second printing, International Workers of the World (IWW) should be changed to the real name of the organization—Industrial Workers of the World (IWW).

PHILIP S. FONER
Lincoln University

GILBERT A. HARRISON. *A Timeless Affair: The Life of Anita McCormick Blaine*. Chicago: University of Chicago Press. 1979. Pp. x, 253. \$15.00.

As the McCormick reaper was transforming American agriculture, Anita McCormick Blaine, daughter of its inventor and wife of a son of James G. Blaine, was being transformed along with it. A woman of generous instincts and large perspectives, her lifetime spanned the Chicago Fire and the Korean War, and she used the years in between to make her enormous wealth count for educational, civic, and internationalist advance. *A Timeless Affair* is as much a biography of a family and an era as of an individual and has at its center a woman whose character was fortunately firm. This often turbulent story astonishingly reveals Blaine as able to surmount the religiosity of her mother without retreating into cynicism, to absorb the business distractions of her father without developing filial hostility, to oversee the care of seriously disturbed siblings with minimal resentment, and to endure the premature deaths of a husband and an only child without plunging into any of the ill-advised forms of consolation available to wealthy widows.

A less impressive side of Blaine's life has to do with her many philanthropies. It is true that she never took the predictable posture of philanthropist autocrat. Sensitive to the competing managerial viewpoints that often vied for dominance in the organizations she supported, especially in the progressive Francis W. Parker School, she usually steered a reasonable course and eschewed excessive interference. It is also true that her benefactions and interests were many and varied—the City Homes Association, the Consumer League, Hull House, a Swiss school for the children of League of Nations delegates, the Foundation for World Government, and others. But there appears in her choosing and planning no central philosophy integrating these activities. Her swift acceptance of the ideas of the

renowned student of psychic phenomena, Sir Oliver Lodge, provides a telling gauge of the ease with which her backing could be enlisted. It inevitably compromises the admiration we feel for a millionairess who fearlessly supported Henry A. Wallace and the Progressive Party and later subsidized two newspapers—the *National Guardian* and the *Daily Compass*—intended to promote Wallace's ideas and career.

Gilbert A. Harrison has drawn from a painstaking search through family archives a portrait both vivid and convincing. Blaine's selflessness and her belief in "concession and cooperation" as "preferable to confrontation" (p. 148), for example, are made compatible with her support of Chicago's outspoken union leaders, just as her militant stand against Hitler is seen as an extension of her earlier advocacy of the League of Nations.

Nonetheless, the impression remains of an heiress too isolated from other women to become a feminist, too reluctant to unsettle her comfortable routine to be an activist, and too deficient in serious education to link seasoned intelligence to well-intentioned money.

ANNETTE K. BAXTER
Barnard College

DEE GARRISON. *Apostles of Culture: The Public Librarian and American Society, 1876-1920*. New York: Free Press. 1979. Pp. xvi, 319. \$14.50.

This is an important, well-written, and entertaining book. Dee Garrison traces the development of the public library as one of the many institutions designed to deal with the problems of an urban, industrial America. More important, she analyzes the development of librarianship in the context of the broader professionalization and efficiency movements in American society. But her greatest contribution is the way she relates this professionalization of the library to its feminization. Although the first woman library clerk was not hired until 1852, by 1910, 78.5 percent of all library workers were females. Only teaching and nursing were more feminine in that year, and, by 1920, with 90 percent of all librarians women, only nursing was a more feminized profession.

The professionalization of the library coincided with the arrival on the American scene of college-educated women who "rejected their mothers' role of piety and passivity." Many of these women became librarians, but they rarely rose to the top positions in their profession. In the words of one librarian of Congress, they lacked the "superior traits of men," specifically "manliness" and "a sense of proportion." Instead, they took low-paying jobs and became devoted followers of the male leaders. It was assumed by men and women alike that women

had more patience and more accuracy" in details than men and thus could be assigned the tedious job of cataloguing. The nature of library work itself, Garrison argues, served to perpetuate the low status of women in American society. But just as important, "female dominance of librarianship did much to shape the inferior and precarious status of the public library as a cultural resource; it evolved into a marginal kind of public amusement service."

A brief review cannot do justice to this provocative and interesting book. We are introduced to some fascinating characters; John Cotton Dana, William Frederick Poole, Justin Winsor, Charles Ammi Cutter, Mary Bean, Caroline Hewins, and especially Melvil Dewey. Dewey was the principal organizer of the American Library Association, the creator of the decimal system of classification that bears his name, and the originator of schools for library training at Columbia and Albany. Dewey was a mathematical genius who could never keep his own accounts straight; he was an apostle of thrift and efficiency who was beloved by many including a group of devoted women; but he was also hazed, and scandal followed him during his frequent moves. Still, this strange and complex man was the single most important force in the development of the modern American library.

This book would be worth reading if only for the chapters on Dewey, but there is much more. Garrison makes a careful analysis of popular American fiction in the nineteenth century and the attitudes of librarians toward that fiction. She makes interesting comparisons between librarians and social workers, and she suggests that World War I and the Library War Service began to change the role of the female librarian and led eventually to the decline of the "genteel library hostess." This book may not please librarians, but historians will find it full of interesting and useful insights.

ALLEN F. DAVIS
Temple University

FREDERICK S. ALLIS, JR. *Youth from Every Quarter: A Bicentennial History of Phillips Academy, Andover*. Andover, Mass.: The Academy; distributed by the University Press of New England, Hanover, N.H. 1979. Pp. xxvii, 770. \$25.00.

Frederick S. Allis, Jr., has declined to write a social history of Phillips Academy, Andover, though he is aware of some recent work following that method. He has written instead a scholarly, institutional history intended both for professional scholars and for an Andover audience curious about their school's past.

Allis follows conventions of other institutional studies, developing themes similar to those of Richard Hofstadter and Frederick Rudolph concerning

colleges and universities. The story of Andover is a tale of progressive reform away from the early "rigid and oppressive insistence on conformity to the old Calvinist creed" (p. 221). He divides Andover's past in half with the turning point coming at about 1870. In the first period, founders established a school to preserve traditional values in a revolutionary society. Between 1810 and 1870, Allis argues, there followed a static period of preserving the ideas of the founders. "The School changed very little" (p. 117). "In its second century it changed markedly" (p. 219). For most of its two centuries, Allis maintains, Andover has remained isolated from social and political changes outside the school. Most of the pressures for change that he discusses came from within Andover.

The book is organized around the administrations of headmasters. It deals with topics familiar in the history of higher education: curriculum, faculty, leading individuals, physical plant, and student life and discipline. It does not explore some significant changes that Allis mentions in passing, like the growth of the student body, its social origins, or influences upon the curriculum from external intellectual currents. Nor does Allis examine ways in which early nineteenth-century Andover was disturbed by changes in New England society, as evidenced by the influx of charity scholars, the wide range of student ages, and the difficulties in housing, feeding, and disciplining students. Identical problems appeared at other schools and colleges. Here Allis's focus on a single institution has exaggerated the sense of Andover's isolation, uniqueness, and static condition.

Allis has made creative use of manuscript material and institutional records in the school collections, and his book demonstrates the richness of these sources. The book raises significant issues concerning the role Andover assumed as a school for an elite. And in treating the great teachers at Andover, Allis has drawn attention to an important question. How did this school continue to demand excellence in teaching, with conscious, critical concepts of method and curriculum? Here has been one key to Andover's distinction. In calling attention to this emphasis, Allis has made his most important contribution.

DAVID F. ALLMENDINGER, JR.
University of Delaware

GERALD MCKEVITT. *The University of Santa Clara: A History, 1851-1977*. Stanford: Stanford University Press. 1979. Pp. viii, 385. \$19.50.

In a brief introduction to his history of the University of Santa Clara, Gerald McKevitt provides a useful summary description of the state of higher education in mid-nineteenth-century America. Pri-

vate sectarian schools predominated, competition for students was intense, institutional mortality rates were high, and quality was low. Furthermore, those colleges that survived into the post-Civil War period were increasingly challenged by the proliferation of state-supported institutions created in response to growing demands for more "democratic" and "practical" higher education.

The University of Santa Clara, founded in 1851 on the ruins of a Franciscan mission, typified the sectarian antebellum college. Organized and operated by Italian Jesuits, the school began as an isolated and strictly-run boarding school for California youth, some of whom enrolled when only six years old. By 1910, however, the features that made Santa Clara a success fifty years earlier seemed likely to consign it to the educational scrap heap. To avoid that, a new president, James P. Morrissey, proceeded to renovate, reconstruct, and reorganize the college. The physical plant was modernized and expanded, the stern student regimen relaxed somewhat, and most important, the academic program significantly overhauled. Besides continuing the traditional Jesuit liberal arts studies, undergraduate training in law, engineering, journalism, and pre-medicine was expanded; students could substitute modern languages and literature for Latin and Greek, and the elective principle was introduced on a limited scale. Finally, the "College of Santa Clara" was officially declared a university at commencement exercises in 1912. Actually, McKevitt notes in his conclusion, Santa Clara did not achieve "full-fledged" university status until well after World War II. Not until then did the introduction of coeducation (1961), graduate programs in the arts and sciences, library facilities, faculty training, and institutional policy bring the university "into line with the prevailing pattern of American Education" (p. 309).

McKevitt's book is gracefully written, thoroughly researched, and considerably more valuable historically than the usual college history. Specifically, it provides an excellent picture of frontier California during the first years of American control and, above all, it offers a splendid and, in this reviewer's opinion, balanced explanation of Jesuit educational ideals. The only mildly distressing features of an otherwise attractively illustrated, well-made, and carefully edited volume are the absence of a bibliography and the placement of notes at the back of the book.

G. M. REED
Georgia Institute of Technology

SUSAN MCINTOSH LLOYD. *A Singular School: Abbot Academy, 1828-1973*. Andover, Mass.: Phillips Academy; distributed by University Press of New England, Hanover, N.H. 1979. Pp. x, 626. \$15.00.

Abbot Academy of Andover, Massachusetts, was the younger sister of Phillips Academy, the oldest incorporated boarding school in the United States. Susan McIntosh Lloyd taught history there until 1973 when she and Abbot merged with Phillips.

As intellectual history her study offers a number of revealing anecdotes. One learns that *Butler's Anthology* held sway till the 1890s when it was supplanted by James's *Psychology*. One finds Abbot Principal Philena McKeen relying on Archbishop Ussher's biblical chronology in her church history syllabus of 1879, though in the same year *McGuffey's Reader* dropped the creation story.

As feminist history, the work presents a fascinating assortment of women: alumna Henrietta Jackson Hamlin, wife of the first missionary to Turkey, who withstood hostile officials and rock-throwing neighbors to win the confidence of needy people; drama teacher Emily Hale, confidante, critic, and platonic love of T. S. Eliot; Cathlyn Wilkerson, who as a senior marched against nuclear power in 1962 and as an alumna blew up her parents' Greenwich Village townhouse making bombs for the Weathermen.

As a history of education, *A Singular School* allows one to plot the changing ideals of private female secondary schools. Early Abbot was a homogenous community—evangelical-Protestant in religion and Whig-Republican in politics. It had a cloistered atmosphere, upheld by rules, regulations, and revivals. A barbed-wire fence kept Abbot and Phillips apart. For students Abbot offered a protective environment in which young women could make the transition from childhood to marriage or career. For faculty (mainly unmarried female) it provided identity, influence, and home, though scant monetary reward. Changes were very slow in coming. In 1930 Abbot admitted its first Jewish student; the first blacks appeared in 1953.

But the 1960s wrought a veritable revolution. Dress codes, chapel, discipline, and requirements were swept away by enthusiastic young faculty and new administrators, despite a few raised eyebrows by alumnae and parents. The transformation brought boys and excitement to the campus but also abortion and drugs. When little was left to be liberalized, Abbot took the final step by merging with Phillips Academy. Thus an institution that had withstood one hundred forty-four years of change ceased to be "a singular school." Such was the power of the 1960s.

Lloyd emphasizes Abbot's ties to the Andover community; she is up on the latest feminist studies and is particularly strong on student culture, assuming that the real education went on outside the classroom. But this can be carried too far. Classes and teaching and lessons and learning are slighted to the point where one has difficulty remembering

that this is the history of a school. Though some scholars will applaud this culturalist approach, to this reviewer it often seemed like attending a performance of the play *Hamlet* with the character Hamlet left out.

STEVEN J. NOVAK
University of California,
Los Angeles

ALFRED RUNTE. *National Parks: The American Experience*. Lincoln: University of Nebraska Press. 1979. Pp. xiv, 240. \$16.50.

In what is more an essay than a narrative, Alfred Runte offers an interpretive journey through the history of the National Park System. Runte's style is lucid and forceful, at times argumentative, and his choice of themes and manner of presentation make his book an important statement on the history of conservation.

Runte offers a number of theses, many of them unremarkable but previously ignored in scholarly literature on the parks. Unfortunately, he often weakens his arguments with selectivity in supporting data and incomplete research. Runte believes that America's park system owes its origins to national shame over the despoilation of Niagara Falls compounded by a search for identifiable national monuments comparable to the ruins and scenery of Europe. That is a notion others have explored, although never to Runte's depth. He offers a great deal of support for his argument but becomes tendentious when he cites early comparisons of Yellowstone's wonders to Old World castles while ignoring the fact that early visitors more often compared the region to Hades.

More broadly, Runte asserts that national parks have consistently comprised economically "worthless" lands and that "complete conservation" remains a dream. That is so obvious that it is remarkable that it has received little previous scholarly attention. It is, however, a fact of life for the National Park Service and its clients. Finally, one must question Runte's tendency to accuse the nation of shortsightedness in not applying in early years concepts of natural area preservation that are of fairly recent origin.

Despite these and other weaknesses of interpretation, this is a good book. There are, however, a number of major and minor elisions in research and text that prevent its being the comprehensive interpretation that the author seeks. As examples of the minor, the account of the origins of the Antiquities Act glosses over events that spanned many years and wrongly gives sole credit to one man; Runte's search for signs of the "national park idea" ignores the "idea's" invention by Hiram M. Chittenden in

1895; and the account of the See America First campaign ignores the historical context of isolationism and disgust with European wars.

Among major elisions, Runte ignores the majority of the National Park System (its historical area category) to focus solely on the great natural parks. More fundamentally, Runte made no evident use of the wealth of historical work performed within the Park Service, which has for many years explored and analyzed most of the subjects that Runte treats, and which could have supported and refined his case.

DAVID A. CLARY
Springville, Indiana

REID BADGER. *The Great American Fair: The World's Columbian Exposition & American Culture*. Chicago: Nelson Hall. 1979. Pp. xvi, 177. Cloth \$22.95, paper \$10.95.

Nineteenth-century Americans took their world fairs very seriously indeed, says Reid Badger in this interesting study of the Columbian Exposition of 1893 in Chicago, the pre-eminent example of its type. A trip to the fair was worth selling the cooking stove, Hamlin Garland told his parents. Perhaps 5 to 10 percent of Americans, Badger estimates, were drawn to the "White City" in Jackson Park, South Chicago, on the shore of Lake Michigan.

Unlike world fairs after 1920, which were only "remnants of an earlier age" (p. xv), Badger says the nineteenth-century events were microcosms of national culture that provided "an objective focus for cultural self-examination and self expression" (p. xvi). As these quotes and the subtitle of the book imply, the focus of this study, when it moves beyond telling the story of how the fair was originated, planned, financed, and built, is on its general cultural significance. The author's point of view is from the top down, which is reflected, among other places, in the book's numerous illustrations, mainly views of the fair buildings. The fair here is not seen as a social event; relatively little attention is given to the fair itself, most to assembling it and speculating about its symbolic cultural importance.

This vantage point may be justified if America in 1893 still enjoyed, unlike the twentieth century, a more or less homogeneous national culture. But Badger does see a "confusion of symbols" (p. 119) at the fair, between, for example, the rigid classicism of its buildings (formal, academic, monumental) and the "sylvan scenery" (p. 66) of the informal lagoon inspired by Frederick Law Olmsted's twenty-year-old plan for Jackson Park. And he grants that the fair's Midway (from which the modern term originates), whose sideshows were presented under

the guise of authentic foreign and primitive cultures, had an independent vigor.

Was this latter fair, what one might call the people's fair, the same event as the other fair into whose planning and staging this book primarily inquires? What did those millions of visitors who flocked to see the Midway's eleven-ton cheese and Arabian "belly-dances" and who whirled around on the world's first Ferris wheel (since moved to Coney Island) think of the severe Court of Honor that dominated the fair's architecture? Opinions and estimates of the fair's meaning here proceed from the spokesmen of official, usually genteel, culture. It is no wonder that *they* see the fair as a cultural counterweight against social change, as something suitable for "a season of mediocrity and repose" (p. 85), as Henry Watterson said in his dedication oration. What the rest of the people thought, felt, or did is not pursued here.

JOHN TOMSICH
Reed College

ROGER LANE. *Violent Death in the City: Suicide, Accident, and Murder in Nineteenth-Century Philadelphia*. (Commonwealth Fund Books.) Cambridge: Harvard University Press. 1979. Pp. xiii, 193. \$16.50.

In this short book on a big subject, Roger Lane examines violent death in nineteenth-century Philadelphia to learn something about how people lived. He concludes that after about 1870 the homicide rate went down and the suicide rate went up. Accidental deaths also rose, in large measure because of increased dangers of the workplace, mechanical means of transport, and the use of liquid fuels in the home. The kinds of accidental death (such as drowning) associated with impulsive behavior and the miscalculation of risk declined, a finding consistent with lowered homicide rates.

Four of six chapters and two appendixes are devoted to an examination of the numbers, the sources upon which they are based, the methods used to derive them, and assessments of their reliability. Lane has not performed any tests of statistical significance; what he has done is meticulously examine and weigh the evidence to convince the reader that his tables are useful representations of reality. Even the most resolutely nonquantitative historian will be impressed by the care with which Lane has generated his figures and the arguments he offers as to why certain statistics, such as the officially reported number of homicides in the 1840s and 1850s, must be considered systematic undercounts.

More important than any absolute numbers are the rates of change noted above, and more important still, the explanation Lane offers for these

changes. In his view the development of more controlled, depersonalized, and bureaucratic work environments and the public school system designed to produce disciplined workers placed a high premium on predictable, nonimpulsive behavior. People socialized in such school and work environments were more likely to internalize their aggressions and thus be more prone to suicide than homicide. They were also less likely to be involved in drunken horseplay around the docks that helped contribute to the higher rates of drowning before 1870. Men in less-controlled work situations and transients committed more than their share of homicides. Since this group made up a smaller percentage of the total population in 1900 than it had in the middle of the century, the overall homicide rate dropped. As Lane notes, his analysis rests upon a "relatively simple behaviorism" (p. 135), one that might not satisfy a psychologist but does make sense to a historian as consistent with the thrust of social change in the nineteenth century. He also offers a useful explanation of the continued high homicide rates and increasing suicide rates among young black males in recent decades.

The writing is clear, often witty, so that the book is enjoyable as well as useful. It is unfortunate that the notes are grouped at the end, especially since many of them are glosses on the text and not just references.

JAMES F. RICHARDSON
University of Akron

IAN S. HABERMAN. *The Van Sweringens of Cleveland: The Biography of an Empire*. (Western Reserve Historical Society, number 148.) Cleveland: Western Reserve Historical Society. 1979. Pp. 202. \$10.00.

Seeking to emphasize for a class of second-year MBA students in business history the profound impact of the railroad upon midwestern river towns, I recently posed the question, "What town in Ohio once confidently expected to become one of America's major cities?" From the back benches came the present-minded response, "Cleveland!" Witty, but poor history, for if the railroad blighted Marietta's ambitions, it was a major factor in the development of Cleveland into a leading industrial and commercial center. The city's location on Lake Erie, halfway between Chicago and the northeastern seaboard; the constant improvement of navigation on the Great Lakes, which Cleveland dominated; the phenomenal rise of an American steel industry based on abundant deposits of bituminous coal just to the south of Cleveland and seemingly limitless beds of high-grade iron ore convenient to its port; the emergence of a new basic industry, petroleum,

and a local boy who would organize it into a vigorous enterprise; and, finally, the completion of a heavy-duty, low-cost railroad system that tied it all together efficiently; these had made Cleveland a great modern city by the turn of the century.

These factors and the famous men who were behind them had been at work for three-quarters of a century by the 1920s, when the brothers O. P. and M. J. Van Sweringen leaped into national prominence as assemblers of one of the most complicated railroad empires in history. In his effort to show that the brothers "were the builders of modern Cleveland," therefore, Ian S. Haberman faces an uphill fight, and many readers will conclude that he has not succeeded.

Beginning—and continuing—on a shoestring, the former newsboys started out in real estate. Their great achievement was Shaker Heights, a pioneer among the classic suburban residential developments that were at first the reward and later the refuge of the urban upper middle class. From there "the Vans" moved to an orgy of monumental civic real estate promotion that gave Cleveland its trademark and famous white elephant, Terminal Tower, ten years after the sun had begun to set on the American passenger train.

If the Vans had stopped with their acquisition of the Nickel Plate Railroad and its logical extensions, which the brilliant railroad operating man, John J. Bernet, stood ready to make the efficient heart of a new eastern trunk line, all might have been well. But the Vans slid ever deeper into the quicksands of meaningless railroad empire building. Their empire collapsed, concurrently with railroad profits, after 1930, and the brothers themselves died, less than a year apart, in the midst of costly bankruptcy and fraud proceedings. As usual, only the lawyers and accountants came through smiling.

Even with such dull boys as the Vans, who never played, a much more interesting book might have been written. Haberman has stuck close to published official documents (one, the Senator Burton K. Wheeler committee report, is cited repeatedly), and his book is mostly a summary of complex financial manipulations that are not very interesting in themselves. It is a case of moving from the particular to the particular, against which Allan Nevins warned us. Notably missing is a setting of the American railroad scene at the end of the "Progressive" era, which would have revealed how truly jejune the Vans' financial thrashings and the Inter-state Commerce Commission's bureaucratic posturings were. We do get, in the ersatz entrepreneurship of the Van Sweringens and the dyspeptic utterances of Commissioner Joseph B. Eastman, a strong hint of how far removed from the fray men with truly constructive ideas were. But the real story waiting to be told is John Bernet's remarkable

transformation of the Nickel Plate. After all, the business of business history is business.

ALERO MARTIN
Harvard University

DAVID E. NYE. *Henry Ford: "Ignorant Idealist."* (National University Publications Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1979. Pp. ix, 147. \$12.50.

Henry Ford was a great symbol of the twentieth century, but what, exactly, did he symbolize? Mass production? The five dollar day? The Alger myth? The lost frontier? As with John William Ward's symbolic Jackson, some of the answers point forward toward a brave new tomorrow, and some of them point backward to a nostalgic past. And like the protean Jackson, Henry Ford, as reinterpreted in this thoughtful and incisive study, somehow encompassed them all. He was the perfect bridge between agrarian and industrial America.

In David E. Nye's view, symbols are inherently unstable, compounded of delicate ambiguities and contradictions. Such a symbol was Ford. By embracing both past and future, both individualism and collectivism, both machine and garden, he helped to compose the inevitable tensions of nascent industrialism, reassuring countrymen that the assembly line was in keeping with their vision of an American Arcadia.

In order to explore these tensions, Nye examines both the external and internal Ford. The external Ford—the man the public knew—was a veritable kaleidoscope of images tumbling over one another: the farmer's son, the backyard mechanic, the compassionate titan, the technological seer, the cracker-barrel philosopher, and, finally the embattled tycoon. Ford himself created these motifs, but his public eagerly seized upon them and shaped them to its own felt needs. A few of the motifs had no takers. Ford the dietary enthusiast, Ford the mystic, Ford the believer in precognition and telepathy—these images were decidedly dissonant and for that reason were ignored. Yet it is to these aspects of the Ford personality that we must look, Nye argues, in order to comprehend the real man.

In other words, the real Henry Ford was the internal one. He is to be explained in religious terms, essentially, and the integrating explanation is his belief in reincarnation. That belief enabled Ford to perfect a cosmology that at once accounted for his odd quirks and his true genius. Only by accepting reincarnation and all its implications was Ford able to break through the linearity and sequentiality of nineteenth-century time and forge the segmented and accelerated sense of time of the twentieth century.

Nye's book is a valuable contribution to our understanding of Henry Ford the man and the multiple Ford phenomena. It is organized with logic and clarity and enlivened with flashes of insight. Its imperfections—chiefly the title, which even when explained is unfortunate—are relatively minor. It is a solid piece of scholarship.

FRANK W. FOX
Brigham Young University

DAVID A. WALKER. *Iron Frontier: The Discovery and Early Development of Minnesota's Three Ranges.* (Publications of the Minnesota Historical Society.) St. Paul: The Society. 1979. Pp. xi, 315. \$16.00.

During the last two decades of the nineteenth century the huge, rich iron ore deposits in northeastern Minnesota attracted the attention of American businessmen. Most notable among these were John D. Rockefeller, Andrew Carnegie, John J. Hill, and later the quintessential American capitalist, J. P. Morgan. As usual the major entrepreneurs appeared on the scene after a great deal of the risky exploration and development had already occurred. The accelerated demand for iron ore, rapid technological developments in steel production, and an improved transportation system that rendered the long rail-water shipment of Minnesota iron ores commercially feasible during the last two decades of the nineteenth century, combined with large capital outlays, brought the iron ranges into large-scale production.

The development and exploration of iron ore deposits as large and as remote as those in northeastern Minnesota required capital and organizational ability beyond the capacity of local individuals such as George Stuntz, John Mallmann, Cuyler Adams, or even the Merritt family. David A. Walker argues that the panic of 1893 and the subsequent long depression "served as a turning point in the growth and organization for Minnesota's iron mining industry" (p. 257). Faced with the need for increasingly larger financial commitments, the numerous small operators who had earlier flourished on the iron ranges gave way to a very few large concerns that rapidly gained control of the industry. These large, vertically integrated corporations commanded the necessary capital, the managerial know-how, and the leadership to operate on the scale necessary for profitable exploitation of Minnesota's iron ranges. Walker concludes that "small, independent mining companies, led by enterprising individual pioneer entrepreneurs, could not function in the deteriorating economic situation nor compete in the ruthless world of big business. Unable to match the financial capacities of the

large integrated firms, many were slowly absorbed or squeezed out" (p. 257).

For one who argues in his preface that "men and women create history" and are the "key elements in understanding past events" (p. viii), Walker places great emphasis on the impersonal role of the market, in the form of the panic of 1893, as the decisive factor in the concentration of Minnesota iron mining into a few large corporations.

The role played in the iron ranges by large corporations closely parallels two basic developments in the American economy. First, with the possible exception of farming, the phenomenon Walker analyzes was the common pattern in the frontier extractive industry whether it be fur trading, grazing, lumbering, or mining. In each, a short initial period of exploitation by individual entrepreneurs was rapidly replaced by domination of opportunity by a few large corporate firms. Second, large corporations increasingly dominated in all sectors of the late-nineteenth-century economy. Although the panic of 1893 may have provided the catalyst, developments in the Minnesota iron ranges too closely paralleled the larger experience for it to have been the decisive factor.

While most of my differences with Walker's interpretations are minor, I do think it would have been profitable to have considered the broader implication of the Minnesota frontier iron-mining experience. *Iron Frontier* is well researched, logically organized, and, except for being a bit repetitious in discussing Rockefeller's role, written in a clear, graceful style. It is a valuable contribution to our understanding of the nature of a frontier extractive industry and American business history.

JAMES D. NORRIS
Northern Illinois University

WILLIAM F. FINE. *Progressive Evolutionism and American Sociology, 1890-1920*. (Studies in American History and Culture, number 8.) Ann Arbor, Mich.: UMI Research Press. 1979. Pp. xv, 302.

In this book William F. Fine tries to do too much. First, he attempts to construct a "paradigm," which he calls "progressive evolutionism," of early American sociological thought. Except for the new terminology, his presentation of the major concepts and assumptions of early sociologists add little if anything to the work of several recent scholars. Furthermore, the paradigm has been presented in a less comprehensible form. Secondly, Fine attempts to explore the internal and external forces that shaped the paradigm. The result is an unsatisfactory mish-mash of elusive abstractions. Apart from the social problems to which the sociologists responded, none of which Fine examines in a careful way, we find

the paradigm is supposed to be a product of, among other things, romanticism, Victorianism, idealism, pragmatism, religion, Matthew Arnold's conception of culture, Darwinism, functional psychology, the German historical school, and Lamarckianism. Finally, the author tries to offer a corrective to what he believes to be the excessive emphasis by recent scholars on professionalization as a key to understanding the shifting orientation of early sociology from an ethical to a scientific perspective. Although Fine does not successfully refute this contention, he does remind us that early sociologists themselves were strongly committed to the methods of science.

This study has been published prematurely. While the author has read widely in both primary and secondary literature of progressive social thought, he should have subjected the results of his study to a rigorous critique by a tough-minded scholar. Then perhaps the diffuse, obtuse, and excessively abstract character of his study could have been remedied.

BENJAMIN G. RADER
University of Nebraska,
Lincoln

H. ROGER GRANT. *Insurance Reform: Consumer Action in the Progressive Era*. Ames: Iowa State University Press. 1979. Pp. xi, 202. Cloth \$14.95, paper \$9.50.

Convinced that the effort to reform the insurance industry remains an unexplored part of the record of the Progressive Era, H. Roger Grant investigates the regulatory drives in five key states during the thirty years after 1885. In his exploration of New York, Missouri, Texas, Wisconsin, and Kansas, Grant aligns himself with the consumer-oriented thesis of David P. Thelen and attaches less importance to Robert Wiebe's search for order or Gabriel Kolko's picture of businessmen seeking national legislation for their own purposes. Very much the partisan of policyholders and regulators against insurance companies, the author takes up first the campaign for life insurance reforms, then reviews changes in fire insurance practices, and concludes with chapters on state commissioners and the abortive drive for federal regulation of the industry. Grant has worked hard in primary sources, and he is aware of the relevant scholarly literature. His book remains, however, more a collection of discrete articles than an integrated narrative, and he fails to bring much life or excitement to what is intrinsically an arid subject anyway.

By any standard, the insurance companies deserved much of the criticism they received in the early twentieth century. Internal mismanagement, callous treatment of claims, and corrupt involvement in politics marred their performance.

Thus Grant is properly skeptical of their defenses of their actions. He did not look at the records of the companies themselves, and he often accepts attacks on their conduct as presumptively correct without an attempt to find out what the archives of the industry might disclose. In the case of Thomas W. Lawson and his "Frenzied Finance" muckraking, Grant does not include relevant details about Lawson's career as a stockjobbing promoter. Similarly, the International Policyholder's Committee is described as a "quintessential consumer group" (p. 55) with only the slightest passing recognition of the less worthy motives that Lawson and other sponsors of the IPC brought to their campaign to elect reform directors onto insurance company boards.

Grant is aware that the forces pushing for insurance revision in individual states often owed as much to intra-industry maneuvering as to consumer pressure. The Robertson Insurance Law of 1907 in Texas, requiring the investment of money from Texas in Lone Star State bonds or stocks, was an amalgam of state chauvinism and local insurance company lobbying. But these moments of even-handedness are brief ones, and soon even self-ish actions in Texas and elsewhere receive the author's blessing as consumer benevolences. Indeed, by the book's close, the word "consumer" has lost almost all substantive meaning and has become a kind of synonym for "the people" in their struggles with sinister corporate interests. Whatever their drawbacks as explanations of progressivism, the Wiebe and Kolko arguments at least moved toward a greater analytic complexity in understanding the period. Grant's study suggests that the consumer thesis may be little more than the older, romantic view of progressive reform doing business under a new label.

LEWIS L. GOULD
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Austin

WALTER KARP. *The Politics of War: The Story of Two Wars Which Altered Forever the Political Life of the American Republic (1890-1920)*. New York: Harper and Row. 1979. Pp. xiv, 380. \$15.00.

American voters around 1890 awoke from a generation of slumber and reasserted the nation's traditional "republican spirit" as they battled powerful political oligarchies through their new populist and progressive insurgencies. Abandoning the partisan loyalties that had permitted the oligarchies to control voters since the Civil War, Americans challenged political elites and their corporate allies. Losing their control of politics, the oligarchies invented foreign adventures and wars to suffocate their critics in a smokescreen of superficial patriotism. William

McKinley and Woodrow Wilson were the dishonest political geniuses who maneuvered the United States into the wars of 1898 and 1917-18.

So argues Walter Karp in *The Politics of War*, a book that combines remarkable insight and remarkable oversight. Karp writes with refreshing eloquence and passion, and this reviewer believes that a strong case could be made to support his argument, particularly for World War I. Karp plausibly traces for each of the neutrality crises how Wilson felt constrained by public opinion from promoting his desire for American participation. He argues, again plausibly, that neither Wilson nor McKinley was the hesitant champion of intervention that both tried to appear in the months and years before they finally led the country to war.

Unfortunately for those who are inclined to accept Karp's approach, *The Politics of War* is seriously flawed in both conceptualization and research. This book is not based on fresh research in primary sources. Four-fifths of the footnote references in the first third of the book are to secondary sources. Twenty-seven footnotes mention Walter Millis's *Road to War* (1935) and only twenty refer to the *Congressional Record*, Karp's favorite primary source for this section. There are, symbolically, no references to New York newspapers but six to Joseph Wisan's *The Cuban Crisis as Reflected in the New York Press, 1895-1898* (1934). More seriously, Karp seems unaware of major recent interpretations of relationships among voters, populist and progressive insurgencies, and party organizations. Although he discusses relationships between the People's Party and other parties, he seems unaware of Lawrence Goodwyn's *Democratic Promise* (1976) or Peter H. Argeringer's *Populism and Politics* (1975). Claiming to be the first to discover connections between domestic events and foreign policy, he shuns relevant books by Walter LaFeber and William Appleman Williams. He mars many of his shrewdest and most controversial observations—about public opinion and the neutrality debates, for example—by simply failing to give examples or citations.

The conceptual problems are equally severe. If the "oligarchs" favored war to quell insurgencies, why were leading Republican Senate oligarchs like Nelson Aldrich, Orville Platt, John Spooner, and William Allison "antiwar senators," in Karp's phrase (p. 91), who resisted McKinley's drive toward war? Did populists and progressives have foreign policies? Were those policies—or their absence—relevant to McKinley's or Wilson's successes? How did voters try to discipline leaders in foreign policy? Are problems in the American system of representative government, as opposed to the ambitions of particular presidents, relevant to these themes?

Professional historians would err in dismissing

this book because of its obvious shortcomings of scholarship. By reminding us that presidents in 1914-17, as well as 1979-80, have used their powers of foreign policy to distract and intimidate popular opposition to their domestic failures, Karp is addressing issues that go straight to the heart of contemporary as well as past crises in American government.

DAVID P. THELEN
University of Missouri,
Columbia

PAUL L. MURPHY. *World War I and the Origin of Civil Liberties in the United States*. (Norton Essays in American History.) New York: W. W. Norton. 1979. Pp. 285. \$16.95.

Paul L. Murphy, to whom both scholars and students are already indebted for his works on the constitutional history of the United States, offers here an extended essay on the era of the First World War, a period that he regards as crucial to American civil liberties. Except in 1798 and again in the Civil War, the protection of civil liberties was of little concern to most Americans. The reason, of course, as Murphy recognizes, was that the federal government during the nineteenth century seldom denied the freedoms of the First Amendment. His point, however, would be clearer if he defined more explicitly the distinction between civil liberties, which protect against government, and civil rights, which depend upon government. Still, more than most authorities, he emphasizes the way in which progressives from Theodore Roosevelt to Woodrow Wilson, in their desire for social control and government regulation, created an environment hostile to traditional individual freedoms. Although he does not accept the contention, which he identifies with the New Left, that "civil liberties were part of the ideology of the middle class, which could be waived at any time" (p. 23), he often follows the legal relativism so popular with many contemporary social scientists. This position may, or may not, be more objective than the old constitutional absolutism. In any case, it deprives his book of the intellectual excitement and moral fervor that characterized Zechariah Chafee's classic study of free speech in the United States.

In his introductory and concluding chapters, Murphy provides a useful overview of the monographic literature, whose help he acknowledges, as well as a brief summary of the post-World War I years when "a good many Americans came to see that domestic radicals and hyphenates were not the threat they had been painted by the Wilson government . . ." (p. 271). It is sad to note, as Murphy does, that with the coming of World War II big

government again took up the crusade against radical dissent. Once this new climate of alleged conspiracy and subversion was imposed, it was easy to revive the old agencies of repression that had been created in World War I.

The main thrust of the book covers this latter and largely familiar story, but Murphy's account is detailed and thorough, especially in its sympathetic summary of the efforts of pacifists and radicals to preserve traditional liberties. Although he is careful to include the war rationale and bureaucratic apologies offered by Wilson and members of his administration, he makes clear their responsibility and the glaring discrepancy between their liberal rhetoric and reactionary policies. It was the government, not the citizenry, that violated the Constitution and destroyed essential American freedoms. In the forefront of this illiberal repression were the lawyers and judges who, with very few exceptions, failed to stand against the popular hysteria that the president's own demagoguery, as in his Flag Day speech of June 14, 1917, helped to arouse. In contrast to Wilson, the true American patriots were such old-fashioned liberals as Oswald Garrison Villard, editor of the *Nation*, and the radical dissenter Roger Baldwin, principal founder of the American Civil Liberties Union.

ARTHUR A. EKIRCH, JR.
State University of New York,
Albany

RUTH WARNER TOWNE. *Senator William J. Stone and the Politics of Compromise*. (National University Publications Series in Political Science.) Port Washington, N.Y.: Kennikat Press. 1979. Pp. x, 278. \$12.50.

Ruth Warner Towne has written a flawed narrative of the public career of William J. Stone, the only Missourian to win election as representative to Congress (1884-1890), as governor (1892-1896), and as United States senator (1902-1918). A native of Kentucky and a Democrat by heritage, Stone moved to Missouri, became a lawyer, and launched his political career at age twenty-four with election as county attorney. In 1914, after war erupted in Europe, Stone became nationally important because he chaired the Senate Committee on Foreign Relations. Towne devotes 40 percent of her book to the period from August 1914 to Stone's death in April 1918.

Towne concludes that Stone was a talented, pragmatic politician who subordinated his own career to advance the welfare of the Democratic Party and the nation. While President Woodrow Wilson "used his magical prose and rational arguments to marshal public and Congressional support," Towne

claims that "Stone moved noiselessly and unobtrusively behind the scenes to smooth the way" (pp. ix-x). She also maintains that during "his long years in office he did not forget the miner in the pit, the immigrant at the gate, the soldier on the way to the front, and the child of the slum" (p. 254).

Unfortunately, Towne's conclusions lack sufficient substantiation. Stone's electoral success attested to obvious ability; his political sacrifices on behalf of party and nation, on the other hand, remain vague. More graphic is Towne's failure to discuss instances when Stone smoothed the way for Wilson's legislative program. She never mentions, for example, The Clayton Anti-Trust Act, The Federal Trade Commission Act, The Adamson Act, The LaFollette Seamen's Act, The Keating-Owen Child Labor Act, and The National Park Service Act. In foreign affairs, Stone broke with Wilson over the president's attempt to arm U.S. merchant ships and five weeks later voted against Wilson's request for a declaration of war. Immediately thereafter, Stone pledged unreserved support for all war measures, although he continued to judge U.S. entry into the war as "the greatest national blunder of history" (p. 233). Wilson, however, needed little help getting war measures through the Senate. Towne also offers no evidence to support her assertion that the senator consistently cared about miners, immigrants, and children of the slums.

Towne's study has other problems. She fails to describe and analyze her statement about "the latent idealism which Stone had carefully suppressed throughout his career" (p. 202); the thrust of her treatment is that he lacked idealism and operated as a politico. At times she draws troublesome conclusions, such as "Progressivism was not so much an ideology as a life style" (p. 71). In her backnotes she cites manuscript collections without providing box or series numbers. In the populism and progressivism portions of her "Essay on Sources" she includes no book published after 1963.

The book does not contribute to a better understanding of progressivism or of the Wilson administration. Beyond presenting factual information about Stone, the study has limitations as biography. The subject merited a more careful and thoughtful treatment.

KEITH W. OLSON
University of Maryland

LARRY ENGELMANN. *Intemperance: The Lost War Against Liquor*. New York: Free Press. 1979. Pp. xvi, 252. \$10.95.

ERNEST KURTZ. *Not-God: A History of Alcoholics Anonymous*. Center City, Minn.: Hazelden Educational Services. 1979. Pp. xiii, 363. \$12.95.

W. J. RORABAUGH. *The Alcoholic Republic: An American Tradition*. New York: Oxford University Press. 1979. Pp. xvi, 302. \$14.95.

What Larry Engelmann writes about in *Intemperance* is, very narrowly, the lost war in Michigan, especially Detroit, 1920-33. This narrow focus yields a chronicle of choice anecdotes about the Canadian Connection, the Prohibition Navy, rum runners in iceboats sneering at the police, bootleggers in tug boats laying underwater pipelines or towing underwater barges toward insatiable and illicit thirsts on the "dry" side of the river. What it does not yield is any device or dimension through which we can approach these characters or their antagonists in any way that might compel our sympathy or understanding. How are we to understand or to explain what had gone wrong in a society that for so long endured all this trickery, treachery, scheming, running, and shooting? We don't have an answer (even the political history is rather shallow), and, moreover, the solid scholarly work of a dissertation is stripped down to flat journalism: a list of sources, but no footnotes or endnotes, no citations; a narrative padded with a predictable sub-chronicle of national prohibition and studded with journalistic overkill. The saloon, we are told, was "receptacle for rhetorical onanism . . ." a place where a man could "air his feelings about the injustices of American society" (p. 7). For the most part, however, the prose is highly readable and a thoughtful reader can understand why the people of Michigan, who voted for a strong state prohibition law in 1918, were the first to ratify the Twenty-first Amendment.

W. J. Rorabaugh's *The Alcoholic Republic* shows us that, for all the foolishness on the Detroit River, prohibition movements had a lot to prohibit. We see that the per capita consumption of absolute alcohol for the drinking-age (15+) population in 1815 was 7.0 gallons, but in 1850, after Neal Dow's crusades, it was 1.8 gallons. In 1910 it was 2.6 gallons, but after the Dry Decade it was 1.6 gallons in 1940. The emphasis here is on the earlier period, and there are other fascinating facts to consider: for George Washington, the return on an investment of Election Day whiskey in 1758 was little better than two votes per gallon; the affliction called the D.T.s may show more about the culture in which it occurs than about alcohol, and it seems to have become an American affliction in the 1820s when the pattern of drinking comportment shifted from a casual if pervasive nipping toward a more morbid solitary or communal binging; a cup of tea then cost more than a drink of whiskey.

In explaining the "great alcoholic binge of the early nineteenth century" (p. 25), Rorabaugh finds cause in an economy of abundance (it was cheaper to drink whiskey than to transport corn to a mar-

ket), in the American diet (strong drink to wash down all that greasy pork and cornbread), and he finds assistance in some often obvious hypotheses from social science (high goals can be self-defeating, and high aspirations may cause heavy drinking, and so forth). He has looked more carefully at the manuscript sources than anyone before him, and this is lively and often impressive scholarship. But there are problems that cannot be set aside. Regarding per capita consumption, as Rorabaugh himself points out, the more significant questions have to do with who got how much—men more than women, whites more than blacks, westerners more than easterners? Even if we take the guess that maybe 75 percent of the nation's workers hoisted a daily four ounces of booze in 1830—say a little more than two honest drinks a day—this is a shallow base for an “alcoholic republic.” Regarding sheer abundance, the fact of plenty really does not explain excessive drinking, nor does the national diet, however dreadful. Regarding more socially scientific hypotheses, Rorabaugh says that “drinking mores can not be separated from and are functions of ideologies, customs, and social processes” (p. 246), but for all he has shown about drinking, he actually has little to say about the social processes, customs, and ideologies that produced so many drunks.

It may be that the drunks themselves will present some of the understanding we need. Ernest Kurtz's *Not-God* explains what Alcoholics Anonymous can say about American culture. Kurtz makes a careful distinction between the AA program, the history of which flows easily into the scientific study of alcoholism, and the AA fellowship, which has evolved in brilliant turns of pragmatism and doctrine as the founding fathers in the 1930s opened themselves to such complex influences as the Oxford Group, Carl Jung, and William James. The founders were convinced by James and Jung that in his melancholy journey, the alcoholic has actually followed the awesome but illusive promise of transcendence; stumbling down his crooked path, he comes to his ultimate disaster because he has failed to acknowledge his essentially human limitations. After a searching analysis of this axiom, Kurtz suggests that “alcoholism can be a metaphor for the dis-ease of modernity” (p. 255) and thus draws his title—that is, the individual hits bottom because he has denied the “essential not-God-ness of either self or others” (p. 226). Because of this primary insight, he says, the AA fellowship, though rooted in American culture, is able to stand outside it and to define some of its most profound paradoxes. This, Kurtz concludes, is what the drunks have learned the hard way: the need for a philosophy of pluralism, for the shared honesty of mutual vulnerability, for acceptance of an ancient religious principle—that humans

need to give-to and sacrifice-for each other, that strength arises from weakness, that the basis for brotherhood is sharing, but it is the sharing of weakness, not strength. The book is divided into two major parts: “The History” and “The Interpretation.” Both are richly scholarly and lead us carefully into an almost unknown field of American intellectual and social history.

NORMAN H. CLARK
Everett Community College

VINCENT P. FRANKLIN. *The Education of Black Philadelphia: The Social and Educational History of a Minority Community, 1900-1950*. Philadelphia: University of Pennsylvania Press. 1979. Pp. xxi, 298. \$19.95.

Touted as “the first book-length study of the education of a minority group in the United States,” Vincent P. Franklin's work examines the public school and community-sponsored education of black Philadelphia in the opening half of the twentieth century. “From the perspective of the black community, the major social purpose of public and community educational activities was the advancement of Afro-Americans in the city.”

Separate (black) public schools were a mainstay of Philadelphia's public system of education from 1822 until the mid-1930s when the schools were abolished. (Legislation passed in 1881 made it illegal to force blacks to attend segregated schools, but not illegal to open them and to encourage black parents to send their children to them). White notions of Negro inferiority (conveniently reinforced by the new intelligence testing movement early in the century), combined with the fact that separate schools were the only schools in the system where black teachers were allowed to teach, encouraged the perpetuation of Jim Crow schools. Despite, however, the increase in the number of black public schools, throughout the period the majority of black children were enrolled in racially mixed schools taught by white teachers. The abolition of separate public schools was directly related to the political coming of age of black leaders who succeeded not only in getting themselves elected to office but also in wringing concessions from the political system by effectively ransoming the black vote to whichever party acceded to black demands.

How the black community educated itself is an integral part of the educational history of black Philadelphians. Community educational activities, broadly defined, were sponsored by a network of established and newly created institutions, organizations, and agencies, the most influential of which was the *Philadelphia Tribune*, the city's most important black newspaper. If black social advancement was the ultimate aim of public and community ed-

educational activities, to what extent did they succeed? "Some of the community education programs were effective; others were not." As for the public schools, whose major objective, ostensibly, was to provide the basic tools in order that individuals might compete equally for society's rewards, Franklin found that they provided no such benefit to Philadelphia's black minority. "This investigation found not only that more public schooling did not greatly improve the overall social status of blacks, but . . . that at times the public schools were considered more an obstacle to the achievement of the larger goal of black social advancement."

True to its subtitle, nearly half of this study examines the unsettling impact the Great Migration and the Depression had on black Philadelphians. As presented (and the fault is largely one of organization), the data is only marginally useful for understanding the educational history of black Philadelphia. In other respects as well, the book retains the earmarks of a study begun as a dissertation. By intention the work focuses on the opening five decades of the present century. While Franklin's selection of years might have been questioned had he confined himself to this period, the book, in just over two hundred pages of text, ranges from seventeenth century beginnings to 1979. This chronological overextension requires the author to leave a number of significant themes undeveloped—the important question of ghettoization, to cite but one example. Finally, although only a minor irritant, Franklin occasionally allows his ideological predilections to creep into his narrative. Thus, black Philadelphians of the mid-eighteenth century, most of whom were by then third- and fourth-generation Americans, are referred to as "Africans" or, again, when he describes the black esthetic as unique!

These strictures should not detract from what the author has accomplished. On balance Franklin offers us a thoroughly researched, carefully documented, clearly written, and richly detailed account of the educational history of blacks in the City of Brotherly Love.

GEORGE A. LEVESQUE
Indiana State University

CARLETON MABEE. *Black Education in New York State: From Colonial to Modern Times*. (A New York State Study.) Syracuse, N.Y.: Syracuse University Press. 1979. Pp. xv, 337. \$18.00.

In this study of black education in New York state from the 1780s through the 1930s, Carleton Mabée succeeds admirably in providing "a fresh perspective on one of the most troubled areas of American democracy in our time" (p. xiv). Written with

grace and based upon impressive research, it treats black education within the context of an ever-shifting, but always complex, web of race relations and the changing legal and socioeconomic status of blacks. Among the significant questions discussed in this work are those related to the emergence and development of separate schools for blacks, the degree of control that they exerted over such schools, and the freedom of black educators to engage in protest activities. The treatment of these and other aspects of the black educational experience is perceptive, closely reasoned, and balanced.

Despite the persistence of antiblack prejudice, New York blacks were seldom without white allies in their quest for educational opportunities. Indeed, benevolent whites provided most of the education that blacks received during the four decades following the American Revolution. But black initiative, evident as early as the 1790s, steadily increased and involved nationally known figures such as Charles L. Reason, J. W. C. Pennington, Henry Highland, and Sarah Garnet. Far from being the "obsequious Uncle Toms that later generations imagined them to be" (p. 102), blacks employed a variety of tactics, from self-help and public appeals to protests and boycotts in pursuit of their educational goals. While ministers monopolized leadership on those school issues involving little conflict, Mabée found that proportionally more lower-status blacks led in school activities involving sharp conflict. In the nineteenth century blacks in upstate New York generally enjoyed greater educational advantages than those downstate. The notable exception was Brooklyn, where blacks early succeeded in acquiring a school and in exercising unusual control over it.

Although a law of 1841 presumably opened public schools to all children regardless of race, separate schools for blacks existed well into the twentieth century. But, as Mabée observes, ambivalence characterized the black response to the issue of separatism. While articulate blacks generally desired the right to attend white schools, few were "willing to immolate black teachers on the altar of school integration" (p. 212). The movement against segregated schools that originated upstate reached a climax downstate in the five-year campaign waged by Elizabeth Cisco in Jamaica. Her crusade paved the way for the Roosevelt law of 1900 that, contrary to conventional wisdom, did not entirely eliminate separate black schools. However, school desegregation, once begun downstate, was "carried farther there than upstate" (p. 225). An influx of blacks from the South thwarted the goal of a thoroughly integrated school system just as it appeared within reach. Ironically, southern blacks were attracted to New York "in part because of its relative freedom from the very segregation which their arrival helped to reintroduce" (p. 283). The battle against *de facto*

school segregation thereafter involved issues, tactics, and false starts reminiscent of an earlier era.

Mabee's volume is educational history at its best and could well serve as a model for future studies of Afro-American education.

WILLARD B. GATEWOOD, JR.
University of Arkansas,
Fayetteville

ALBERT CAMARILLO. *Chicanos in a Changing Society: From Mexican Pueblos to American Barrios in Santa Barbara and Southern California, 1848-1930*. Cambridge: Harvard University Press. 1979. Pp. xiii, 326. \$17.50.

Albert Camarillo has written an excellent monograph dealing with the urban Chicano community. He forcefully and convincingly argues that Anglos subordinated Chicanos economically and politically in order to foster the development of California, exploiting minority group members as a pool of cheap labor in times of economic growth and casting them aside in times of recession and depression. People of Mexican descent were a useful adjunct for capitalism only if they were on the lower rungs of the socioeconomic ladder. This monograph provides empirical data to substantiate the thesis advanced by other scholars such as David J. Weber, Leonard Pitt, and Juan Gómez-Quíñones. The author concentrates on Santa Barbara, California, and makes comparative references to other southern California towns. Scholarly examination of this traditionally neglected group fills an important void in the history of the American West since 1848.

In the aftermath of the American military conquest, Mexicanos-Chicanos could not maintain their hegemony for long. Economic depression, fraud, and outright theft caused the loss of most Mexicans' cattle ranches. The Anglo majority, drawn to the area by tourism, agriculture, and related economic developments, gave rise to nativist politicians who stripped Chicanos of their remaining political power through gerrymandering and persecution of political dissidents. This displacement was carried out as part of a campaign to "Americanize" the area. By 1873 Chicanos were limited to unskilled and semiskilled jobs, principally in tourism and agriculture. Residential and social segregation into barrios developed at the same time.

These developments set the stage for the reception of large numbers of Mexican immigrants in the twentieth century. Patterns of discrimination in wages, housing, education, employment, and nativistic outbursts from groups such as the Klu Klux Klan helped to sustain and intensify the earlier nineteenth-century patterns. After 1930 local Anglos employed public agencies to rid the area of many Chicanos and Mexican immigrants.

Throughout his study Camarillo details the internal dynamics of Chicano society in response to change. Within the barrio these "foreigners in their own city" (p. 76) formed mutual aid societies, maintained whatever parts of their culture allowed them by the imperious dictates of law and economics, and sought continuing self-identity through family life. The many efforts at self-help through strikes and political pressure usually resulted in few gains because of the lack of allies, including Anglo-dominated labor unions. There is an admirable analysis of the sometime tension between native californios and newly arrived immigrants.

This is a model study. It is well written, closely argued, and employs a wide range of relevant printed, oral, and manuscript sources. It is a lasting contribution to the new social history in general and Chicano scholarship in particular. One anxiously awaits the author's comparative study of other urban areas. Those interested in ethnic history and western economic development would do well to consult this monograph.

LAWRENCE A. CARDOSO
University of Wyoming

MYRON BERMAN. *Richmond's Jewry, 1769-1976: Shabbat in Shocktoe*. Charlottesville: University Press of Virginia, for the Jewish Community Federation of Richmond. 1979. Pp. xxii, 438. \$12.50.

MARC LEE RAPHAEL. *Jews and Judaism in a Midwestern Community: Columbus, Ohio, 1840-1975*. Columbus: Ohio Historical Society. 1979. Pp. x, 483. \$19.50.

There was a time, not more than a generation ago when American-Jewish urban history was political-economic and elitist at the best, a local who's who of financial backers at the worst. Since then professional historians have become the writers of most urban histories, and statistical, quantitative methods have been introduced so as to include the 85-90 percent of the population who were not part of the elite. The two books under review present the history of two medium-sized Jewish communities of ten to twenty thousand people, but there their similarities end: Myron Berman's *Richmond's Jewry* is anecdotal and institutional whereas Marc Raphael's study on Columbus, Ohio, is representative of the latest interdisciplinary model, sociological as well as historical.

Berman includes twenty-one illustrations of people, buildings, and so forth in his work but no statistics; Raphael includes ninety-seven statistical tables as well as many photographs and other illustrations. Berman's narrative style, always anecdotal and often disjointed, presents the two-hundred-year

story of Richmond's Jews; the Spanish, German, and East European phase; and ultimately the fusion of all these elements in the modern American Jew, native-born, college-educated, middle-class, proud of his heritage, and aware of his responsibilities for Israel. Assimilation and conversion took their toll especially among the early colonial families, so much so that none of their descendants of today are still Jewish. Even the good Biblical Jewish name Mordecai was denatured into Mor-de-kée (p. 119)!

Edward Nathan Calisch (1865–1946), the twelfth graduate of the recently organized Hebrew Union College, was to serve Richmond's reform temple for fifty-five years. In the mold of classic reform, from which Calisch did not deviate during all these fifty-five years, Americanism and the public image of the Jew became the watchwords of his career. Acceptance of the peoplehood of Israel—as in so many other southern reform congregations—had to await the passing of the old generation and the absorption of the German-Jewish elements by the children of the East-European immigrants. Berman's narrative identifies personalities, institutions, and trends of a southern community; he does not provide real critical insights and clues for perceiving the role played by Jews in the story of the South and in their own symbiosis with modern America.

Raphael succeeds in these respects in his analysis of Columbus, Ohio, and our historical and sociological understanding is carried a step further. The study reflects the spirit of the "new history" interested in the lives of ordinary people as much as that of the elite, in wide-ranging analogies and comparisons rather than often only trivial episodes. (Richmond's mayor assured a certain family that a road commemorating their grandmother would not be renamed [Berman, p. 267]; Raphael lists 1932–33 sermon titles of two rabbis in tables 48 and 49.)

Sociological, economic, political, and demographic data drawn from state and federal census schedules, tax lists, school and court records, business and city directories as well as newspapers, synagogue, and federation files have become the tools for quantifiable analysis and extensive comparisons with data from other communities. Much of this raw material is presented in the almost one hundred statistical tables by Raphael and his research team; this reviewer regrets, however, that too often no explanations for these mathematical constructions are provided (for example, what is "N" in Tables 86 and 87?). Table 29 enumerates the Fulton Street School First Graders in 1911–12, but why are the other grades omitted? In spite of these minor criticisms *Jews and Judaism in a Midwestern Community* constitutes a major breakthrough in the writing of Jewish local history in America.

FRANK ROSENTHAL
East Los Angeles College

JEFFREY S. GUROCK. *When Harlem Was Jewish, 1870–1930*. New York: Columbia University Press. 1979. Pp. xi, 216. \$15.00.

Before its renown as the black metropolis, Harlem, as a remote district of New York City, was an ethnically diverse area inhabited by Irish, Germans, and Jews mainly of German origin. The extension of mass transit lines to Harlem from the 1890s stimulated building activity to a feverish although irregular pace, and these processes in their turn made for an area densely settled by Italians, some blacks, and especially by Russian Jewish immigrants, most of whom had recently moved there from the Lower East Side. In 1900 Harlem was already the home of some 17,000 Russian Jews, and their number boomed to 178,000 in 1918 and declined somewhat to 168,000 in 1923 before it dropped to near the vanishing point in 1930 when no more than 5,000 remained.

Jeffrey S. Gurock's excellent study distinguishes two segments within Harlem Jewry. One consisted of newly middle-class East European Jews as well as the older German stock, who were merchants, professionals, and white collar workers. The other segment was composed of immigrants who had left the Lower East Side because they could find slightly better housing and employment in the uptown neighborhood. They worked at crafts like carpentry, painting, and tobacco manufacture, as well as petty trade. (Garment workers, who worked long hours in an industry concentrated far downtown, rarely were able to live so far from their work as Harlem.) Gurock argues that the establishment of this "Second Ghetto" was less a matter of upward mobility than the outcome of extensive demolition on the Lower East Side, which forced thousands of residents to move away. Their area was a stronghold of socialism, trade unionism, fraternal societies, and little orthodox synagogues. There were few large, but many small, strikes and a great deal of militant consumerism aimed against offending butchers, bakers, and landlords.

Gurock regards the "Second Ghetto" as merely transitory, for the future lay with the prospering, upwardly mobile Jews of Central Harlem. Here, the new and the old Jewish stock accepted each other and cooperated willingly for communal purposes. They established modern Jewish schools, community centers, and institutional synagogues, all directed to harmonizing Jewish tradition with American life.

If their efforts mark the beginning of American Jewry's evolution into its contemporary forms, as Gurock argues, he should have examined those institutions somewhat more closely. One would also wish for greater attention to the inter-ethnic relations and for fuller exposition of the swift decline of

Harlem Jewry. It seems exaggerated to say that the World War I housing shortage that ruptured the "residential equilibrium" within Harlem brought about this decline. Did mass black settlement force out Jews and other whites, or did blacks enter en masse behind departing Jews and other whites? One would welcome a more definitive statement. Regrettably, no comparison is made with other cities, (except Chicago—the sociologists' model) or even with other areas of New York City.

Gurock's work is thorough and exact, its many details always serving the larger picture he draws. He employs press sources, census schedules, real estate literature, and local ephemera. Although his maps are too schematic to be useful, the index, typography, and photographs are good. Gurock's literary style, while readable, oscillates between the clear and the ponderous. But these are minor qualifications concerning a book that is a significant, admirable contribution to New York City and American Jewish history.

LLOYD P. GARTNER
Tel-Aviv University

DAVID D. LEE. *Tennessee in Turmoil: Politics in the Volunteer State, 1920–1932*. Memphis, Tenn.: Memphis State University Press. 1979. Pp. xvi, 204. \$15.00.

This brief but informative book details the changes that took place in Tennessee politics between 1920 and 1932. David D. Lee demonstrates that the election of Austin Peay to the governorship in 1923 set in motion a shift of power from the legislative to the executive branch of state government. Peay's increased control of expenditures produced a much-needed program of improvements in education, roads, and parks and a modernized and more equitable system of taxation.

Although Peay was a strong leader who kept corruption to a minimum, after his death in 1927 the newly centralized government of Tennessee fell into the hands of a political machine dominated by publisher Luke Lea and businessman Rogers Caldwell. The machine's control seemed absolute until 1931 when its leaders were accused of misusing state funds in an attempt to bail out Caldwell's failing business ventures. The collapse of the Lea-Caldwell faction coincided with the ascendance of a more durable political organization, that of Edward H. Crump of Memphis. Peay's efforts to centralize and improve state government had actually created the foundation for machine rule in Tennessee.

Lee's summary is valuable, but his conclusions raise some questions about the assumptions underlying his research. He characterizes the 1920s as a period when the "sibling processes of urbanization and industrialization were gradually creating a new

power base at the expense of the farmers and rural-oriented small town merchants and professionals" (p. 3). Thus, he describes Austin Peay's "business progressive" program of modernized government and expanded public services and facilities as one designed to "meet the needs of an increasingly industrial, urban-oriented society" (pp. xii–iv). Lee's interpretations seem logical enough until he demonstrates that Peay's policies were "demanded, supported and enacted largely by the people from the farms and small towns" while "major urban areas provided the most determined opposition" (p. 152). Lee surmises that urban voters objected to being taxed for better roads and schools in the countryside but makes no effort to explain why a program he describes as urban-oriented actually appeared to be of greater benefit to rural areas.

Lee's apparently contradictory interpretations may be related to the assumption that urbanization and industrialization enjoyed a "sibling" relationship in Tennessee. As of 1930 the rural South was still nearly three times as industrialized as rural portions of the manufacturing belt. Lee chronicles the growth of industry in major cities but offers no evidence that Tennessee's overall pattern of industrialization was more urbanized than that of the South as a whole. In fact, though it remained sizable, the percentage of the state's manufacturing employees residing in large urban counties decreased slightly between 1919 and 1929. Lee assumes that farmers and small-town merchants were the backbone of Peay's support, but a program that promised improved transportation and better-educated workers largely at the expense of big-city taxpayers must also have been appealing to industrial interests based in the state's rural and small urban counties.

Closer attention to the actual distribution of industrial development in Tennessee might have enabled Lee to present a more plausible interpretation of the events he describes. Similar attention to the locational patterns of economic development in other states might assist historians in understanding not only business progressivism but also the persistent rural influences that shaped the South's politics and culture long after its economy became less dependent on its farms than its factories.

JAMES C. COBB
University of Northern Iowa

GARY B. OSTROWER. *Collective Insecurity: The United States and the League of Nations during the Early Thirties*. Lewisburg, Pa.: Bucknell University Press. 1979. Pp. 287. \$17.50.

Despite a title that promises a good deal more, this slender monograph deals essentially with the relationship of the United States to the League of Na-

tions during the Manchurian crisis of the early thirties, to which all but one of its chapters is devoted. It thus traverses ground already well covered in Christopher Thorne's *The Limits of Foreign Policy: The West, the League and the Far Eastern Crisis of 1931-1933* (1972) and Armin Rappaport's *Henry L. Stimson and Japan, 1931-1933* (1963). The final chapter touches on the American decision to join the International Labor Organization in 1934 and on the unsuccessful attempt to join the World Court in 1935.

The juxtaposition of these events is intended to demonstrate the existence of "an internationalist impulse . . . within the Department of State and among an important segment of the politically articulate public" (p. 199) but does not do so convincingly. Even Stimson, as Rappaport had already shown and as Gary B. Ostrower now confirms, was "internationalist" only sporadically, and his feints at cooperation with the League fell far short of anything that might indicate a commitment to collective security. Cited signs and portents, such as the efforts in 1933 to provide for regular contacts with the League through reorganization both in Washington and Geneva, were largely confined to internal memoranda that produced no actual results.

The evidence here and elsewhere indicates, in fact, that the state department was divided on most relevant issues and that so-called internationalism reared its head only fitfully in the statements and actions of Stimson, Hornbeck, Castle, and other major protagonists. It does little to substantiate the claim, made at least for the late twenties, that "(a)n isolationist public partially handcuffed an internationalist State Department" (p. 55), a claim that Ostrower himself ultimately dilutes to the vanishing point by noting that within the department were "strong countercurrents that, at certain crucial points, checked both internationalism and the evolution toward a collective security system" (p. 199) and that "(t)he issue of public opinion is likewise less clear than it has often seemed" (p. 202). Indeed, the most that could possibly be claimed is that the state department demonstrated a streak of activism that at times resulted in a modicum of international cooperation. In relation to the League, Stimson and the others played it safe, "so safe, in fact," as Ostrower himself points out on one occasion, "that many League members were unable to determine where America actually stood" (p. 152).

The book is not helped by the author's penchant for clichés and for meaningless words and phrases. What is "a willingness to semicooperate" (p. 53), "the most humane and non-Tory of Conservatives" (p. 71), or "relatively small changes made outside the public eye" (p. 167)? How does one act in "a bold yet cautious manner" (p. 102)? And what do we know after we are told that "all pleas fell on deaf

ears. Stimson therefore ate his cake and had it too" (p. 100)?

Ostrower has clearly done his homework and mined the relevant sources, often to good effect. But his central proposition is essentially a weak one, and his style does nothing to disguise that.

MANFRED JONAS
Union College

IRWIN F. GELLMAN. *Good Neighbor Diplomacy: United States Policies in Latin America, 1933-1945*. (John Hopkins University Studies in Historical and Political Science, 97th series, number 2.) Baltimore: John Hopkins University Press. 1979. Pp. xii, 296. \$18.50.

On the threshold of the 1980s a major university press has chosen to publish a work that ignores critical scholarship of the last two decades. Suddenly issues so painfully raised by William Appleman Williams, Lloyd Gardner, Robert F. Smith, James Petras, Andre Gunder Frank, and others no longer exist. By a miracle of evasion the reader is thrust back into the era of Fourth of July history (New Deal version). The Roosevelt presidency, Irwin F. Gellman solemnly writes, was "the golden age of Pan American cooperation" (p. 227). Roosevelt himself was not merely president of the United States but a "citizen of the Americas" (p. 209). Gellman even revives the inaccurate cliché that subsequent presidents have "neglected the Western Hemisphere, with tragic consequences" (p. 228). To be sure, he makes no effort to substantiate the latter charge. His story ends within three months after Roosevelt's death. He then merely lists resignation dates of "leading" Roosevelt-era policy makers, as if a turnover of personnel proves discontinuity of policy.

The problem is not that Gellman has failed to do his homework; for the book contains a mass of data. The difficulty lies in the way he organizes and interprets his material. What is one to make of an analysis dismissing the Inter-American Bank in two paragraphs (p. 160) but devoting two pages to Sumner Welles's homosexuality (pp. 177-78)? Gellman seems unable to assess the relative importance of, or indeed the relationship between, personal and structural factors. His characters live in a world unconstrained by socioeconomic structure or internalized political assumptions, limited only by that phenomenon described as "the depression" and a troublesome legacy of "interventionism." This endows them with both more and less freedom of action than they actually had. For a far more sophisticated approach, the reader should consult Lloyd Gardner's *Economic Aspects of New Deal Diplomacy* (1964), a work mentioned briefly in Gellman's footnotes but the import of which seems to have escaped him.

Gellman's preoccupation with personalities leads him in some odd directions. On one hand, he offers a gratuitous lampoon of Hull's speech impediment; on the other, his uncritical acceptance of testimony contained in interviews granted him by the late Nelson Rockefeller blinds him to Rockefeller's own role in ensuring policy continuity after Roosevelt's death. But then, that continuity Gellman cannot afford to recognize anyway since it wreaks havoc with his thesis.

In his summary of Roosevelt's contribution to the Good Neighbor Policy, Gellman observes that the president's economic understanding was "limited" (p. 227)—an ironic accusation considering Gellman's own failure to integrate economic issues. Not once in his discussion of the fall of Grau San Martín and the subsequent abrogation of the Platt Amendment in Cuba does he even mention sugar (pp. 36–37). He recognizes the "political" nature of wartime loans to Latin America, but never relates these loans to postwar planning or to issues of permanent peacetime economic strategy (p. 162). He entirely neglects U.S. efforts to exercise veto power over Latin American national development corporations and simply dismisses crucial economic negotiations at the 1945 Chapultepec Conference as "disappointing" (p. 207). Beyond this, Gellman's insistence upon personalities leads to a sharp oversimplification of Latin American history. In discussing 1944 state department policy toward Argentina, he writes: "Hull never realized that Perón thrived and rose to power because of the secretary's keeping Argentina in a state of flux through his inconsistent actions" (p. 195). Argentine historians will laugh this interpretation out of court. The 1944 Argentine political situation was infinitely more complex than this simplistic approach can allow.

Finally, Gellman's book begins and ends with an inaccurate and pompous assertion that other writers have "virtually ignored" his subject and that he is therefore breaking new ground. Other writers have not ignored the Good Neighbor Policy; Gellman has merely found it convenient, indeed necessary, to ignore them. Were this book to be taken seriously, it would set back the cause of scholarship by at least twenty years.

DAVID GREEN
University of Saskatchewan

MILLARD L. GIESKE. *Minnesota Farmer-Laborism: The Third-Party Alternative*. Minneapolis: University of Minnesota Press. 1979. Pp. ix, 389. \$15.00.

While the presence of a workable third party is unusual in American politics, it was not totally unexpected in Minnesota when the Farmer-Labor Party

emerged. Aiding its development was a strong tradition of agrarian discontent, progressivism, and trade unionism. In detail Millard L. Gieske chronicles the successes and failures of the party from its inception in 1918 until its final merger with the Democratic Party in 1944. In the process, Farmer-Laborites won state and national offices and replaced Democrats as the principal opposition to Republicans. But the third party experience was troubled with constant internal schism and failed in becoming a national party.

In this solid study, based on thorough research in manuscript collections and newspapers, Gieske emphasizes the origins and structure of the party, electoral patterns, ideological differences, and personalities in the movement. Initial F-L success came in the 1920s with the election of Senators Henrik Shipstead and Magnus Johnson. These victories were in the "moderate" and "farmer" wing of the party, as exemplified by Shipstead, an independent politician who "deemphasized partisanship and collectivist ideology" (p. 139) and later became a Republican. In Gieske's assessment, support of public ownership distinguished "left" from "moderate" in the party (both groups left of most Republicans and Democrats).

F-L Party appeal peaked during the depression years when Governor Floyd B. Olson became the movement's unquestioned leader. While Olson espoused certain radical views, Gieske argues convincingly that he was mainly a "pragmatic liberal" (p. 145) who endorsed the New Deal and downplayed the leftist 1934 F-L platform. But F-L patronage problems, Olson's death in 1936, charges of Communism and anti-Semitism, foreign policy issues, and the appeal of liberal Republican Harold Stassen spelled defeat for the party. The 1938 election, including the bitter Elmer Benson-Hjalmar Petersen primary fight, signaled the decline.

Gieske's study is informative on Minnesota's Democratic Party weakness and gradual revival during the era, with periodic overtures for DFL merger, consistent Irish-American candidates, and an intraparty split. Discussion of complex internal F-L organizations (for example, the F-L Association) is helpful. Election tables are good.

The book does have shortcomings. There is some repetition, a pre-occupation with labeling every individual "left" or "moderate-conservative," and the narrative style is a bit heavy. Ethnicity, particularly Scandinavian appeal and the dominance of Scandinavian-American candidates, and F-L presence on the Iron Range and in Duluth could have been discussed in greater depth. Oral history might have been expanded and publications by James Shields, James Weinstein, and James Youngdale noted. Charles Lindbergh, Sr., did not support Johnson in 1923 (p. 78).

On balance, *Minnesota Farmer-Laborism* is a significant contribution in understanding F-L history and becomes a principal source on the party. In Gieske's judgment, the movement's major contribution was not its third-partyism or economic theories, but "the tradition of effective leadership" (p. 172).

BRUCE L. LARSON

Mankato State University

DONALD L. MILLER. *The New American Radicalism: Alfred M. Bingham and Non-Marxian Insurgency in the New Deal Era*. (National University Publications Series in Political Science.) Port Washington, N.Y.: Kennikat Press. 1979. Pp. viii, 240. \$17.50.

Donald L. Miller's account of the life and times of Alfred Bingham advances the lesson Frank Warren drew almost fifteen years ago in his examination of Bingham's independent reform thought: "If historians are ever to reach the core of liberalism—1930's variety—it is necessary that they become more than peripherally interested in the liberals whose commitment was to principles rather than to an administration." Noting that efforts to discern principled liberalism outside the New Deal have fixed on general ideas, rather than the lives and actions of particular reformers, Miller seeks to add insights that can come only from close examination of an activist. His story unfolds as a morality tale about a Connecticut Yankee aristocrat, turned Galahad, who never finds the Holy Grail of "New American Radicalism" and must then devise a measure of consolation and new purpose.

Bingham was most provocative in his emphasis on the middle class as the locus of a "New Radicalism." He insisted that America was an "exceptional" case where there was no fixed lower class to revolutionize, only an aggregate of have-nots striving to achieve the middle-class stature that they believed was their proper lot. Those who did have middle-class status had greater revolutionary potential, for they were losing their security and were ripe for outrage at the ever-dwindling elite that was coming to own most of the property. This conception owed much to Veblen's theory that status-seeking was more important than class loyalty and to Berle's and Means's findings in *The Modern Corporation* (1932) of the growing concentration of wealth.

Had Bingham aligned himself in the beginning with the New Deal, he might have gained some influence and authority—even a niche in government. Such an establishment stance would have set him up as a critic who presaged the postwar revival of Tocqueville's view of American middle-road consensus. Instead, Bingham chose radical opposition to the New Deal and exposed himself to deadly fire from all sides. Yet, as Miller astutely describes,

Bingham's futile efforts to promote a New Radicalism based on middle-class support for a socialized society helped reveal the inherent nature of many of the more influential movers and shakers who opposed him. By seeking a coalition with the left, for example, Bingham's farm-labor group moved the Marxian socialists to show that they would not cooperate when they could not control. On the other side, Bingham's carefully detailed case for middle-class democratic planning rebutted proclaimers of an elitist "managerial revolution," like James Burnham, and so provided continuity between early hopeful visions of an economy of shared abundance and present-day movement, under the press of scarcity, toward some sort of equitable planning. And, finally, Miller's focus on Bingham amidst Deweyan hopefulness helps explain how the triumph by Lewis Mumford, Reinhold Niebuhr, and other "realists" over "naive" faith in essential goodness was Pyrrhic. It led, not to the expected era of greater humane and esthetic sensibility but to existential gloom in the absence of anything as constructive as the old pragmatic zest for social experimentation.

Because he remained true to his ideals, Bingham faced the coming of war and the end of reformism with a regretful sense of failure. And yet, as Miller shows with cogent analysis, Bingham's ideas have retained value by providing unique perspective on problems in American life that have never been resolved.

ALAN LAWSON

Boston College

JOHN F. STACK, JR. *International Conflict in an American City: Boston's Irish, Italians, and Jews, 1935-1944*. (Contributions in Political Science, number 26.) Westport, Conn.: Greenwood Press. 1979. Pp. xiv, 181. \$22.50.

John F. Stack's laws governing ethnic conflict stress the battleground of rhetoric, the importance of isolated ethnic enclaves or absence of residential contiguity, and the value of international issues that foster group consensus against a common foe. He uses these criteria to explain the interethnic antagonisms among Irish, Italian, and Jewish Bostonians during the decade 1935-1944. He collects evidence from newspapers such as *Gazzetta del Massachusetts*, the *Boston Pilot* (Roman Catholic archdiocesan publication), and the *Jewish Advocate*. He relies heavily on the dissertation of Charles Trout, "Boston During the Great Depression 1928-1940" (1972) and John Diggins's *Mussolini and Fascism, the View from America* (1972).

In describing the support of Jewish liberals for Roosevelt's domestic program, he ignores their criti-

cism of Roosevelt's policy on Jewish refugees. Stack credits Father Coughlin's antisemitic, anticomunistic, and profascist Spain policies with the coalescing of Irish opinion. But he mentions, only in passing, the socioeconomic factors of the Irish Bostonian working class. And he does not consider the possible appeal of Coughlin's policies to the profascist Italian Bostonian working class. Stack also assumes monolithic support for Mussolini among Boston's Italians. He fails to examine the role of internal conflict among Italians spearheaded by labor leaders Luigi Antonini of the ILG, Frank Bellanca of the ACWU, and antifascist Italian exile Gaetano Salvemini, who spent these years teaching history at Harvard.

Stack accepts uncritically the autobiographical, intuitive study of Italian Americans by Richard Gambino and the clever asides of Andrew Greeley describing Irish-American behavior as documentary evidence. Throughout the book he mentions ethnic conflict between Boston's Italians and blacks, Irish and Jews, Italians and Jews, but he provides no specific, detailed examples. Stack's attempt at a complex analysis of ethnic group behavior responding to international issues while beset with the problems of depression, local politics, and aspirations of mobility becomes entangled in rhetoric and never gets beyond its veneer to a more critical interpretation of ethnic popular opinion and behavior.

JEAN SCARPACI
Towson State University

STUART E. KNEE. *The Concept of Zionist Dissent in the American Mind, 1917-1941*. New York: Robert Speller and Sons. 1979. Pp. xiii, 268. \$12.95.

Today's overwhelmingly pro-Israel sentiment within the American Jewish community often obscures the fact that Jewish nationalism was a minority viewpoint prior to the Holocaust. While many American Jews casually sport buttons or bumper stickers proclaiming "I Am A Zionist," even today less than one-sixth of American Jewry belongs to organized Zionist bodies, and the number was far smaller in the days before World War II. Thus a study of anti-Zionist and non-Zionist attitudes in the United States would be a welcome addition to the growing body of literature on American Jewry and the development of a pro-Zionist consensus.

Regrettably, Stuart E. Knee's book fails to fill this gap. While it purports to deal with opposition to Zionism, the reader may have great difficulty in discerning the precise meaning of his terms. For undisclosed reasons, Knee seems to have adopted a Weizmann-Labor Zionist program as the normative form of the movement, although Weizmann and the Labor factions were not that closely

aligned, and Labor Zionism never commanded more than a small minority among Zionists in this country. Against this supposed norm, though, Knee asserts that the Brandeis-Mack group, advocating an Americanized version of the movement, should be seen as similar to antinationalist Jews such as the assimilationists in the Reform movement or Protestant sects with ties to missions in the Middle East.

The author has gathered a great number of quotations but fails to impose an orderly analysis on his material. He apparently takes his evidence uncritically and at face value, without placing it in context. Moreover, disturbing factual errors undermine the reliability of the work. He suggests, for example, that "less than ten percent" of America's 3.3 million Jews were enrolled in Zionist organizations in 1914; in fact, that figure should be one-third of one percent, and even in 1919, at the height of the Brandeis group's power, only some 6 percent paid the shekel. He declares that Weizmann snubbed the Americans by not inviting their participation in the Palestine Commission in 1918 (p. 124). Just the opposite is true; Weizmann begged Brandeis to join the Commission or at the very least to designate some Americans to go with him, but Brandeis steadfastly refused on the advice of the state department. Similarly, Knee's discussion of Reform attitudes would have benefited enormously had he seen David Polish's fine book, *Remember the Days*.

There is much material in this book, but scholars wishing to use it must do so carefully and selectively. We still await a study of anti- and non-Zionist attitudes in this country.

MELVIN I. UROFSKY
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ZVI GANIN. *Truman, American Jewry, and Israel, 1945-1948*. New York: Holmes and Meier. 1979. Pp. xvi, 238. Cloth \$24.50, paper \$12.50.

Scholars of American-Middle East relations will be both pleased and disappointed by various aspects of this book. On the positive side of the ledger, Zvi Ganin's use of material from previously unavailable sources enhances his work significantly. Particularly commendable is his ability to draw relevant information from the Foreign Relations of the United States series as well as state department records relating to Palestine.

In large measure because of Ganin's reliance upon these recently released primary sources, this volume represents the clearest and best-documented account of the state department's effort to block the establishment of a Jewish state in Palestine. Another strength of the book is its thoroughly researched and thoughtful account of the American Zionist organizations' lobbying role to promote sup-

port for a Jewish state on the part of the United States government. A major thesis of the book is the author's contention that the Zionist organizations in this country have been given far too much credit for bringing the American government around to a position of support for Zionist goals.

One major figure in American Zionism, Rabbi Abba Hillel Silver, comes under particular scrutiny. Cochairman of the American Zionist Emergency Committee (AZEC), Silver is generally acknowledged to have been a crucial figure in making American Jewry a potent political force. Ganin demonstrates, however, that Silver so alienated President Harry Truman on a personal level that he diminished not only his own effectiveness but also that of AZEC.

Ganin, who teaches at Beit Berl College in Israel, is less successful in analyzing Truman and his pattern of careening back and forth between support and nonsupport for the Zionist cause from 1945 to 1948. Although the author ascribes humanitarian reasons to Truman's ultimate commitment to Zionist aims, he is totally unable to substantiate his claim that it was Truman's concern for displaced Jews, and not political considerations, that was the motivating factor in Truman finally supporting pro-Zionist positions.

Truman's dramatic recognition of the state of Israel only eleven minutes after it came into existence occurred during a critical moment in the president's uphill election campaign of 1948. Ironically, instead of dispelling the thesis that Truman operated largely within a political context when he supported Israel, the evidence presented by Ganin strongly reinforces such an analysis.

The author displays little understanding of the American political system, especially how the system can function during a presidential election year. Truman was influenced, writes Ganin, "by the strong support . . . large segments of public opinion had given to the Zionist cause" (p. xv). Actually, polls indicated that the Palestine issue was not one in which the general public took much interest. Yet Ganin misses the key point that within the presidential electoral system small minorities—Jews in this case—located in closely contested states with large electoral votes, can easily swing an election. Since at that time no significant opposition to a pro-Zionist policy existed in American politics, the president had nothing to lose politically, and possible victory to gain, by supporting the demands of American Jewish voters.

A serious flaw is the book's partisan slant. Ganin is long on understanding Zionism's supporters and short on understanding its opponents. Typical of Ganin's slant is his remarkable understatement when he refers to how the Arabs viewed the creation of Israel. A Jewish state in Palestine, Ganin

writes, "seemed to involve an injustice to the Arabs" (p. xiv) [emphasis mine]. King Abdul Aziz Ibn Saud, ruler of Saudi Arabia, is described as a "fanatic Moslem" (p. 111); no one is referred to as a "fanatic Zionist" or a "fanatic Jew." Similar examples abound.

In some areas the author simply does not have the evidence to back up his claims. For example, Ganin challenges the traditional interpretation of the role Arabs had in trying to influence Truman when he says that the president "was subjected to steady, strong, and effective Arab pressures" (p. 110). The problem in this instance is that Ganin fails to present even the slightest evidence that would prove this controversial point.

JOHN SNETSINGER

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RICHARD S. TEDLOW. *Keeping the Corporate Image: Public Relations and Business, 1900-1950*. (Industrial Development and the Social Fabric, number 3.) Greenwich, Conn.: JAI Press. 1979. Pp. xx, 233. \$26.50.

Richard S. Tedlow has written the best and most complete history of public relations, the vocation and the corporate policy. With wider chronological scope, greater concern for the political environment of business, and extensive use of recent secondary works, his book surpasses and supplants the reviewer's own earlier work. As a useful addition to American business history, *Keeping the Corporate Image* should be the first reference for those interested in the history of PR.

Despite obvious merits, the book offers surprisingly little new interpretive insight, however. Tedlow follows earlier explanations for the emergence and growth of corporate PR as both a defensive reaction against political threats and labor unions and an institutional search for order, rationalization, and efficient organization in the era of bigness and impersonalization. Not an apologist for corporate policies or the PR specialists, Tedlow joins scholars who emphasize the diversity and transformation of business opinions and practices, and he criticizes radical scholars who impose a consensus where none existed. His own stand toward PR he calls ambiguous.

Examining the half century of the vocation, Tedlow correctly points out its limitations and pretensions. PR specialists exaggerated their manipulative power over public opinion and their influence within corporate policy-making, the "two-way street." Most PR specialists identified too closely with their employers' interests to serve as impartial advocates of substantive reforms, and those few who tried usually failed. PR acquired, according to Ted-

low, only an "essentially peripheral influence" (p. 201) within corporate policy-making, mainly concerned with the consciousness and style of publicity. Nevertheless, he is unwilling to discount entirely the vocation's so-called reforming influence on business and suggests that, in any case, it probably did contribute to the American public's acceptance of the corporate system in the twentieth century.

At just that point, when he moves beyond the history of the vocation, his book is more ambitious, and less satisfactory. The main problem is not lack of concern for context but improper perspective and proportion. While admitting that PR's influence was peripheral, Tedlow examines the transformation of business opinions and practices as expressions of what he calls public relations-mindedness. Thus he explores activities of such business organizations as the NAM and the CED, but on those subjects the book is too derivative and superficial. (His account of the CED's position on the Full Employment Bill, for example, ignores the evidence of Stephen Bailey's standard work, *Congress Makes a Law*.) More importantly, focus on a nebulous public relations mindedness just will not illuminate the transformation of basic business policies regarding prices, wages, unions, quality of products and services, consolidation, and competition. Conversely, without proper emphasis on those basic issues, the reputation of business and the effectiveness of publicity cannot be adequately explained.

Whatever the book's limitations, it certainly deserves better from the publisher than numerous typographical errors, inadequate footnotes, and an unhelpful index.

ALAN RAUCHER
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WARREN JAMES BELASCO. *Americans on the Road: From Autocamp to Motel, 1910-1945*. Cambridge, Mass.: MIT Press. 1979. Pp. ix, 212. \$14.95.

Seldom does one confront a book, not to say an excellent book, in which the author begins with expressions of dissatisfaction with his chosen point of departure. "Sharing recent fears that cultural history may have relied too heavily on purely literary and intellectual concerns," Warren James Belasco writes near the end of his preface, "I saw the motel as a revealing interface between business and culture, a material embodiment of tourist dreams of community, independence, equality and spontaneity. . . . [Yet] like the car, the motel did not fulfill the dreams that created it. Thus my journey ended with a feeling of frustration, a sense that for all this movement basic needs remained unsatisfied. Maybe motorists took the wrong path."

Belasco, at any rate, after dallying with numerous points of departure, will be found to have taken the right path. His focus, which grows stronger as his narrative develops, is on what large numbers of Americans actually did *with* their cars, rather than what cars did to America.

"Gypsyng" is what Belasco calls the first stage, the "squatter anarchist" stage, of the automobile vacation on the road. One of the thoughtless characteristics of this vacation mode, one that grew ever more common and ever more objectionable to residents along the way, was the gypsies' practice of abandoning garbage and other litter as they drove off. This abuse spread as "gypsyng" grew with the growth of the automobile business and ultimately called for local government control.

Free municipal campgrounds built and maintained by towns along major roads proved a satisfactory alternative until town-and-town competition sharpened and budgets ballooned. Rising municipal costs soon led to the imposition of fees on the campers, a practice that promptly engaged the attention of private capitalists to the business of accommodating highway travelers.

Business competition, in turn, soon centered on the improvement of sites, service, and facilities for those able to pay. "Thus," Belasco writes, "the motel industry was born" (p. 4), and with it what one auto camper called "hoboing de luxe."

During the business boom of the mid-1920s, when sanguine capitalists, in Belasco's words, "tired of carrying equipment," cabin operators added good beds, good linen, stoves, and indoor plumbing to their facilities. The "great crash" of 1929 and the ensuing long depression, rather than harming the business of these enterprises, only improved it by drawing to them many of the erstwhile well-to-do now willing to forego their normal grand hotel life. By the mid-1930s, trailers had become very popular on the road, reviving the practice of "gypsyng" while promoting at the same time the rapid development of trailer camps.

Space does not permit any elaboration here of Belasco's grasp of all these innovations and tendencies and their deep social impact, but it is continuously evident nevertheless. As scholarly books go, his is a short one yet comprehensive enough to do full justice to the many facets of his subject. His readers will look forward to more from the same pen.

WILLIAM MILLER
West Redding, Connecticut

FREDRICK J. DOBNEY. *River Engineers on the Middle Mississippi: A History of the St. Louis District, U.S. Army*

Corps of Engineers. Washington: Government Printing Office. 1979. Pp. vii, 177. \$7.50.

As a long-time participant in the U.S. Army's military history program, I am well aware that organizational histories of that service are sometimes less than satisfying. It was a pleasure, therefore, to read this rather slim monograph, which covers the history of the St. Louis District of the U. S. Army Corps of Engineers, and find it both visually pleasant and intellectually stimulating.

Frederick J. Dobney, a history professor at St. Louis University who accomplished the project under contract, has chosen the chronological approach to tell his story. And he tells it well. He begins by briefly recounting the formation, exploration, and settlement of the Mississippi Valley but quickly turns to the importance of the river in the growth of our nation, with emphasis on the influence of the St. Louis environs.

In pursuing his chronological approach, Dobney weaves the strands of natural phenomena (snags, regularization of the channel, flood control) with federal response and interest in the area. The latter was largely determined by political and economic factors. Throughout the narrative, we catch fleeting glimpses of the organizational machinery that governed engineer efforts. Ironically, given the subject of the volume, the organizational coverage is the weakest. We gain little knowledge of the reasons for organizational change or the relationship of the St. Louis District to Washington or to its sister districts.

The Great Depression and World War II brought new initiatives for the district, and the environmental concerns of the past two decades have added new responsibilities and problems.

The author has relied primarily on a wealth of secondary material, including the annual reports of the chiefs of engineers. Copious illustrations, both art and photographs, enhance the text. Although it is good to see what all the district engineers looked like, four of them on a page are sometimes too many. And it is disconcerting to see these officers anachronistically depicted as generals or West Point cadets without explanatory captions. The author includes all sorts of interesting data—commercial tonnage on the Mississippi and Missouri since 1824, steamboat accidents and fatalities, steamboat arrivals at the port of St. Louis. The index is brief and weak.

Is Dobney, a hired hand, objective in presenting the story of the St. Louis Engineer District? I think so. The most sensitive area is the most recent one, and his coverage is even handed. The author acknowledges the organizational conservatism of the Corps of Engineers, but this is tempered by a tradi-

tional tendency to adapt to the prevailing values of society. After all, the new Engineer slogan, The Corps Cares, did not emerge from a vacuum.

BROOKS KLEBER

U.S. Army Training and Doctrine Command

JOSEPH G. KNAPP. *Edwin G. Nourse—Economist for the People*. Danville, Ill.: Interstate Printers and Publishers. 1979. Pp. 544. \$11.95.

Appointed by Harry Truman in 1946 as the first chairman of the Council of Economic Advisors, Edwin Nourse confronted the pitfalls facing the economist practicing his profession in a political milieu. Nourse served on the Council only three years before he resigned his post because he was unwilling to adjust to the politics involved in formulating economic policy. Since then, the Council has experienced varying degrees of success in influencing economic policy.

Nourse will be remembered primarily as the first chairman of the Council and secondly as an agricultural economist and director of economic studies at the Brookings Institution before he entered government.

Nearly half of the book is devoted to Nourse's early life and career before coming to the Council. Joseph G. Knapp discusses Nourse's involvement in the cooperative farming movement and in the development of agricultural economics as a profession in the 1920s. Nourse's greatest work before entering government came during the New Deal when he undertook a major evaluation of the Agricultural Adjustment Administration and directed the Brookings Institution's influential four-volume study of the "capacity" of the American economy. Walter Lippman called this latter work the most useful economic study made in America during the depression.

The second half of the book is devoted to Nourse's work in the Council of Economic Advisors. Much of the material can be found in Nourse's *Economics in the Public Service* and in secondary accounts of the Council by Edward Flash and Hugh Norton. The author shows that Nourse carried into his work on the Council his firm belief in "non-partisan" economics and in the necessity of a balanced budget. Nourse made a subtle distinction between "advising" the President and "advocating" or defending current policies. He therefore refused to testify before the Joint Economic Committee; yet he advised Truman that the Marshall Plan was sound economic policy. Not enough discussion is offered, however, for the reader to judge the extent to which Nourse's attitudes were in fact political. Clearly Nourse advocated fiscal restraint as the best means

of controlling inflation, a policy considered outdated by liberals. In 1949, feeling his days on the Council were numbered following Truman's reelection, Nourse openly attacked the administration for fiscal irresponsibility. Truman accepted Nourse's resignation shortly afterwards. Although Nourse, like many other moderates, came to accept the huge budget outlays for defense that coincided with the Korean War, he continued to be haunted until his death by his fears of inflation.

Knapp undertook this project in 1966, eight years before Nourse's death in 1974, as a "monument to the memory" of a close personal friend. Knapp, who completed this work after retiring as an economist with the Department of Agriculture, based his research on extensive interviews with Nourse and his associates, as well as Nourse's published writing and some unpublished speeches. Little use was made of other available primary sources, however, especially those in the various presidential libraries, which have remained unexploited in other recent histories of economic policy as well. Knapp's study is rich in detail, although he frequently loses the interpretation in telling the story. The meaning of "economist for the people" is never explained. Another weakness is his failure to place Nourse in a larger context by describing the general economic conditions of the time or the views of other contemporary economists. Still, this first biography of Nourse tells the reader much and should fulfill the author's hope that his work will serve as a guide for further explorations into this relatively new historical field of public policy.

DONALD T. CRITCHLOW
North Central College

DARLENE CLARK HINE. *Black Victory: The Rise and Fall of the White Primary in Texas*. (KTO Studies in American History.) Millwood, N.Y.: KTO Press. 1979. Pp. xv, 266. \$19.95.

The intensive study of black history, so astonishingly fruitful for the eras of slavery and Reconstruction, is producing a growing list of works on the twentieth century. *Black Victory* is a modest, useful contribution to that list despite a major problem of focus. "The Supreme Court's path to this position [i.e., invalidating the white primary]," Darlene Clark Hine writes, "and the political and social significance of the 1944 *Smith v. Allwright* case [in which that was accomplished] are the central foci of this study" (p. xi). In spite of this statement and of the subtitle as well, the core of the book is an account of the effort, chiefly of NAACP leaders and lawyers, to mount a successful legal challenge to the white primary laws of Texas. The real focus is on the maneuverings within the NAACP and between

that organization and local blacks, especially in El Paso and Houston. Efforts to find satisfactory cases and defendants, to secure competent local counsel, to raise funds, and to see the cases through lengthy appeals are treated in some depth. The most original and important sections discuss differences over tactics and strategy between local blacks and the NAACP, including the desire of the Texans for more involvement in the cases and for greater use of black attorneys. Hine is especially effective in describing the tensions these differences produced, and in this her study supplements earlier work by her mentors August Meier and Elliot Rudwick. This facet of the subject has not previously been detailed, and Hine's doing so is the chief contribution of her study.

The book is marred, however, by its imperfect focus. Major facets of the subject central to Hine's purpose are poorly treated or treated not at all. Constitutional issues are insufficiently delineated, and the precise implications of the various cases are not always intelligible. The results are a distorted reading of some of the decisions and a tendency to overstate their significance. It is difficult to agree, for example, that "the Supreme Court had served as defender of black rights" (p. 174) in *Nixon v. Herndon* and *Nixon v. Condon*, since those decisions rested on such narrow grounds that they produced no meaningful relief for blacks. It is equally difficult to agree that *Smith v. Allwright* "had a profound impact on jurisprudence and American politics" and was "the watershed in the struggle for black people" (p. 233; emphasis added). Certainly Hine does not substantiate these judgments.

Nor does she tell us much about other key aspects of her subject. The white primary, Hine writes, was "the most effective of the disfranchisement subterfuges" (p. ix), but she offers no voting or registration data before and after its advent or comparative data about the impact of other subterfuges to test that judgment. Nor does she detail the actual operation of the white primary in Texas (some blacks in some counties, she notes only in passing, were always permitted to vote as Democrats); nor does she explain how and why black Texans, most of whom were Republicans until the mid-1930s, came to focus their political and legal attention on the Democratic primary rather than on other forms of disfranchisement. There is still much to be written about the rise and fall of the white primary in Texas.

I. A. NEWBY
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PATRICIA DAWSON WARD. *The Threat of Peace: James F. Byrnes and the Council of Foreign Ministers, 1945-1946*.

Kent, Ohio: Kent State University Press. 1979. Pp. x, 227. \$12.50.

Patricia Dawson Ward's *The Threat of Peace*, the latest volume in Kent State University's studies in American diplomatic history, is the first serious treatment of the diplomacy of Secretary of State James F. Byrnes in the Council of Foreign Ministers since the publication of George Curry's *James F. Byrnes* in 1965 in the prestigious *American Secretaries of State and Their Diplomacy* by Robert H. Ferrell and Samuel F. Bemis. Up until then, historians of United States foreign relations in the immediate postwar era had perforce to rely on Byrnes's own account of events in *Speaking Frankly* and *All In One Lifetime* published in 1947 and 1958, respectively. At the Potsdam Conference held in early July 1945, the United States, the USSR, and Great Britain called into existence a Council of Foreign Ministers (plus France and China where appropriate) to draw up draft treaties for Italy, Bulgaria, Rumania, Hungary, Finland, and, hopefully, Germany. Byrnes served as the principal United States representative on the council from the abortive London meeting in September 1945 until his resignation from the state department in January 1947. Within this time frame and within the space of 179 pages and 8 tightly-written chapters, including a concluding chapter—the rest of the book turned over to extensive notes, a skimpy bibliography, and an adequate index—Ward “details Byrnes’s attempt to harmonize the conflicting Soviet and American positions at the six conferences of the Council of Foreign Ministers held during 1945 and 1946” (p. x).

Faced with tasks beyond his talents, as ignorant of foreign affairs as President Truman, and generally unprepared, “Byrnes,” according to Ward, “went to the London Conference of the Council of Foreign Ministers with only a vague conception of what the United States wanted written into the treaties, but with the firm assumption that American military and economic power would allow the United States to dictate the treaties” (p. 174). There, Byrnes encountered the stiff resistance of his Soviet counterpart, V. M. Molotov, who made it clear that neither atom bombs nor dollars would or could deter the Soviet Union from attaining its status as a victorious great power replete with treaties sanctifying control of the Balkans, large reparations, a share in the control of Japan, and bases in the Mediterranean—to name but several goals. By the time of the Moscow Conference in late 1945, Byrnes had shed his “benevolently arrogant expectation” (p. ix) of what Washington could force Moscow to do and instead shifted to a new foreign policy tactic of quid pro quo negotiations. But while relatively successful with the Soviets privately—for example, token concessions in Japan for

token concessions in the Balkans—publicly Byrnes blamed the Kremlin for the breakdown in the Grand Alliance, thus feeding the growing demand in 1946 “to get tough” with the Russians. On the other hand, Ward finds Byrnes himself guilty of diplomatic incompetence, for while it is true the secretary was instrumental in helping to realize the peace treaties (less Germany of course), it is also true that he was responsible for encouraging “the impetus toward the cold war” (p. 179). And so yet another postwar American secretary of state must bear the burden of history for failing to recognize openly the “realism” and “legitimacy” of allowing the Soviet Union to dominate the bulk of the Eurasian landmass.

Methodologically speaking, *The Threat of Peace* exhibits more holes than a slice of Swiss cheese. For one thing, in her obsession with apportioning guilt, Ward, at her own peril, has replaced the traditional historicizing question, “How it actually was,” with the very different question, “How was it possible for it to come about?” Such an approach could only vitiate the historical worth of her effort. For another thing, Ward fails to adduce a scintilla of evidence that Soviet goals “were more realistic than those of the Americans, and [that] the treaties reflected reality” (p. 176). Of whose reality does she speak? Moreover, Ward cannot begin to prove that “Molotov manipulated the others [in the council] like a puppet master” (p. 176), which is not to say, however, that the Soviet foreign minister did not know how to play the puppet himself. Of course, the trail here leads straight back to Stalin, and he thought Byrnes, if the story in the *Henry Wallace Diary* can be taken as a guide, “the most honest horse thief he had ever met.”

In any case, the Council of Foreign Ministers still awaits its historian. Perhaps the best that may be said of *The Threat of Peace* is that it will one day encourage others to come to grips with this truly unique diplomatic experience in a more objective manner.

JOSEPH M. SIRACUSA
University of Queensland,
Australia

JUSTUS D. DOENECKE. *Not to the Swift: The Old Isolationists in the Cold War Era*. Lewisburg, Pa.: Bucknell University Press. 1979. Pp. 289. Cloth \$17.50, paper \$8.95.

Justus D. Doenecke's book bristles with insights about the persistence and permutations of American isolationism in the first decade of the Cold War. He surveys reactions by foes of American embroilment in World War II to a series of Cold War developments, among them Bretton Woods, the UN,

the 1946 British loan, atomic diplomacy, aid to Greece and Turkey, the Marshall Plan, NATO and the military assistance program, various German issues, the fall of China, the Korean War, and the 1954 Indochina crisis. The isolationists' role in the 1948 and 1952 elections, in the McCarthy controversy, and in the fight over the Bricker amendment is also detailed.

The isolationists, Doenecke believes, have much to teach us. Their objections to globalism, the growth of executive and military power, and policies provocative to the Soviets were often prescient. Doenecke defines isolationism chiefly in rational ideological terms. He is less impressed with sociological or geographical interpretations, but his approach is eclectic. He sees a strong anti-urban bias among isolationists and, in their efforts to restore the pristine virtue of a lost polity, a resemblance to Bernard Bailyn's revolutionaries. Doenecke examines both politicians and publicists; his focus is broad, intellectual as well as political.

Notwithstanding his respect for the old isolationists, Doenecke is remarkably critical of them. In many isolationists, dread of communism clashed with fears of interventionism to produce an unstable mix. Doenecke captures their erratic and inconsistent moments and the bizarre and phobic elements of their beliefs. Thus, William Lemke warned that with the Truman Doctrine Uncle Sam had deserted "beautiful Miss Columbia" to philander after "red, pink, green and off-colored skirts all over the world" (p. 81).

Doenecke has rescued the isolationists from the limbo to which the victorious interventionists consigned them. Partly he achieves this by demonstrating their perceptiveness, but, more interestingly, he shows how elements of their supposed heresy found their way into the mainstream of policy. As the Truman administration's German and Asian policies evolved, they came to resemble isolationist prescriptions, as did the nation's increasing resort to a defense based on air power and nuclear deterrence. In this sense, the defeat of isolationism was equivocal, and the label itself was inexact and transitory, as indicated by the fact that John F. Kennedy and Gerald R. Ford were once contributors to the America First Committee.

The author's analysis is cogent, balanced, and invariably suggestive. One might quibble that a more rigorous quantitative analysis of the isolationists in Congress would have been rewarding. Additional manuscript collections, including the papers of Norman Thomas, William Langer, and Karl Mundt, might have been profitably consulted. Then too, one of Doenecke's major achievements, the elaboration of the full spectrum of isolationist views, poses a problem: Senator Robert Taft and other potentates are juxtaposed with publishers of interesting

but little-read newsletters; the precise center of gravity of this motley group is hard to locate. However, these are minor criticisms of a well-researched, subtly argued, first-class piece of scholarship.

RICHARD M. FRIED
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Chicago Circle

JANE SANDERS. *Cold War on the Campus: Academic Freedom at the University of Washington, 1946-64*. Seattle: University of Washington Press. 1979. Pp. xii, 243. \$12.50.

In 1949, the Board of Regents of the University of Washington, rejecting the recommendations of a divided faculty hearing panel, dismissed three tenured professors for their avowed or presumptive membership in the Communist Party of the United States. An outgrowth of the revelations of the Canwell Committee (the first of a rash of state legislative inquiries into subversion that adopted the techniques of the House Un-American Activities Committee), this action by the Regents was followed by a series of aftershocks—repeated banning of controversial speakers from the campus rostrums, labeling of professors who belonged to so-called "front" organizations, appending of a disclaimer affidavit to the pledge of loyalty required of all employees—that roiled the university for decades and earned it the reputation as the place where academic freedom buckled under Cold War stress. Jane Sanders, who was a student and teacher at the university during the closing phase of its long ordeal, tries to fix the blame for the woes that befell her alma mater, relying on institutional records that had not heretofore been used for this purpose and on the detached perspective that is presumed to emerge from a lapse of time.

Only in a limited sense can it be said that she is successful. She does show, by delving into local history, that much of the hue and cry against subversives was prompted less by concern for the nation's security than by personal feuds and party conflicts—for example, the efforts of Dave Beck, vice-president of the Teamster's Union and a regent, to settle old scores with a radical labor enemy, Harry Bridges; the desire of certain Republican politicians to gain votes by stigmatizing their opponents; and the fierce ideological conflicts that had long been waged within the Democratic camp. She also shows, by quoting several presidents against themselves, that nothing stood higher in the administrative scale of values than securing the next year's appropriations and that they would veto acts of surpassing harmlessness, like asking J. Robert Oppenheimer to give a lecture, rather than give any jaundiced keeper of the purse offense. But aside

from adding colorful detail to the propositions that superpatriotism is the last refuge of petty scoundrels and that presidents of state universities are a cautious lot, the author contributes little to our understanding of these events. She offers no cultural or structural comparisons that would help us see why Michigan and Wisconsin, not to mention Chicago and Harvard, fared better than Washington or California in those dark times. She brings scant technical knowledge or paraphrastic skill to her discussion of the constitutional and legal issues: on this score, Vern Countryman's contemporary account of the Washington debacle remains a more useful, if truncated, work. And—a more serious fault—she greatly oversimplifies the moral and professional issues that were raised when American Communists in the forties and fifties pressed their academic freedom claims.

In the author's view, the chief responsibility for what went wrong rests with liberal academics on and off the campus—with faculty organs of opinion that split over the crucial question of whether Communists were fit to teach; with the builders of academic departments at UW who longed for national recognition and were willing to trade silence for supporting funds; and with the national AAUP, which was so torn over the Communist issue that it took six years to report on the Washington dismissals and thus forfeited the opportunity to carry weight. This failure-of-liberal-nerve hypothesis does not pass key empirical tests. The national AAUP did not depart from its early pronouncement that membership in the Communist Party should not in itself be grounds for dismissal; it failed to issue a prompt report for the same reason that it failed to answer its mail—its general secretary had become incapacitated but was able to fend off internal criticism until at last he was replaced. On the author's own evidence, those who acceded to oaths and inquisitions were not "cosmopolitans" but "locals"—the faculty of the schools of business, forestry, and engineering. Here, as doubtless elsewhere, the courage to stand up to local pressures was fanned by the ambition to stand high among distant peers. Finally, she misconstrues the character of the Communist Party in this period and thus passes judgments on its critics that are uninformed and overharsh. To treat the view that the party was a Russian tool as an example of the dementia of that moment, to write a book about the Cold War on the campus without once mentioning Stalin or Stalinism, is to revise, not recall, the past. To fail to see that civil libertarians could also be anti-Communist—that they could regard the party as totalitarian but not every member as totally committed to it or oppose incontinent exposure at home while resisting Soviet moves abroad—is to rule in favor of simple minds. And to hold the faculty culpable because it was not "unan-

imous" is both to minimize its persistent and ultimately successful fight for academic freedom and to demand a measure of conformity that negates what academic freedom is about.

WALTER P. METZGER
Columbia University

DONALD R. MCCOY. *The National Archives: America's Ministry of Documents, 1934-1968*. Chapel Hill: University of North Carolina Press. 1978. Pp. ix, 437. \$19.00.

In less modest moments, of which there are depressingly few, archivists claim to be as important to scholarly research as are bankers to the nation's economy: both hold the keys to the vault of resources. Donald R. McCoy's history of the National Archives and Records Service is particularly valuable to those scholars who rely on NARS for the resources that make their research possible.

Although McCoy apparently had the full cooperation of NARS, his product is not court history in the pejorative sense. He used NARS records, interviewed NARS personnel, and submitted his manuscript to NARS officials; but he does not hesitate to point out NARS flaws. Neither is the book institutional history in the derogatory sense. McCoy does not dull the senses with a dry narrative reconstruction of organization charts and statistics. Rather, he injects life into the volume by organizing his tale around the first four Archivists of the United States.

McCoy begins his account with the 1934 National Archives Act, which established the National Archives as an independent federal archival agency. The archives became part of the General Services Administration in 1949, where it was rechristened the National Archives and Records Service. McCoy concludes with the 1968 appointment of James B. Rhoads as Archivist of the United States, symbolizing for McCoy a new generation of institutional leadership. Coincidentally, the volume's publication preceded Rhoads's unexpected retirement by only a few months.

Three themes emerge of special importance to historians. The first concerns NARS's dual role. Is NARS an educational and cultural institution serving the public's right to information about its democratic government? Or does NARS serve the federal government's desire to minimize the huge cost of federal records creation, storage, reference, and disposition? Although the National Archives was established primarily for the former reason, its executives quickly found that the latter justification generated more funds from Congress. McCoy argues that NARS should continue to fulfill both functions, albeit independently of the General Services Administration.

A second theme of direct consequence to historians is restriction of access to federal records. NARS originally acted as a neutral observer of this process, simply passing on the bad news from agency to researcher. McCoy credits this tack with allowing NARS to persuade federal agencies to turn over their records, not an easy task in the early years of NARS existence. He also acknowledges that researchers were slow to bring pressure on NARS to negotiate better bargains with the agencies. As researchers have become more assertive in recent years, NARS has become a more forceful advocate of researcher access.

The third theme is the leading, though intentionally muted, role NARS has played in the development of the archival profession through the Society of American Archivists and its journal, *The American Archivist*, and the International Council on Archives, with a journal currently in gestation. This nonstatutory contribution to scholarship has improved research opportunities in archives throughout the United States and around the world.

MARTIN I. ELZY

Lyndon Baines Johnson Library

MARK H. ROSE. *Interstate: Express Highway Politics, 1941-1956*. Lawrence: Regents Press of Kansas. 1979. Pp. xii, 169. \$14.00.

That the "highway lobby" was influential in promoting American road building comes as no surprise to anyone. That this lobby contained a variety of special interests that differed on many basic points and that the resulting confusion and disagreement effectively deterred legislation for a national system of highways is, however, something new—and the subject of this book.

The greatest pressure for roads came from an explosion in the numbers of motor vehicles that no government could long ignore. Particular groups also touted highways, but they hardly agreed on purposes and methods and often reflected local biases and narrow prejudices. Farm organizations concentrated on rural market mileage, planners were interested in shaping and reshaping cities and accomplishing a variety of social objectives, truckers wanted to increase profits and minimize costs, and engineers were concerned almost solely with traffic movement and efficiency. Economists looked first to whatever promised the greatest economic growth.

Commissioner of Public Roads Thomas H. MacDonald's proposal for the 40,000 mile national interstate expressway system was included in the Federal Highway Aid Act of 1944 but was not allocated a portion of the roads budget. As elements of the highway lobby feuded over construction priorities and methods of financing—tolls, bonds, increased user taxes—the project seemed doomed to endless

controversy. Lucius D. Clay, tapped by President Eisenhower to coordinate a new federal road-building program, struggled to forge a consensus with no greater success.

The Interstate Highway Act of 1956, introduced by Congressmen George Fallon and Hale Boggs, finally succeeded because it satisfied the long-term goals of every major highway interest group (except, perhaps, farmers), required few sacrifices, did not drain the Federal budget, and avoided difficult issues. This legislation was not, Mark H. Rose emphasizes, the product of constructive give-and-take among the highway interests themselves. "References to compromise, at least in highway politics," he writes, "served as functional myth" (p. 89). The act provided for 90 percent Federal financing, a Highway Trust Fund, and no substantial new taxes on truckers or other highway users. Thus was the biggest public works project in human history begun.

This extensively researched, brief, and important study adds to our knowledge of interest group politics and the impact of the motor vehicle in the United States. While hardly the final word on all aspects of the subject, it provides a useful framework for further investigation and contains especially interesting information on urban highway planning. It also reminds us that assessments of the impact of technology that omit economic interests and political decisions from the list of variables are exercises in futility. Ironically, at no time, by any group, was the cost and availability of petroleum mentioned as a significant issue.

BLAINE A. BROWNELL
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CANADA

DOUGLAS MCCALLA. *The Upper Canada Trade, 1834-1872: A Study of the Buchanans' Business*. Toronto: University of Toronto Press. 1979. Pp. viii, 231. \$15.00.

This lucid and valuable case study, based on company records and a wide and varied range of primary sources, traces the rise and fall of an important wholesaling firm based in Glasgow, Scotland, and in Hamilton, Upper Canada, during the prime period of the commercial and economic growth of what was to become the province of Ontario. Douglas McCalla's name is already well known in Canadian business and economic history circles through his constructive contributions in the field and through his able commentaries on such recent works as that of Tom Naylor. This book provides a detailed and able analysis of a key firm in its line—

precisely the type of groundwork study that is needed before attempts are made to write a general history of Canadian business enterprise.

The Buchanan brothers, Peter and Isaac, launched into the Canada trade in 1830, after disappointments in the failing West India trade. Their line, operating first in Montreal, was essentially the wholesaling of dry goods imported from Britain, but Isaac, the younger brother and manager at the Canadian end of operations, resolved earlier that Upper Canada was the more promising venue of operations and moved to York (Toronto) in 1832 and, in 1834, to the then frontier area of Niagara and Hamilton, which was to become his hub. A man of "vision and enthusiasm," he was energetic and full of ideas for the development of the area. As an "ideas man" he was soon heading the local business community, building up an extensive clientele among storekeepers and settlers over a wide area, and making handsome profits averaging 30 percent on the initial capital of £25,000 that included, in characteristic Scottish fashion, a "cash credit" of some £5,000 from the Glasgow Banking Company. Traveling constantly, winter and summer (he disliked office routine), he built up his business, attributing the success of the firm to picking Scotsmen as his customers, distrusting "Yankees," and accepting the "Canadian born" only "after the most vigorous scrutiny." The Buchanans, in fact, acted as business consultants to their storekeeper clients, and in these early years of the 1830s and 1840s their profits increased steadily. Unfortunately, the ebullient Isaac became involved in politics and in the Protection issue, in railroads, and in other diverse and time-consuming activities. Chapter 3 ("Success in Toronto") presents a fascinating picture of the firm's working methods and of the manner in which Upper Canadian trade at city, town, and country levels was carried on. Isaac's belief in his own judgment led to further ventures into ironware and pig iron importation and into tea, tobacco, and other importations through New York. The fact that the firm had made a sixfold increase in its capital by 1845 may have seemed to justify this—the company was the biggest wholesaler in Canada West by this time—but speculation in Upper Canadian wheat and involvement in the promotion of the Great Western Railroad in the early 1850s sapped away the partners' time, energies, and attention to their basic business of wholesaling dry goods. By the crisis of 1857 the firm lacked liquidity of funds, and far too many of their storekeeper customers were irrecoverably in debt to them.

McCalla skillfully links these booms and recessions of the time to the firm's changing fortunes. The portrayal of Isaac's character, problems, and essential weaknesses is balanced and judicious, albeit sympathetic, for this was an *impulsive* though creative businessman. The sad and tangled tale of

the final bankruptcies and of the attempts at reorganization is well told. The promising town of Hamilton lost ground as railroad trade confirmed the position of Toronto and Montreal as Canada's main importing and distributing centers. As the Upper Canadian wheat-growing boom faded in the 1860s, more specialized trading firms replaced the older style of Buchanans' wholesaling. Isaac's "perennial optimism" was no counter to the more effective and tightly controlled credit management of his rivals.

Throughout the analysis there is a clear exposition of how and why decisions were made, while the competitive conditions, the business and economic climates against which they were made, are clearly shown. There is a whole series of useful, well-designed tables recording sales, shipments, and profits for most of the period. Altogether this is a precise, perceptive study of a great firm's rise and fall.

DAVID S. MACMILLAN
Trent University

BRYAN D. PALMER. *A Culture in Conflict: Skilled Workers and Industrial Capitalism in Hamilton, Ontario, 1860-1914*. Montreal: McGill-Queen's University Press. 1979. Pp. xviii, 331. \$10.95.

A book like this shows why the new labor history has become an emotional subject. Bryan D. Palmer tries to describe and explain the culture of the skilled workers of Hamilton, Ontario, in the decades prior to World War I. He intends to show how and why they struggled for control over their lives, homes, and workplaces and how they evolved ideologically. The book gives us a fuller picture of these workers than we have yet had, along with a useful summary of the course of industrial conflict in southern Ontario during these years. Palmer's analysis (done with Peter Warrian) of the evolution of reform thought in the Hamilton area is also valuable. This book can teach us much about skilled workers in Hamilton.

But Palmer does not leave it at that. As a practitioner of the new labor history, he believes the only way to analyze the actions and attitudes of workers is to study their culture. Here the book is weakest. Palmer's discussion of working-class culture, for example, tries to show how the "associational" life of Hamilton workers contributed to a sense of solidarity. He describes friendly societies, drinking clubs, and sports teams that had workers as members, but he cannot describe the extent of their participation. He admits that membership data are "extremely rare" (p. 41). Aside from two societies he "can only speculate" (p. 42) about the extent of worker participation. This is even true of the Mechanic's Institute. When discussing charivaris and whitecapping, Palmer admits that participants in such events "do

not lend themselves easily to identification." Even so, he goes on to exclaim that "it would be strange indeed if a crowd of 300 young men, girls and boys gathered on Hamilton's street corners on a Sunday afternoon contained no workers" (p. 65). Palmer thus presents no real evidence that most of the culture he describes is any less that of the banker or the shopowner.

So, too, with his treatment of control. The "culture of control" (p. 71) was a culture of "autonomous workers," he asserts (p. 222). These were "manly" artisans (p. 85) smoking and drinking their leisure time away (p. 91), controlling their pre-industrial work environment. But there is no proof that the authority of the owners stopped at the shop door. Even then employers had the basic power to hire and fire and to set the size of their workforce. Palmer describes many battles fought over control issues, but closer examination shows they were almost always not control but economic struggles. Palmer admits this (p. 91) but claims that these struggles formed a context for more traditional control battles. We are left to wonder if Palmer has arbitrarily dispensed with economism in favor of control as a general category into which all strikes and lockouts should somehow fit.

In two places Palmer generalizes too much. The discussion of reform thought in Hamilton is excellent. But Palmer (and Warrian) then claim, without proof, that this was the reform thought of the whole "Canadian working class" (p. 122). Palmer later asserts that class unity had developed in Hamilton by 1914 (p. 227) but then shows in the following pages that this unity was only that of the English skilled workers.

This book could have used a strong editorial hand. Phrases like "matrix of control mechanisms" (p. 128); "contours of the conflict" (p. 130); "synthesis that transcended the fragments constituting the whole" (p. 154); and "web of legitimization" (p. 212) abound.

The new labor historians are dedicated to rescuing the working people from obscurity. This book does move in that direction. But Palmer's evidence to support key parts of his analysis is thin, and his book contains too many generalizations. Surely the rescue work should begin with old-fashioned empiricism.

DAVID J. BERCUSON
University of Calgary

LATIN AMERICA

SHERBURNE F. COOK and WOODROW BORAH. *Essays in Population History*. Volume 3, *Mexico and California*. Berkeley and Los Angeles: University of California Press. 1979. Pp. xiii, 333. \$20.00.

This last volume in their series, *Essays in Population History*, closes a quarter century of fruitful collaboration between Woodrow Borah and the late Sherburne F. Cook, a rich collaboration that has made groundbreaking contributions to the demographic and socioeconomic history of Spanish America, provided methodological models and techniques for historical demographers, and opened up heated historical debate. This book should do the same.

In the first of their three essays the authors attempt to establish the low point for the Indian population of New Spain in the seventeenth century. Relying on new documentation on royal revenues and tribute assessments in the Audiencia of Mexico for 1646 uncovered by E. William Jowdy, Cook and Borah compare this new count with those already extant for 1568 and 1595. Establishing ratios for the Indian population in *pueblos* found in ten regions of Mexico documented in all three counts, they calculate that the indigenous population fell to its lowest ebb ca. 1620-1625, the adjusted average date for tribute assessments found in the 1646 document and a "reasonable compromise" for the low point of the Indian population. They conclude that ca. 1620-1625 approximately 730,000 Indians lived in the Audiencia of Mexico or 3 percent of the population in 1519.

The second essay is a remarkably informative piece on pre- and post-conquest Indian diet and food production, an analysis demanding digestion and an integration of literature on nutrition, ecology, physiology, biophysics, and a number of other disciplines. Admittedly tentative—but convincing—their findings show that the pre-Columbian Indian had a meager but diverse diet of maize, *frijoles*, squash, chiles, *chía*, *hualli*, agave, cactus apples, ducks, dogs, turkeys, algae, and perhaps human flesh. Per capita daily intake based on the average height, weight, and metabolic rate of an adult Indian male between fourteen and forty (159 centimeters, 53.6 kilograms, and 1,425 kilocalories) was somewhere between 1,400 and 1,800 kilocalories daily, sufficient for sustaining a good deal of physical effort but considerably less than the consumption of most European and American laborers today. Based on production of 700 to 1,200 kilograms of maize per hectare, only 10 to 15 percent of the land available needed to be under production to feed the population in 1519.

The last essay demonstrates how much can be gleaned from the registers of eight California missions and sets up models of what might be done also with the registers for the other fifteen. Among other things Cook and Borah provide information on ages at baptism, life tables for specific cohorts, ratios of birth to adult females, population statistics on non-Indians, and data on racial mixing for the late eighteenth and early nineteenth centuries. They conclude also that the Hispanic residents of the Alta

California missions were "as robust, as long-lived, and as fecund as the *habitants* of French Canada."

As in all their collaborative work, these two thoughtful, innovative scholars meticulously lay out their assumptions and methodology; explain their sources in detail; crosscheck with other data wherever possible; and candidly assess the degree of reliability of their conclusions. Based primarily on documents, the first and third essays will probably elicit little debate, but the second, once again justifying high estimates of the Indian population in 1519, will open up new controversy, but controversy that should bring us closer to historical reality, the overwhelming goal of all of Cook and Borah's research.

JOHN J. TEPASKE
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THOMAS L. KARNES. *Tropical Enterprise: The Standard Fruit and Steamship Company in Latin America*. Baton Rouge: Louisiana State University Press. 1978. Pp. xv, 332. \$20.00.

Central Americanists have long craved a solid history of the activities of the transnational banana companies in Central America and the Caribbean. They have particularly hoped for a study of the most important and most controversial of these firms, United Fruit. United Fruit remains a somewhat mysterious and controversial factor in Central America, but in the meantime Thomas L. Karnes's *Tropical Enterprise*, the best historical study of the banana industry yet to appear, provides considerable food for thought.

The focus of the book is on the Standard Fruit and Steamship Corporation and its principal predecessor, the Vaccaro Brothers and Company. It narrates the resolute efforts of the Vaccaro and d'Antoni families, starting in 1899, to establish themselves in Honduras for the purpose of selling bananas in New Orleans. It records the details of their success, despite the competition of the larger and more diversified United Fruit, in making Honduras the leading banana-exporting nation of the world by the mid-1920s, when the families incorporated as the Standard Fruit and Steamship Company. Although Honduras and New Orleans hold center stage, there are highly informative chapters accounting for the company's forays into Nicaragua, Mexico, and Haiti. The narrative closes with an account of Standard's overtaking of United in the post-World War II period and the negotiations and purchase of the company by Castle and Cooke in the mid-1960s.

Perhaps the most important contribution of Karnes's study is the light shed on the controversy about the banana companies' unwarranted interference in host country politics. However true it

may be in other cases, this accusation does not appear to apply to Standard Fruit. The Vaccaro brothers established a clearcut policy of neutrality toward Honduran politics and identification with the region of their operations; the company managed to make profits in Honduras no matter which party was in power and despite unstable political conditions. Furthermore, Standard expressly accepted the Calvo clause, prohibiting business requests for diplomatic intervention, and seemed little inclined to ask for aid from the United States government. The Standard formula, so successful in Honduras, did not work in Nicaragua, Mexico, or Haiti, but the reasons for the failure lie more in local conditions than in company policies. Karnes does not expressly weigh the benefits and costs of the presence of the banana companies in any of these countries, but his conclusion is clear enough. Summing up Standard's decision to pull out of Haiti after a history of harassment, Karnes states: "The tragedy is Haiti's" (p. 259).

Despite the successful narrative and its substantial contribution, it needs to be said that Karnes appears to be insensitive to some of the larger issues involved in the international business scene. Surprisingly, he ignores the dependency argument and literature altogether. Furthermore, there is a notable neglect of the many Central American studies critical of North American companies in general and Standard Fruit in particular. Company papers and United States archival material are the principal sources used—leading to the heavy focus on the company rather than on its impact on the host countries. The fact that there is no bibliographical entry later than 1974 helps to account for this weakness.

CHARLES L. STANSIFER
University of Kansas

MAGNUS MÖRNER. *Perfil de la sociedad rural del Cuzco a fines de la colonia*. Lima: Departamento Académico de Ciencias Sociales y Políticas, Universidad del Pacífico. 1978. Pp. xi, 186.

No other non-Spanish European historian has done more in recent years for the elucidation of social change in colonial Peru than Magnus Mörner. This thin but well-illustrated and carefully documented examination of a small area of the Viceroyalty of Peru during a period of approximately a century, from 1689–90 to 1786, is a testimonial to his continued interest in racial evolution and class consciousness in Spanish America. It is also indicative of a trend among Hispanicists toward microscopic scholarship in preference to the formulation of broad generalizations.

Why was the intendency of Cuzco selected for this investigation? The answer is multifold. Al-

though endowed with a smaller population and administratively less significant than some other areas, the region was strategically located on the route between Upper and Lower Peru. It was the ancient center of the Inca civilization. More important, there were ample sources available in the Archives of the Indies and in Peru. The author's aim is to conduct a demographic and economic analysis of two exceptionally full and generally reliable founts of census data, the reports, or *informes*, compiled for the bishop of Cuzco of the parishes of his diocese in 1689-90 and a set of *informes* assembled in 1784 at the order of the intendant of the district. Hence the treatment is mainly topical. The coverage is comprehensive, with chapters on population, haciendas, production, communication, and exploitation and rebellion.

Unquestionably, the author's conclusions expand our knowledge of diverse social and economic conditions in the late colonial and pre-independence eras. Population did increase, though slowly, in an economy marked by the interaction of the capitalist tenets of the *hacendados* and the "archaic, passive, and isolated economy of the Indian pueblos, direct successors of the Incaic ayllus" (p. 156). Surprisingly, mestizo proliferation took place as rapidly in the remote districts as in the more populous areas.

With respect to the great Indian uprising of 1780, the author's views are revisionist. Discounting somewhat the iniquitous labor institution of the *mita* and the unfair distribution of goods by the *corregidores* as general causes, he cites the depressed status of the system of transportation as a specific factor. In his opinion, and perhaps rightly, the loss of life in the region was less than traditional estimates, and the impact on the general economy was more complex than hitherto supposed.

Despite minor shortcomings, this is a valuable contribution to the history of colonial Peru. The inclusion of comparisons with contemporary conditions in other areas of the Spanish Empire might have disclosed similarities or contrasts. The omission of a table of contents and the lack of a satisfactory index are regrettable. One might wish that the appearance of this study would encourage the undertaking of other works of a limited geographical and chronological nature as a foundation for future syntheses.

J. PRESTON MOORE
Louisiana State University

SILVIO ZAVALA. *El servicio personal de los indios en el Perú (extractos del siglo XVII)*. Volume 2. Mexico: El Colegio de México. 1979. Pp. vii, 299.

Perhaps no problem is more central to an understanding of colonial Spanish American history as the persistent and agonized attempts of the crown

to successfully coerce Indian labor without simultaneously destroying the basis of native economy and society. Into this complex equation went the imperial perception of economic advantage and social justice, the aspirations of the conquerors and their descendants, the cold abstractions of demography, the dictates of Catholic morality (difficult for even the most subtle of casuists to circumvent), and the resistance, both passive and active, of the natives. Indeed, an entirely satisfactory solution to the problem was never arrived at, though it is safe to say that the conquerors profited more in the midst of their ideological vexation than did the conquered.

Silvio Zavala has devoted decades of valuable research to the problem, and his latest volume concerns seventeenth-century Peru, which witnessed an abortive effort to abolish the dreaded *mita* for the mines and other Spanish economic enterprises dependent on forced Indian labor. The scope and content of the work are made explicit by the title. This is an impressive compilation of extracts from archival sources and printed documentation and commentary, supplemented as appropriate by modern scholarship, on the changing status of what the Spanish chose to call the "personal service" rendered to them by the Peruvian Indians. Particular and welcome attention is paid to the supremely important silver-mining area of Potosí. No serious scholar of the period can afford to ignore a work that explores in such exquisite detail the efforts of a war-weary bureaucracy and theocracy to cling to the benefits of America's richest colony with consciences unscathed.

But a word of warning to the reader. Zavala has chosen to let his characters speak almost entirely for themselves; to the extent that Peru's achievements and ills during the seventeenth century are analyzed, we rely on no one but the participants.

FREDERICK P. BOWSER
Stanford University

JESÚS CHAVARRÍA. *José Carlos Mariátegui and the Rise of Modern Peru, 1890-1930*. Albuquerque: University of New Mexico Press. 1979. Pp. xi, 247. \$14.95.

Peruvianists in this country and elsewhere have awaited the publication of this book for many years, and, happily, they will not be disappointed. Although there are a number of studies of the life of José Carlos Mariátegui and his influence on Peruvian and Latin American thought, none equals in depth and comprehensiveness this volume by Jesús Chavarría.

Chavarría begins with a cogent analysis of the economic and political changes that occurred in Peru, 1870-1919, and then moves to a treatment of the impact this period of modernization had on Peruvian intellectuals (for example, Manuel González

Prada, the lawyer-professors at San Marcos University, and the "Generation of 1900"). The author is at his best when dealing with changing intellectual currents, but he is equally adept at explaining economic and political events.

In the first two chapters, Chavarria succeeds in providing the reader with a clear understanding of and feeling for the Peru in which Mariátegui grew up and began his career in journalism. The next two chapters trace Mariátegui's early working years and his increasing politicization that culminated in 1919 in his being exiled to Europe. Here, Chavarria is particularly effective when comparing Mariátegui's "Generation of 1919" to that of "1900" and analyzing the impact of European (primarily Italian) leftist political thought in the late 1910s and early 1920s on Mariátegui's political development. The section on Mariátegui's return to Peru and his involvement in the newly established *Universidades Populares de González Prada* is also well done.

The final four chapters treat the last seven years of Mariátegui's brief but incredibly productive life, and it is here that Chavarria makes his most important contributions to Peruvian historiography. Chapter 5 deals both with the founding and impact of Mariátegui's influential journal *AMAUTA* and with the initial friendship and then bitter conflict between Mariátegui and Víctor Raúl Haya de la Torre and the fledgling APRA party. Chapter 6 provides an excellent analysis of Mariátegui's monumental *Siete Ensayos de Interpretación de la Realidad Peruana*, while chapter 7 constitutes the most valuable critique available of Mariátegui's highly eclectic, revisionist brand of Marxism. In the last chapter Chavarria describes Mariátegui's radical *indigenista* nationalism and persuasively argues that it was this that really set Mariátegui apart from his contemporaries in Peru and from the international leftist movement in general.

In addition to being solidly researched, the book is well written with the result that Mariátegui's ideas are elucidated with a clarity all too rare in intellectual history. My only substantial criticism is that neither the notes nor the bibliography include citations to the more recent scholarship on Peru, but that should not detract from the importance of this book.

THOMAS M. DAVIES, JR.
San Diego State University

EUL-SOO PANG. *Bahia in the First Brazilian Republic: Coronelismo and Oligarchies, 1889-1934*. (Latin American Monographs, second series, number 23.) Gainesville: University Presses of Florida. 1979. Pp. xvi, 256. \$14.75.

Eul-Soo Pang has undertaken an analysis of a major area of Brazil in which he reveals how a system

of politics designed to accommodate various socio-economic models demonstrated resiliency under the impact of national and international change. Pang's work deals with the full complexity of factions, varieties of "colonels," and regional diversity that characterized the historical processes depicted. An ambitious, if not monumental, research effort was required to provide the nuances involved in this study. Although the book is primarily concerned with the years 1889-1934, other areas and periods are touched upon to round out the picture. One result of this study is the laying to rest of certain stereotypes and myths such as the image of the "colonel" as always being a landowner or the notion that land tenure was a predominant factor in *coronelismo*. The figure of Ruy Barbosa receives a well-deserved rectification as a politician dedicated primarily to self-aggrandizement.

It is in dealing with developments in the *sertão* region that Pang excels. When introducing factors from the national scene, he sometimes falls victim to some inaccuracy. The overthrow of the Franco Rabelo regime in 1914 provides an example. This incident was related to the "military salvations" that Pang sees as "rescuing northern politics from the hands of the ruling oligarchies that Pinheiro Machado controlled." The "military salvations" scheme was national in scope and reached its climax when the minister of war announced his intention to serve as "military savior" in Rio Grande do Sul (Pinheiro's home state). President (Marshal) Hermes was forced to choose between his military comrades and Pinheiro Machado for support. He chose the latter, and the showdown in Ceará occurred when the Acioli-Cícero forces (aided by Pinheiro) toppled the regime of "military savior" Rabelo. The Ceará incident was just another phase of Pinheiro's victory over the military saviors. Pang sees it as some type of defeat for Pinheiro with his prestige "reaching a new low." Moreover, this cooperation between the national government and colonel-type forces possibly amounted to an antecedent to the phase of politics of the Old Republic that Pang characterizes as the "politics of presidents," which he believes began with the administration of Arthur Bernardes.

All in all, Pang has produced an admirable study that should be considered essential reading for all who would understand the evolution of that area of Brazil before, during, and after the period of the Old Republic.

ROBERT A. HAYES
Texas Tech University

ENRIQUETA E. MOLINE DE BERARDONI. *Historia de Marcos Paz: Desde sus orígenes hasta la creación del Partido, 1636-1880*. La Plata: Archivo Histórico de la Provincia de Buenos Aires. 1978. Pp. 184.

Although her work lacks themes or interpretation, Enriqueta E. Moline de Berardoni has assembled through extensive archival research intriguing data on the growth of the town and *partido* of Marcos Paz (48 kilometers southwest of Buenos Aires city). She thus provides us with a microscopic view of shifting patterns of land ownership and of rural change in the decades that preceded the great national economic boom of the 1880s.

Through eleven brief chapters the author traces the evolution of Marcos Paz from 1636, when the first land grants in the area were made, to 1878, when the *partido* was created. In chapter 1 she describes the main geographical features of the zone encompassed by Marcos Paz and traces rapidly the major legal changes in its status through 1878. The remaining chapters chronicle the process of ever greater subdivisions of the land (chaps. 4–11), primarily between 1830 (the beginning of the Rosas dictatorship) and 1880 (the inauguration of Julio A. Roca's presidency).

In various sections the author gives us glimpses into political and economic matters, early town planning, and rural life. The heart of this work, however, lies in the author's painstaking reconstruction of land title changes. In 1636 land grants divided the future *partido* between Rodrigo Ponce de León and the Jesuit Order; by 1880 Marcos Paz had 87 proprietors. Through a combination of well-drawn cadastral maps and accompanying charts,

Moline de Berardoni summarizes vividly what took place over the intervening years. Equally valuable is her narrative description of the transfers themselves and the fate of individual owners.

The author has pursued the titles through complex and often confusing transfers, as with the fate of the Jesuit lands after the order was expelled in 1767 from Spanish America. In general, land ownership seems to have been subjected to capricious and haphazard events. Determining boundaries often produced court battles and political conflict among large landowners as nature eliminated old markers or surveyors made casual errors. Such problems sometimes inadvertently dispossessed *de facto* homesteaders from the land they occupied and worked. At times this process occurred through conscious action. Through it all, no pattern of land accumulation emerges. Instead, most would-be ranchers and *hacendados* eventually had to subdivide and sell their holdings, while others passed their estates on to relations who often mismanaged their inheritance and sold parcels to keep out of debt.

Unfortunately, the author's failure to place her work within a larger context limits its utility. For those readers familiar with Argentina's past, however, this exercise in local history should prove to be most interesting.

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

BARBARA FINKELSTEIN, editor. *Regulated Children/Liberated Children: Education in Psychohistorical Perspective*. New York: Psychohistory Press. 1979. Pp. 225. Cloth \$15.95, paper \$6.95.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

I would like to make a few remarks about James A. Henretta's article, "Social History as Lived and Written" (*AHR*, 84 [1979]: 1293-1322), and particularly about his references to French methodology, which were not, I think, fully appraised in the comments of either Darrett B. Rutman or Robert F. Berkhofer, Jr. Nobody could seriously deny the need for a little more philosophizing within social history's rank and file. Therefore, what a pity it is that one of the rare attempts to satisfy this need should misrepresent thoughts from a country that has probably contributed, in the last two generations, more ideas about the nature of social history than any other.

Here are what I find to be the most misleading parts of the article. First, it is difficult to see how the *Annalistes*, by whom I presume is meant the contributors to *Annales: Économies, sociétés, civilisations*, represent a "coherent intellectual system." The United States has no monopoly on intellectual anarchy where historical writing is concerned. There is no one single method, no one theory, no one ideology that dominates the pages of this journal. In fact, it is interesting to note how great an influence English-speaking historians have on *Annales*, in terms both of works cited and of articles contributed. The variety of works published in *Annales* surely belies the existence of an *Annales* school.

Second, many would be surprised to see Fernand Braudel's historical world classified as a variety of French structuralism (*à la* Claude Lévi-Strauss); while I think I understand Professor Henretta's reasoning, I believe it is deceptive. The chronology implied in the article is wrong. Braudel's *La Méditerranée* was first published in 1949. Ferdinand de

Saussure's linguistics, it is true, were formulated at the beginning of this century, but they only became current knowledge in the 1950s. Likewise, the full impact of Lévi-Strauss's structural anthropology was not felt until the late 1950s and early 1960s. It is not possible that Braudel "proceeded to apply this analytic framework to the history of the vast Mediterranean" (p. 1298)—the Mediterranean had already been braudelized. Analogies between Braudel and Saussure or Lévi-Strauss are superficial; an approximation of terminology does not prove an approximation in epistemology. Furthermore, the famous three-part schema that shapes *La Méditerranée* is neither the only nor even the predominant perception of time found in *Annales* today. Perhaps a discussion of Bergson's flux of time would have been more pertinent. But, again, there is no single school of thought here.

Third, Saussure's opposition between *langue* and *parole* is no longer seriously entertained by linguists today. Specifically, the sort of social contract that the idea of *langue* represents (whereby all members of a linguistic community submit themselves, en bloc, to an unchangeable, *static* framework of rules) has given way to the more contemporary appreciation of linguistic competence (in which memory becomes a *functioning* system of rules rather than an idle depository of signs). Linguists seek to establish a system of rules from which an infinite number of phrases can be generated—the creative capacity of language knows no limits. Thus, if language be its model, there can be no basis whatsoever for Professor Henretta's claim, repeated almost verbatim by his commentators, that French structuralism is "static" and "pessimistic."

Fourth, Professor Henretta was right in setting Michel Foucault up against nineteenth-century notions of unilineal causation, but he was mistaken in citing him as an example of structuralist thinking—at least, if he is to be consistent. Once more, Henretta has confused terminology with epistemology; Michel Foucault comes from an entirely different tradition. If Henretta really intended to demonstrate that "the concern with power and change

clearly differentiates the Marxist analytic framework from that of the later *Annalistes*, with its static assumptions, its causal diffuseness, and apolitical stance" (pp. 1305-06), he should have steered clear of Foucault. Foucault's works on asylums, prisons, and sexuality demonstrate a central concern with the relationship between knowledge and power or, if you will, the problem of the political effects of scientific discourse. The vocabulary might be lacking, but Foucault himself has recently shown that this was also a major concern of his *Archeology of Knowledge* (see his "Verité et pouvoir," *L'Arc*, 70 [1978]). It is also worthy of note that Foucault has most vehemently denied ever having been a "structuralist."

Fifth, I find Professor Henretta's proposed fusion of Marxist phenomenology with the "objective" structuralist viewpoint, along with the opposition he made earlier in his article between "rigorous statistical proof" and "individual motivation," quite admirable. But he has opened himself up to the charge from some sectors—I am thinking primarily of those "more materialist-oriented Marxist scholars like Louis Althusser"—of subjectivism and its various ideological escorts. Personally, this would not bother me one bit, but I fear Henretta will get short shrift from those scholars of high moral purpose whom he so obviously admires.

All of the problems I have indicated, save the last, are the result of the widespread tendency among historians to throw, when they scent the slightest similarity, batches of different ideas, methods, and philosophies into the same bread-basket. Perhaps this tendency results from the universities' demand for survey courses of ludicrous breadth, their lack of language requirements, or the publishers' demands for books of extraordinary scope. I am wholeheartedly sympathetic to Professor Henretta's call for more thought in this new, exciting field of history. But I am afraid we are going to be hearing about the static, pessimistic *mentalité* of the *Annalistes* for some time to come. It is a shame.

GEORGE DALLAS
Princeton, New Jersey

TO THE EDITOR:

I have just received a copy of the review of my book *The Defence of British Trade, 1689-1815* (*AHR*, 83 [1978]: 1006) via my publishers and should like to reply. The review contains a great many errors of fact and understanding, and a detailed response is called for.

In the final paragraph George F. Steckley stated, "A map of the Baltic contains none of the place names mentioned in the text." But page 74 names Riga, Stockholm, Danzig, and Gothenburg, all of which appear on the map. I accept, however, that "Tonnage" in the title to Table 6.2 should read

"Number." "Market value" is no more accurate a term than "real value"; as the market varied, so did the price. "Commerce" is never used as an adjective; at times it is used as a compound noun—that is, commerce defence. My sentence construction is grammatically correct, and no other reviewer has found the book hard to understand. On this basis, I cannot accept any of Steckley's final paragraph except for one small point.

Turning to the opening paragraph, Professor Steckley apparently believed that my book covers the seventeenth century. In fact, it includes no more than the last eleven years of it. His remarks about piracy thus have little relevance, since they refer to the period before my book starts. The last remaining pirates were rounded up in the final years of the seventeenth century, as I noted on page 176. Steckley repeated the old myth about "Barbary pirates." In fact, they were privateers—that is, private warships licensed by the state. There is a fundamental difference; pirates by comparison acted outside the law and were hanged when caught. Steckley's assumption that England's trade with the countries bordering on the Mediterranean was under effective control of the Barbary corsairs is also wrong. England possessed a strong navy and defended its commercial interests; other nations were less fortunate.

In the second paragraph Professor Steckley stated that "a chapter on insurance sketches the early history of Lloyd's." In fact, the chapter is about *marine* insurance and describes in some detail the development of English marine insurance. Lloyds is misspelled; equally important, the chapter describes the way in which the terms of the policy came to be defined in law. Nor did I "suggest that merchants, insurers, and the Admiralty cooperated in enforcing convoy discipline." On page 93 I *stated* that they did.

I do not accept Professor Steckley's remarks in the fourth paragraph, and his opening remark is unfounded. The argument hinges on being able to establish accurate records of shipping losses and their causes. No records exist, and they would be of doubtful value if they did. Contemporary lists of convoys outward bound do not record whether they arrived safely. In any case, tonnage figures usually varied from voyage to voyage. Nor has Steckley appreciated that "losses" include vessels captured by enemy privateers and warships as well as those sunk at sea from a variety of causes. My approach has been to take estimates of total trade and show how effective naval defense was in each war. Similarly, Steckley's remark in the following paragraph—"Crowhurst offers no conclusion about the efficiency of the various defensive measures"—I reject on the grounds just outlined.

Finally, I should like to comment on Professor

Steckley's suggestion that East Indiamen or vessels of a similar type—that is, built like naval frigates—would have been more suitable to carry the produce of the tobacco and sugar trades. In the first place, the East India Company needed the ships for its own trade and would have kept them; it had enormous political power. If Steckley has assumed that similar ships could have been built at short notice, he has not considered the cost of ship-building in wartime or of the time needed to build large ships when materials were scarce. Also, Steckley has not considered who would have had to pay for them or the political furor such a proposal would have caused. As I have demonstrated in my book, governments wanted allies among the merchants during wartime, not enemies. The huge financial demands forced governments to raise money by loans, and for this they had to have good relations with merchants and commercial companies.

Professor Steckley's comments are thus based on a misunderstanding of much of the subject matter and on a hasty reading of my book. Other reviewers have been more perceptive, and I have received a very favorable press on this side of the Atlantic.

PATRICK CROWHURST
Leicester Polytechnic

PROFESSOR STECKLEY REPLIES:

I welcome this opportunity to reply to Patrick Crowhurst's comments on my review of his book, and I will do so in the order of his criticisms.

I should have thought that the map of the Baltic was included on page 51 to help the reader locate places named in the discussion of the relevant place names found on page 52, not on page 74. But none of the relevant place names on page 52—the Sound, Heligoland, Elsinore, and Hamburg—is found on the map.

Professor Crowhurst's comments on real value confirm my earlier suspicion that he does not understand the meaning of the term. He stated that the "real value of commodities imported by [the East India Company] had declined" (p. 212), but he offered no evidence that he deflated the market values of those goods with an appropriate price index—that is, he failed to show that East India goods per unit were exchanging for less in terms of other goods. What I think he intended to say is that the nominal market prices of Eastern goods were falling below the "set of book values" (p. 213) used by the Customs officers in assessing the trade. And this has nothing whatsoever to do with a change in real value.

I stumbled frequently over such compound nouns as "commerce defence," "commerce war," and "commerce raiding." I would have preferred more conventional forms. But surely no one should

be asked to read sentences like the following: "By the close of the seventeenth century Massachusetts and New York was paying for its English imports by exports to the Caribbean" (p. 108); "There was also difficulties of controlling such a large body of ships at sea" (p. 148); and "Many were attracted by the opportunity for a little private trade, some were obtained by crimps and entered the service unwillingly, but the voyages though long held no more hazards than the Caribbean trade and was far healthier than the African" (p. 218). There are others. But I must admit that I enjoyed trying to imagine the event that the following sentences describe: "The French made a number of plans to capture [St. Helena] but none succeeded. During the Seven Years War d'Estaing and Frogier de l'Eguille had both proposed the capture of the island, and in the War of American Independence Kerguelin planned another attempt—it was captured on board the *Libre Navigateur*" (p. 246). I believe that I was giving the author the benefit of the doubt by supposing in my review that there had been a failure to edit the text.

Nothing in my review suggests that the book "covers the seventeenth century." I merely noted in my introductory paragraph that the English in an earlier period had found means other than conveying to protect merchant ships against some hostile powers. And, if Professor Crowhurst is right in maintaining that all Barbary corsairs were privateers licensed by the state, he has strengthened my point.

There seems to be an honest disagreement among distinguished British authors about how one ought to spell "Lloyd's." I have followed, among others, Ralph Davis, whose excellent studies of eighteenth-century trade Professor Crowhurst has frequently cited, and D. E. W. Gibb, historian of Lloyd's and himself a member of the firm.

Nothing in my review suggests that heavily armed merchant ships should have been built "at short notice . . . in wartime" to serve the "sugar and tobacco trades." I have merely asked why some ships of such design, like those that the East India Company found the resources to build during peacetime, might not have been built for use in various trades in order to reduce the need for convoys and to shift the cost of protection from the taxpayer to the consumer.

I am not the only reviewer, I have recently discovered, who has noted that Professor Crowhurst's method is one of assertion rather than demonstration and that his book lacks comparative generalization. And, because Crowhurst has failed even to estimate the costs of providing convoy, he cannot offer useful conclusions about the efficiency of that service.

Finally, I can assure Professor Crowhurst that I

did not give his book "a hasty reading." I felt a responsibility as a reviewer to consider his arguments as carefully as I could; and, even if I had been reading the book for pleasure alone, I would have been unable, for the reasons given, to read it with haste.

GEORGE F. STECKLEY
Knox College

TO THE EDITOR:

Paula S. Fass is so far off in her imaginings (*AHR*, 84 [1979]: 1465-66) that I wrote my *Vanguards & Followers: Youth in the American Tradition* with "evident disgust," with "alarm and fear," "anger," "pain," and "distant disdain for trivial youth" as to require comment. Please note that these fantasies about my feelings—there is a prefatory statement she could have quoted—obviously relate to my account of youth in the 1960s and early 1970s. I cannot understand how she could try to relate "evident disgust" and the like to my chapters on the larger part of the youth tradition, involving as they do leaders and developments (including "Young America") and going back to American beginnings.

My book, according to Professor Fass, is not history but a "personal testament." It lacks methodology. It emphasizes individuals. Is this true, also, of my *Randolph Bourne*, which deals with the first modern youth crystallizations, from which later ones derived? There is no evidence that Fass knows this book. In her own research about 1920s youth, she thinks *conformist* youth of the time, defined precisely as "native, white, middle-class, and almost exclusively college-going" (p. 7) writing for college journals, is the true youth. She manifestly has no idea of the role of *visible and effective* youth of any given decade. She would deem Carlyle no historian. I assert, without derogation, that youthful fads and vagaries such as she expanded upon do not determine the career of a Hitler, a Mussolini, a "Che," and their regiments of young followers—or, at home, a young Garrison, a young Bellamy, a young Upton Sinclair (as distinguished from a young John Reed), and others who built traditions long before the 1920s, and the grand pay-off of the 1960s—all of whom I have treated in writings to which Fass made no reference.

I reduce, she claimed, culture to personality, as if, for example, Norman Mailer's mayoralty challenge did not determine a New York election or, before him, Walt Whitman's or Ralph Waldo Emerson's explicit appeal to youth was "trivial." Were they trivial, or did they motivate vital segments of youth?

The basic wrongness, I repeat, of her unsubstantiated charges, which do not cope with any of the above, passes over the fact that nowhere in *Vanguards & Followers* did I separate the youth tradition

from the necessary aid and cooperation of elders. Youth was not age. Youth was a point of view. I cannot here recap my *evidence* as set down for the several eras involved, and for the book as a whole. It is displayed at length in my *Randolph Bourne* and, indeed, elsewhere, for those who are interested. It suffices that the reviewer cites no proof of any kind for her assertions.

Most glaring, most expressive of unstated emotion, is her stated hope that "serious historians" will not "include [my] book within the growing literature on youth today." I must curb many thoughts that sentence suggests. It must suffice that this forthright appeal to repression in the history field—even to refute—does demand, in so basic a publication as the *AHR*, that she give us some of the names and titles of the "many historians" not included in my 20-page bibliography who have been investigating the larger picture of youth in America, whose methodology she approves. Others beside myself will await her response and her list.

LOUIS FILLER
Antioch College

PROFESSOR FASS REPLIES:

I gladly offer the following books and articles as examples of the range of currently available work useful for the study of "youth in history." (1) Philippe Ariès, *Centuries of Childhood: A Social History of Family Life* (1962); (2) Robert H. Bremner et al., eds., *Children and Youth in America: A Documentary History*, 3 volumes (1974); (3) Natalie Z. Davis, "The Reasons of Misrule: Youth Groups and Charivaris in Sixteenth-Century France," *Past & Present*, no. 50 (1971); (4) S. N. Eisenstadt, *From Generation to Generation: Age Groups and Social Structure* (1956); (5) John R. Gillis, *Youth and History: Tradition and Change in European Age Relations, 1770-Present* (1974); (6) Oscar Handlin and Mary Handlin, *Facing Life: Youth and Family in American History* (1971); (7) Joseph F. Kett, *Rites of Passage: Adolescence in America, 1790 to the Present* (1977); (8) Gilman M. Ostrander, *American Civilization in the First Machine Age, 1890-1940* (1970); and (9) Steven Schlossman, *Love and the American Delinquent* (1977).

"Youth today," which is Louis Filler's misquotation from my review, is a quite different subject and one about which neither I nor Professor Filler are expert.

PAULA S. FASS
University of California,
Berkeley

TO THE EDITOR:

I would like to respond to several issues that Richard J. Evans raised in his review of my book, *The*

Jewish Feminist Movement in Germany (AHR, 85 [1980]: 417–18). Evans believed I “strained” to give the Jüdischer Frauenbund (JFB) “feminist credentials.” He quoted from a section entitled “the contradictions of Jewish feminism . . .” in which I explained the constraints on the feminist leadership by a more conservative following, by their own bourgeois upbringing, and by their fears of antisemitism. It would be equally facile to pick out the “feminist” quotations, but the goals of the JFB speak for themselves. These included equal education and career opportunities for women, support of the goals of the German women’s movement, suffrage in the Jewish community, equal pay for equal work, the care of unwed mothers, and the elimination of discrimination against women in Jewish law and the elevation of women’s image in general. There was a “social feminism” (p. 13)—practical social reforms to improve the circumstances of women’s lives—similar to the social feminism described in such historical works as J. Stanley Lemons’s *The Woman Citizen: Social Feminism in the 1920s* (1973) or William O’Neill’s *Everyone Was Brave* (1969). While justifying itself in reformist language, the JFB went well beyond “a comfortable resemblance to traditional Jewish charities” to attack male hierarchy and provide woman-oriented alternatives. Its ideas, accomplishments, and failures must be understood within the context of German history: the weakness of German liberalism, the omnipresence of antisemitism, and the intransigence of antifeminism. In any case, “feminism” ought not to be narrowly defined. We should look for women’s achievement and appraise their lives in a far broader context. An understanding of the variety of ideas and methods with which women tried to gain more control over the making of their own history suggests that feminism is a *process* rather than an “ideological commitment.” While recognizable over time, it assumes different forms and expresses different goals specific to its historical situation. Many women knew well enough to pursue their own interests without Evans’s “most minimal definition.”

Evans wrote that I presented no evidence that the JFB “ever demanded anything more than women’s suffrage on local and Jewish welfare bodies.” He has failed to understand the importance of these bodies to Jews and the radical nature of the JFB’s demands within this religious context. The *Gemeinde* was a publicly constituted corporation, automatically embracing all Jews in any one place and empowered by the government to tax its members. One could take the extraordinary step of seceding from the *Gemeinde*, but few Jews did. The decisions of this corporate community regarding cultural and religious life and social welfare policies had a profound impact on the lives of all Jews. In condemning JFB members for not having done much

for “wider feminist goals” in Germany, Evans has denied the legitimacy of their primary religious and ethnic identification and disregarded their perceived and actual rejection by German society.

Evans suggested that I “missed an important opportunity to explore the psychological implications” of the fact that the leader of the JFB campaign against white slavery was Bertha Pappenheim, Freud’s “Anna O.” Although it is tempting to speculate about the personal and psychological motivations of any purity reformer, the important point is that white slavery was a major problem that troubled Jewish communities in Eastern and Western Europe and in the Americas. White slavery was not the figment of someone’s repressed sexual fantasy.

I nowhere portrayed the JFB as the “most important religious organization” in the German women’s movement. What I said was that “the JFB was the only religious women’s organization in Germany to promote feminism in its goals and to belong to the German women’s movement.” Jewish feminists were painfully aware that the right-wing Protestant Women’s League carried far more weight within the national organization, despite the JFB’s longer loyalty, larger membership, and more progressive complexion.

Finally, my book is based on the JFB’s periodical—about 15 percent of the footnotes refer to it—as well as on secondary literature, archival materials, memoirs, interviews, and other primary sources, including a variety of publications by Pappenheim and the JFB.

MARION A. KAPLAN
New York City

PROFESSOR EVANS REPLIES:

Marion A. Kaplan claims that “the goals of the JFB speak for themselves.” But the goals of the JFB as listed on pages 86–87 of her book are not the same as those claimed in her communication. Equal education and career opportunities for women and equal pay for equal work were supported by the JFB only insofar as they were (like votes for women) on the program of the umbrella organization of the German women’s movement, the BDF, to which the JFB belonged. The BDF program was not binding on member associations, and many of them did not support many of the points on it or paid only lip-service to them. The German Association against the Abuse of Alcoholic Drink, for instance, belonged to the BDF; it was in fact the largest member association (not the JFB, as Kaplan claimed); but it can hardly be regarded as an active feminist association; it only belonged in order to secure the BDF’s support for the temperance cause. Membership in the BDF did not, in itself, make an

organization feminist; the antifeminist Protestant Women's League, for example, also belonged. Nor are the other JFB goals that Kaplan listed necessarily feminist. The Catholic Church had homes for unwed mothers—did that make it feminist? The Nazi Party campaigned for what it understood as the elevation of women's image in general—did that make it feminist? On many more accurate touchstones of feminist commitment, the JFB must be found wanting. The discussion of the JFB's attitude to careers for women on pages 171–72 of the book fails to show that the JFB ever demanded women's entry into the higher professions, for instance. And on page 187 Dr. Kaplan even remarked that "Jewish feminists almost never demanded for themselves the right to work outside the home."

Certainly the JFB was feminist, if we accept Dr. Kaplan's definition of feminism as the process of women's pursuit of their own interests, or their attempts to gain more control over the making of their own history. But this definition is so wide as to be virtually useless in historical analysis. Since the act of organizing under their own leadership, for whatever purpose, constitutes *ipso facto* an attempt by women to gain more control over their own lives, and is bound by them to be regarded as being in pursuit of their own interests, any women's organization will be a feminist organization on Kaplan's criteria. Does she want to suggest that women's antisuffrage leagues were feminist? Suprahistorical definitions of this kind simply will not do. Nor do I think O'Neill's concept of "social feminism" is very helpful. Some consideration of ideology is necessary. Judged by the standards of their own time, and taken at their own estimation, the women of the JFB were not feminist, and the evidence that Kaplan presented for this view in her book far outweighs any evidence on the other side.

Dr. Kaplan's second paragraph is a grotesque and insulting misreading of what I wrote in my review. I fail to see how any rational observer could interpret my review as denying the legitimacy of the JFB members' primary religious and ethnic identification, disregarding their rejection by German society, failing to understand the importance to them of welfare bodies, or belittling the significance of their demands within the Jewish community. In my review, indeed, I went out of my way to praise Kaplan's handling of the problems that Jewish women encountered within the Jewish community and in German society as a whole. Where I did—and do—think the book is misleading is in its attempt to imply that the JFB actively pursued wider feminist goals advocated by leading feminist organizations of the day, such as the parliamentary suffrage. Kaplan resorts to the wild and fantastic allegations that constitute the second paragraph of her communication because she is unable to provide a

reasoned reply to the criticisms that I made of her book in this respect.

Similarly, I fail to see how on earth my review could be taken to imply that "white slavery" was "the figment of someone's repressed sexual fantasy." Once more, Dr. Kaplan is reading into the review something that just is not there. Like all social reform movements, the crusade against "white slavery" arose out of the identification of a real social problem, but it was also conditioned by the motives and attitudes of those engaged in it. One needs to explain why Pappenheim devoted her energies to this rather than to other causes, and Kaplan did not, in my view, explore the personal dimension of this choice satisfactorily. Kaplan herself admitted on page 133 of her book that the JFB exaggerated the dimensions of the "white slavery" problem in the 1920s, but she did not ask why, nor did she consider the possibility that it might have done the same earlier.

Dr. Kaplan's denial that she portrayed the JFB as the most important religious organization in the German women's movement is thoroughly disingenuous. Her quotation continues: "The Protestant Federation of Women, begun in 1899, supported ecclesiastical suffrage, but did not approve of women's suffrage in the nation and withdrew from the BDF shortly after joining it" (p. 152). Here Kaplan seemed to me to be making parliamentary suffrage the criterion of feminist commitment. Yet the JFB gave active support only to the equivalent within the Jewish community of ecclesiastical suffrage within the Protestant communion. Indeed, on the definition of feminism that Kaplan advances above, I cannot see how it can be denied that the Protestant Women's League, which played within the Protestant community a comparable role to that played by the JFB in the Jewish, was a feminist organization. But the crucial point in the text on page 152 is the claim that the Protestant women withdrew from the BDF shortly after joining it and so, by implication, played no significant role in it. In fact, they belonged to the BDF for a whole decade, from 1908 to 1918. I am glad that Kaplan now concedes that Jewish feminists were aware that the Protestant Women's League carried more weight within the BDF than did the JFB, a point she nowhere made in her book.

Finally, Dr. Kaplan's claim that only 15 percent of her footnotes refer to the JFB's periodical, though true, is equally disingenuous. A large proportion of the notes refer to comparative and secondary material. In every chapter, references to the JFB's periodical outnumber those to archival material. In all, there are over one hundred footnotes referring to the periodical and fewer than seventy referring to unpublished sources. Moreover, the overwhelming majority of the archival material comes from one

institution, the Leo Baeck Institute. The total unpublished source material consulted in Germany amounts to a single file in a single archive.

RICHARD J. EVANS
Columbia University

TO THE EDITOR:

Richard N. Current in his review of my book, *The Day of the Carpetbagger: Republican Reconstruction in Mississippi* (AHR, 85 [1980]: 465), has distorted my treatment of Reconstruction politics. He claimed that I follow "the conservative line of that time," and he even suggested that I justified Ku Klux Klan and Democratic violence. Nothing could be further from the truth. My purpose, which other reviewers believe I achieved (see *Reviews in American History*, 8 [1980]: 69-73, and *History: Review of New Books*, 8 [1979]: 27), was to write a balanced account of postwar affairs without resorting to either old or new stereotypes. For example, I avoided labeling all conservatives as reactionaries or all Radical Republicans as honest crusaders for right and justice, as Current apparently prefers. The reviewer's charge that I used conservative sources uncritically and even adopted the comments of "reactionaries" as my own is untrue. In quoting and paraphrasing conservative spokesmen, who were frequently divided on issues, I attempted to demonstrate how they perceived Reconstruction events and developments, which to a great extent explains why they reacted the way they did to the Republican ascendancy and why the new order ultimately failed. In discussing the Ku Klux Klan, my purpose was to explain it in a broader context than has been previously done and also to indicate why the Klan achieved only a partial success before the federal government stepped in to suppress it in 1871. At no point did I justify the Klan's existence or suggest that Republican mistakes or policies justified white violence and intimidation.

Professor Current has also charged me with slighting or ignoring Republican sources. Again, he is incorrect. I made extensive use of such Republican sources as the *Jackson Pilot*, the *Vicksburg Times and Republican*, the official correspondence of the governors, the printed reports of Republican officials, Republican addresses, and Adelbert Ames's correspondence with his wife. I did not cite extensively, as Current said I should have, the congressional testimony regarding the 1875 campaign because more immediate and informative Republican, as well as Democratic, materials existed. I might also point out that Republican statements, like those of the Democrats, could be self-serving.

Professor Current was disturbed by my criticism of carpetbagger Ames, contrasting my treatment of him with my alleged failure to condemn the Demo-

cratic leadership. Since Ames and the Republicans governed the state during Reconstruction, I naturally emphasized their policies and performance, but I did mention the racism and even the hypocrisy of many Democratic leaders, although perhaps not as frequently or as strongly as Current desired. Current seemingly ignored my generally sympathetic treatment of such moderate and anti-Ames Republicans as Governors Alcorn and Powers, U.S. Senators Revels and Pease, James Lynch, and others of both races.

My criticism of Ames, which was not unqualified, was based to a great extent on non-Democratic sources. Many Republicans, including national leaders, expressed dismay at Ames's efforts in 1869 to build up a political following in Mississippi while serving as the military district commander. As I indicated in the book, General Ames temporarily "opened wide the door for adventurers and scamps," and in early 1870, while still in command, high-handedly secured his own election to the U.S. Senate. But, in the same sentence in which I charged Ames with playing "the role of military despot," I commended him for his "vigorous efforts to maintain the social equilibrium in the state and prevent attacks on Republicans" (pp. 253-54).

Contrary to what Professor Current said, I have not placed the sole blame on Ames and his followers for the failure of Reconstruction. Finally, Current's concluding comment that Harris "does subscribe to the principle of racial equality—as did the Mississippi conservatives in the 1875 platform" is gratuitous, to say the least.

WILLIAM C. HARRIS
North Carolina State University

PROFESSOR CURRENT REPLIES:

Nothing in William C. Harris's letter persuades me to change my opinion of his book or my review of it. His letter does, however, provide an opportunity for me to confirm and, perhaps, to clarify my criticism. My review describes his work on Mississippi Reconstruction as "prodigious," one dictionary definition of which is "extraordinary in size, amount, extent, degree, force, etc." The review also refers to it as the most "circumstantial" account of Reconstruction in any state, "circumstantial" meaning in this context "dealing with or giving circumstances or details." These characterizations certainly imply that the study is valuable if not indispensable for any scholar interested in the subject. The review also mentions other strengths of the book and then, to caution users of it, goes on to indicate the author's bias. Harris obviously thinks he has none, but any careful reader who is familiar with the Mississippi politics of the period will see that, in important respects, he did indeed follow "the conservative line of

that time." When, for example, he said the Ku Klux Klan directed its activities not against black schools as such but only against "obnoxious" teachers, he was clearly following the line and he was also mitigating if not justifying Klan atrocities. If he quoted and paraphrased conservatives "to demonstrate how they perceived Reconstruction events," he neglected to give the reader any hint as to which views were exclusively theirs and which are also his own.

As for his "extensive use" of Republican sources, the real question is the kind of use Professor Harris made of them, which was essentially to corroborate conservative charges rather than to present a case for Ames and the Ames Republicans. Not that all of the Radicals were at all times simply "honest crusaders for right and justice"—a judgment that Harris falsely imputes to me. But, after all, Ames as

governor was trying to advance the cause of Reconstruction, including the assurance of political rights and social justice for blacks. The conservatives, Alcorn along with them, were doing all that sedition and terrorism could do to frustrate that cause. Hence, it seems like rather strange logic for Harris now to explain that, since "Ames and the Republicans governed the state," he has "naturally emphasized their policies and performance" in accounting for the failure of Reconstruction in Mississippi. Finally, there is nothing "gratuitous" in the concluding sentence of my review; it is intended to clinch the point of the like-mindedness (apparently quite unconscious on his part) between Harris and the Mississippi conservatives of 1875.

RICHARD N. CURRENT
*University of North Carolina,
Greensboro*

Recent Deaths

SIR GEORGE NORMAN CLARK, formerly Regius professor of modern history at Cambridge University and provost of Oriel College, Oxford University, died on February 6, 1979, in Oxford. Born on February 27, 1890, Clark was educated at Bootham School and Manchester Grammar School before going up to Balliol College, Oxford, where he was a Brackenbury Scholar in 1908. He took a First in Literae Humaniores in 1911 followed by a First in modern history in 1912, the same year in which he became a fellow of All Souls. Following service in France during World War I, he returned to Oxford to become a fellow and lecturer at Oriel College from 1919 to 1931. During those years he held a succession of college and university positions, becoming tutor at Oriel in 1922 and librarian in 1930, as well as being university lecturer in modern history 1927-31 and university proctor 1929-30.

Already in these years he had established himself as a formidable and original scholar. His *Dutch Alliance and the War against French Trade* (1923) was instantly recognized as a significant contribution, while his outstanding talents as an editor were revealed in *The Churchwardens' Accounts of Marston* (1925) on which he collaborated with F. W. Weaver. Those same talents were put to the service of the *English Historical Review*, the staff of which he joined in 1919; he became editor of that journal in 1920, holding the post until 1925 and then serving a further year as joint editor. He was ultimately to have the unusual distinction of serving as editor of the *English Historical Review* on two separate occasions, for he resumed the editorship for a brief period in 1938, paving the way on that occasion for the editorships of Goronwy Edwards and Richard Pares. Perhaps the most noteworthy product of these years was his remarkable *Seventeenth Century* (1929), a highly original synthesis of material at the time of its publication and still an inspiring work more than half a century later.

In 1931 Clark was elected Chichele Professor of Economic History and fellow of All Souls, positions he held with distinction until 1943 when he moved to Cambridge University as Regius Professor of

Modern History and fellow of Trinity College. He returned to Oxford as provost of Oriel College in 1947 and presided there for a decade, retiring in 1957. From 1961 to 1975 he was again a fellow at All Souls.

Numerous academic distinctions were awarded to Clark, including honorary degrees from a host of institutions (Aberdeen, Utrecht, Durham, Sheffield, Hull, Columbia, Dublin, and Cambridge), honorary fellowships at Trinity College, Dublin, Trinity College, Cambridge, and Balliol, Oriel, and All Souls Colleges, Oxford, fellowship in the British Academy, of which he was president 1954-58, and a knighthood in 1953.

Throughout his career, Clark was a prolific publisher, making signal contributions both to English history and to Dutch history, in which field he continued to contribute reviews to the *English Historical Review* for a number of years after his retirement. His guiding hand was vital to two of the ambitious cooperative projects of his time, the *Oxford History of England* (to which he contributed the well-known volume, *The Later Stuarts* [1934]) and the *New Cambridge Modern History*, the planning of which coincided with his Cambridge professorship. The range of his scholarship is well indicated by the titles of his most significant volumes in addition to those already mentioned: *Science and Social Welfare in the Age of Newton* (1937); *Guide to English Commercial Statistics, 1696-1782* (1938); *The Colonial Conferences between England and the Netherlands*, with W. J. M. van Eysinga, two volumes (1940-51); *The Wealth of England* (1946); *Early Modern Europe* (1957); *War and Society in the Seventeenth Century* (1958); and *History of the Royal College of Physicians*, two volumes (1964-66). In addition he held a number of distinguished lectureships, being Creighton Lecturer at London (1948), Ford Lecturer at Oxford (1949-50), Murray Lecturer at Glasgow (1952), Wiles Lecturer at Queen's University, Belfast (1956), Donnellan Lecturer at Trinity College, Dublin (1960), Whidden Lecturer at McMaster (1960), and Leslie Stephen Lecturer at Cambridge (1965).

Sir G. N. Clark established himself throughout a

long and productive life among the masters of early modern history. Thorough and meticulous in scholarship, clear and penetrating in expression, broad and original in the conception of his themes, he has left behind a legacy of scholarship of first-rate dimensions.

ROGER HOWELL, JR.
Bowdoin College

CEDRIC C. CUMMINS died April 12, 1980, in a hotel fire in Madison, South Dakota, where he was attending the annual Dakota History Conference.

Professor Cummins was born in English, Indiana, on March 14, 1909. He completed his Ph.D. work in 1944 at Indiana University where he was a student of R. Carlyle Buley. He was professor of history at the University of South Dakota from 1946 until his retirement in 1976. He was department chairman for fifteen years. His major publications include *Indiana Public Opinion and the World War, 1914-1917* and *The University of South Dakota, 1862-1966*.

It is perhaps as a teacher and friend that he will be best remembered. His kindness and thoughtfulness were his hallmark to both students and colleagues. His patience and gentle manner were always present, whether in the field hunting pheasants, or trying to capture a wildlife scene on film, or in a raucous department meeting to discuss the development of a Ph.D. program. He was at peace with himself and with his fellow man, a state of tranquillity to which we all aspire, often with lesser degrees of success.

R. ALTON LEE
University of South Dakota

AMY M. GILBERT, professor emeritus of history at State University of New York, Binghamton, died in Endicott, N.Y., on February 27, 1980, after a long illness. She was born in Chambersburg, Pennsylvania, on February 23, 1895. After attending Wilson College, from which she received her A.B. in 1915, she went on to the University of Pennsylvania, where she earned her M.A. in 1919 and her Ph.D. in 1922. She was awarded an LL.D. by Wilson College in 1939, in recognition of her academic achievements. While still a graduate student, she taught at Clinton (N.J.) High School and Wilson College. Upon receiving her Ph.D. she accepted a position at Elmira College, where she rose to the rank of professor and served as head of the history department. She left Elmira College in 1936 to become academic dean at Milwaukee-Downer College. Five years later she was appointed dean of women at Rhode

Island College, where she remained until 1945. She served as visiting professor at Temple University during 1945-46. In 1946 she was named head of the departments of history and political science of the Associated Colleges of Upper New York (ACUNY)—comprising Champlain College, Mohawk College, Sampson College, and Middletown Collegiate Center—which had just been established as a temporary institution designed to meet the postwar demands on higher education. After the termination of ACUNY, in 1950, and the transformation of Champlain College into the first liberal arts unit of State University of New York, she was named the college's dean of academic instruction. Two years later, when the existence of the college was threatened by the U.S. Air Force's declared intention of taking over the land on which the college was located, she was appointed head of the college. After all efforts to save the college had failed and it was closed, in 1953, she was invited to teach at Harpur College, which was to become the liberal arts unit of State University of New York at Binghamton. She taught European history there until her retirement as professor emeritus in 1965.

Professor Gilbert was a remarkably dedicated person, of exceptional strength of character and firmness of purpose, who never forgot that institutions exist to serve people. In the course of her long career she performed outstanding work in many roles. As an administrator she faced and successfully dealt with many difficult challenges. This was particularly true of the ACUNY years, when she dealt magnificently with her broad and demanding responsibilities in that fledgling and amorphous institution. Then followed two years of pioneering work in setting Champlain College on its course, succeeded by a year at the thankless task of administering a college under the threat of extinction. As a teacher she acquired a loyal following among her thousands of students, who appreciated her devotion to learning, her breadth of intellectual interest, her commitment to intellectual discipline, her concern for them as human beings.

Professor Gilbert's first book was *The Work of Lord Brougham for Education in England* (1922). She contributed a chapter on the history of the woman's movement to *The History of New York State* (1933-37), edited by A. C. Flick. Her *ACUNY: The Associated Colleges Of Upper New York* (1950) is an official history that ably records an ephemeral yet important chapter in the history of higher education. After her retirement, she was able to complete a work on which she had long been engaged, *Executive Agreements and Treaties, 1946-1973* (1973), a work that reflected her continuing interest in international law. This was the first book on the subject since Wallace McClure's study published a generation earlier.

The many friends Professor Gilbert made during

her extended career will long remember her with respect and affection.

SIDNEY HARCAVE
State University of New York,
Binghamton

RAPHAEL N. HAMILTON, S.J. died in Milwaukee on April 19, 1980, after almost fifty years at Marquette University as a historian and archivist. Born in Omaha, Nebraska, in 1892, he was educated there and received his A.B. from Creighton University in 1913. That winter he entered the Society of Jesus. His graduate training was completed at St. Louis University. There he received a master's degree in history in 1919 and his clerical training, which culminated in his ordination in 1926. Finally in 1931 he received a Ph.D. in American history.

In 1930 he came to Marquette University as a member in the history department and in 1932 received the appointment as professor and head of the department. From then until he was retired as emeritus professor in 1964, he directed the department until 1957 and taught a wide range of courses. He was an excellent teacher and, in particular, carefully guided a considerable number of M.A. students who later taught history in Milwaukee schools and in Catholic high schools throughout the Midwest.

Fr. Hamilton joined the American Historical Association in the 1920s and wrote for its *Review* and other historical and religious journals. His first major monograph was a history of Marquette University (1952). Later he wrote on Marquette, the man, with a biography of Jacques Marquette (1970) in the Great Men of Michigan series. And in 1970 the University of Wisconsin Press published his *Marquette's Explorations: The Narrative Reexamined*, an investigation of the sources of Fr. Marquette's account.

A second career began for Fr. Hamilton in 1961, when he was asked to organize the Marquette archives, and from 1964 he devoted his full time to this new task. Along with the university archives, he invited other collections, and developed a body of records that documented the trend of American Catholic social changes in the late nineteenth and twentieth centuries. He retired as university archivist in 1972, but kept on working at the archives until the end of 1977.

Fr. Hamilton had a wonderful ability to help others and inspired a great number of historians and archivists. At his wake, a student who took his classes in the 1930s remarked that he still retained the vision of history that Fr. Hamilton had taught him.

ROBERT CALLEN, S.J.
Marquette University

THURMAN EVERETT PHILOON was born in 1914 in Winchester, Massachusetts. As an undergraduate at Bowdoin College, he was inducted into Phi Beta Kappa as a junior, won the senior Greek prize, the Latin prize two years in a row, and graduated with Highest Honors in Classics, *summa cum laude*.

After graduation, he taught Latin, history, and English for three years in a Massachusetts high school. In graduate school, he received the M.A. from Harvard in 1941, then served in the United States Army until 1946. After World War II, he entered Yale, receiving the Ph.D. in 1950. In 1953, following three years of teaching history at the University of Bridgeport, he ventured from his native New England into Pennsylvania to join the department of history at Franklin and Marshall College.

He wore his impressive academic credentials and honors with unselfconscious grace, never overproud, never seeking to impress. Fluent in German, he could nevertheless joke about his difficulties with the language. From his sabbatical quarters in Marburg, he wrote a friend: "[Helen and I] wish we could adapt to the language as quickly as the radio we brought [with us] has."

During his past twenty-seven years at Franklin and Marshall, he taught a variety of courses covering ancient, medieval, Renaissance, and Reformation history . . . and his speciality, military history. Not long ago he introduced a new course into the curriculum, and during the past few years he was experimenting with the integration of films into his military history course.

His scholarly interests were as varied as his courses, with publications in medieval history and Reformation studies, and research interests ranging from the role of the elephant in the Greek and Roman worlds, to the role of Hessian mercenaries in the American Revolutionary War.

His students were fond of him, perhaps because he never seemed too busy to talk with them about history or their personal problems, perhaps because of his friendly, warmhearted manner. He was a veritable pipe-smoking, academic Santa Claus of a man, with a twinkle in his eye, and a wonderful down-East brogue. Through his enthusiasm, some of his students undertook independent study on military history. He would invite them along on his many research trips to the Army War College Archives in Carlisle.

A longtime member of the American Historical Association, Thurman E. Philoon died of a heart attack on May 11, 1980. A memorial service at the College on May 14 honored him.

LOUIS L. ATHEY
Franklin and Marshall College

PETER ROMANOFISKY, associate professor of history at Jersey City State College, died of cancer, sur-

rounded by his family at his home in New York City, on May 4, 1980. He was thirty-seven years old. A native of Long Beach, New York, Peter received his B.A. from Queens College (1964) and his M.A. (1965) and Ph.D. (1969) from the University of Missouri, Columbia, where he worked with John Lankford. He spent one year as a visiting assistant professor at Sir George Williams University before joining the faculty of Jersey City State College in 1970.

Peter was a remarkably beautiful human being. A kind and compassionate friend, he was unfailingly generous with his time and hospitality. He was a gifted teacher, supportive colleague, and thoughtful scholar whose integrity, friendship, dedication, and curiosity were never more evident than in his courageous struggle against the dreaded disease that took his life. He never gave up and lived a fuller life in his last five years than most of us do in a lifetime. A conscientious historian, Peter was a member of the American Historical Association, the Social Welfare History Group, Columbia University's Faculty Seminar on the City, and the Immigration History Society and a life member of the Organization of American Historians.

He leaves behind a rich scholarly legacy. A specialist in the history of social welfare, Peter's articles on dependent children, foundlings, and adoption have appeared in the *Missouri Historical Review*, *American Jewish Historical Quarterly*, *Bulletin of the History of Medicine*, *Jewish Social Studies*, and *New-York Historical Society Quarterly*. These essays are characteristically well documented, meticulously crafted, and gracefully written. His lasting and most enduring scholarly accomplishment was the publication of a prize-winning, monumental two-volume reference work on *Social Service Organizations* in the Greenwood Press *Encyclopedia of American Institutions*. Its appearance was all the more remarkable in view of Peter's hospitalization during both the research and writing.

Peter's love of New York City was one of his prized idiosyncracies. He gloried in riding his bicycle during rush hour, doing without a car, and discovering Chinatown's most obscure but delicious restaurants. Nothing gave him more pleasure than taking his nephews to a Yankee baseball or Ranger hockey game. He brought joy to those whose lives he touched, and he will be greatly missed but not forgotten by his friends who loved him dearly.

EUGENE M. TOBIN
Hamilton College

GEORGE GORDON WINDELL, professor of history at the University of New Orleans since 1969, died on July 3, 1979, in Geneva, Switzerland. He and his wife

were en route to Innsbruck, Austria, where he was to have participated in the university's Innsbruck Summer School Program. After falling ill in Spain, he was hospitalized upon reaching Geneva, and died following emergency surgery to repair a cerebral aneurysm.

Born in Seattle, Washington, in 1920 to George A. and Grace E. Windell, he grew up in Kansas City, Missouri. In 1940, he took an A.B. degree with distinction at the University of Tulsa, and in the following year he received a master's degree in history from the University of Missouri. Serving the United States Army from 1942 to 1945, he participated in five European campaigns as a member of the 45th Field Evacuation Hospital.

He resumed his graduate studies at the end of World War II, first at the University of Missouri, where he was also instructor of history, and then at the University of Minnesota, where, in 1953, he earned his doctorate under the tutelage of Lawrence D. Steefel. He joined the faculty of the University of Delaware in 1948, and he taught there for twenty years, as well as serving as president of the local chapter of the American Association of University Professors and participating in other faculty and cultural affairs.

During his tenure at the University of New Orleans, Professor Windell was coordinator of graduate studies in history and served on many university, collegiate, and departmental committees. He also taught regularly in the University's European summer program, in addition to being visiting professor at Cornell and Tulane Universities. Among his other activities, he was noted in New Orleans as the producer-host of "Opera Hall," a weekly broadcast of WWNO, the local public radio affiliate.

An outstanding historian, whose fields were German and European intellectual history, Professor Windell authored *The Catholics and German Unity* (1954), as well as numerous articles and reviews which appeared in major scholarly journals. His honors included awards for postdoctoral research in Germany, a Fulbright-Hays Fellowship in 1954, and a grant from the American Philosophical Society in 1962. He held memberships in several scholastic honor societies, as well as the American and Southern Historical Associations.

At the time of his death, he was preparing the manuscript for a second book, which was to have examined the relationship between romanticism and nineteenth-century science. He had also undertaken research into the careers of nineteenth-century German feminists.

One of the most popular teachers at the University of New Orleans, Professor Windell introduced many neophytes to the discipline of history. His keen mind and sharp wit attracted both undergraduate and graduate students to his courses, and

a number of his former students at Delaware and UNO have become practicing historians.

Professor Windell is survived by his wife, the former Marie Elizabeth George. He was memorialized by colleagues, friends, and students at the Episcopal Church of the Holy Comforter, New Orleans, on August 27, 1979. His ashes were interred at Arlington National Cemetery, where his father, a

Navy veteran of World War I, and his mother lie buried.

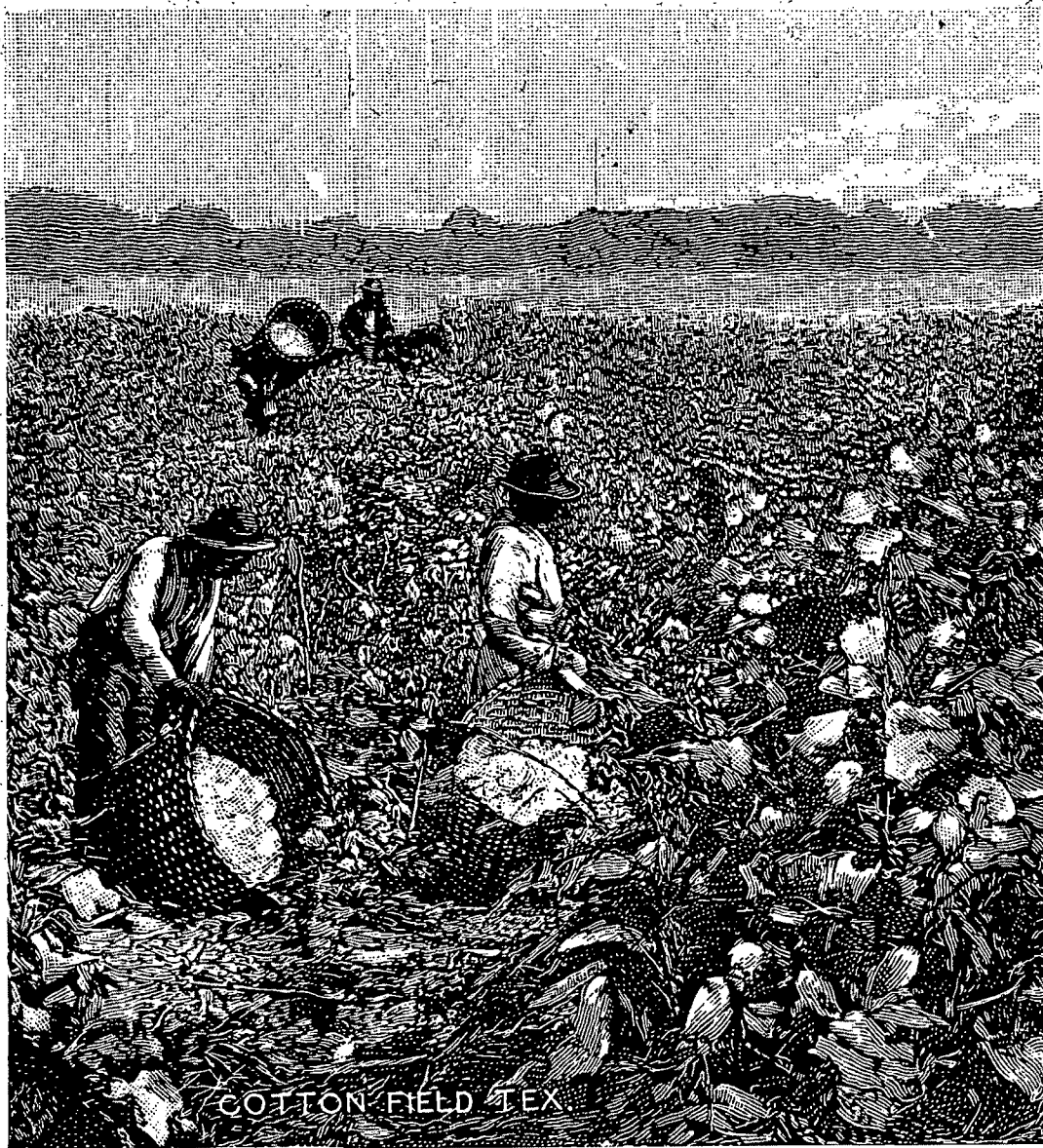
The department of history at the University of New Orleans has established a scholarship fund as a memorial to Professor Windell.

WARREN M. BILLINGS
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The American Historical Review

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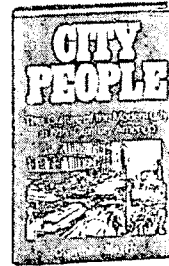
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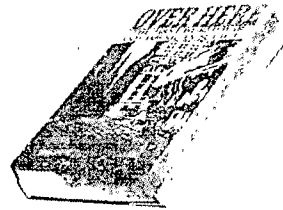


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CARVILLE EARLE, associate professor of geography at the University of Maryland, Baltimore County, is the author of *The Evolution of a Tidewater Settlement System: All Hallow's Parish, Maryland, 1650-1783* (1975) and various articles on the historical geography of the United States. Earle received his Ph.D. from the University of Chicago, where he studied with Marvin Mikesell and William Pattison. Currently, he is working on the geography of labor during the Gilded Age as codirector of the Project on the Geography of American Labor and Industrialization (GALI).

RONALD HOFFMAN, associate professor of history at the University of Maryland, College Park, is the author of *A Spirit of Dissension: Economics, Politics, and the Revolution in Maryland* (1973) and various articles and essays in eighteenth-century social, political, and economic history. He received his Ph.D. from the University of Wisconsin, Madison, in 1969. Currently, he is editing the papers of Charles Carroll of Carrollton and is directing a series of annual symposia for the United States Capitol Historical Society.

EARLE and HOFFMAN began their collaboration in a study of eighteenth-century economic development that appeared as "Staple Crops and Urban Development in the Eighteenth-Century South" in *Perspectives in American History*, 10 (1976). Some of the issues raised there, particularly the relationship between agriculture and labor economics, seemed germane to antebellum industrialization. Further research into the transition from pre-industrial to indus-

trial economies in the nineteenth century resulted in the study printed here.

FORREST MCDONALD is a professor of history at the University of Alabama and a distinguished senior fellow of the Center for the Study of Southern History and Culture. He is the author of fourteen books, including *We the People* (1958) and *E Pluribus Unum* (1965, 1979). His latest book is *Alexander Hamilton: A Biography* (1979). In addition to his joint research with Grady McWhiney and to his work in constitutional, legal, and economic history, he is engaged in a study of political rhetoric in the English-speaking world from the sixteenth through the eighteenth centuries.

GRADY MCWHINEY, a professor of history at the University of Alabama and a distinguished senior fellow of the Center for the Study of Southern History and Culture, is the author of *Braxton Bragg and Confederate Defeat* (1969), *Southerners and Other Americans* (1973), and *Attack and Die: Confederate Military Tactics and the Southern Heritage* (with Perry D. Jamieson; forthcoming), and various articles. He received his Ph.D. from Columbia University, where he studied with David Herbert Donald. Currently, he is completing a study of social life in the Old South.

MCDONALD and MCWHINEY began their collaboration with "The Antebellum Southern Herdsman: A Reinterpretation" in the *Journal of Southern History*, 41 (1975). They have continued their reinterpretation of Southern history jointly and separately, and they are at present working together on the Celtic influence on the South.

EDWARD PESSEN, Distinguished Professor of History at Baruch College and The Graduate School and University Center, The City University of New York, studied at Columbia University under Richard B. Morris. Pessen's books include *Most Uncommon Jacksonians* (1967); *Riches, Class, and Power before the Civil War* (1973); and *Jacksonian America: Society, Personality, and Politics* (rev. ed., 1978). In recent years he has published articles on the new histories, the changing American social structure, opportunity in America, and the distribution of power in American history. As a Guggenheim Fellow in 1977-78, he began a study of social mobility over the course of American history. He is preparing for publication a book-length manuscript on this theme and related studies on the social origins of Tin 'Pan Alley's leading songwriters and the social backgrounds of the American presidents.

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The Foundation of the Modern Economy: Agriculture and the Costs of Labor in the United States and England, 1800–60

CARVILLE EARLE
and
RONALD HOFFMAN

THE ADVENT OF INDUSTRIALIZATION and the emergence of the industrial city during the last three centuries have fundamentally transformed human society; no other combination of developments has equaled their importance. In the Anglo-American world, where the transition to industrialization first occurred, scholars have emphasized that the high cost of labor played a critical role in the sequence of economic expansion. Indeed, so uncontested has been the traditional account of the decisiveness of labor costs in the process of industrialization that its basic premises have gone almost unexplored. The classic interpretation represented most fully in H. J. Habakkuk's *American and British Technology in the Nineteenth Century* (1962) ascribes the introduction of manufacturing technologies in the United States to a shortage of labor created in a nation that contained a modest population and abundant land.¹ Habakkuk contended that between 1810 and 1840 would-be entrepreneurs adopted new technologies to increase the productivity of labor. His thesis rests on the assumption that American labor was very limited and, hence, high-priced. Yet a close comparison of the relationship of labor costs to seasonal agricultural employment during the first half of the nineteenth century in three distinctive economies—the grain belt of the Mid-Atlantic states and the Old Northwest, the cotton South, and the English countryside—suggests that this argument needs to be reversed. The results of

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¹ For a recent summation of the decisive and continuing influence of H. J. Habakkuk's *American and British Technology in the Nineteenth Century: The Search for Labour-Saving Inventions* (Cambridge, 1962), see Paul Uselding, "Studies of Technology in Economic History," *Research in Economic History: Supplement 1* (1977): 159–219, esp. 207–08: "In many respects Habakkuk's work is like John Hicks' celebrated *Value and Capital* in that it has a kaleidoscopic quality. Certain changes in perspective reveal an endless variety of possibilities for viewing technological events, their causes and consequences. Economists are fond of saying 'It's all in Adam Smith,' and one is tempted to remark that for economic historians concerned with technological events, 'It's all in Habakkuk.'"

such a comparative analysis indicate that industrialization and the growth of the industrial city depended more upon cheap labor, which allowed for industrial investment, than upon expensive labor, which necessitated it—a pattern exemplified in the notably rapid industrialization that took place in Philadelphia during the 1820s.

In all of the regional economies examined, the nature of staple production was the principal controlling mechanism that regulated the cost and availability of labor and, therefore, the levels of industrialization and urbanization. More precisely, in both the United States and England during the period 1800–60, the production of regional agricultural staples, by determining the amount rural laborers earned, governed the processes of economic development, urbanization, and industrialization. The three regions possessed remarkably different seasonal demands for agricultural labor, and, in each, the cost of unskilled urban labor was a function of rural labor's earnings: where rural laborers did poorly, urban labor was cheap; where they did well, urban labor was expensive. The Old Northwest, for example, featured a wheat, corn, and livestock economy with a production schedule that required heavy labor inputs for only four months of the year with the greatest demand arising at harvest time. Consequently, most rural wage earners could anticipate full work for only one-third to one-half of a year at best. Because their annual earnings were correspondingly low, these workers could be induced into urban employment easily and inexpensively. In England and the cotton South, however, crop seasons ranged from eight to twelve months. In these economies, the yearly earnings of rural workers were higher, and, therefore, the costs of attracting rural labor to city jobs also increased substantially. Although the nearly full-year employment created by England's diversified and intensive agricultural economy did not hinder migration to the cities, it did raise the expense of hiring urban workers to a level that prevented extensive investments in machine technology. In the cotton South, aspiring urban entrepreneurs confronted labor costs that were truly prohibitive. The escalating price of cotton in the 1850s easily enabled nonslaveholders to earn high annual incomes in a variety of rural occupations—as small planters, plain farmers, overseers, or even tenants—and thus materially diminished any possible attractions of city life.

In summary, the grain economy of the Mid-Atlantic states and the Old Northwest and the cotton economy of the South represented the extremes. In the Old Northwest of the 1850s (as in the Philadelphia region of the 1820s) labor was cheap, a condition that encouraged both urbanization and the diffusion of machine technology. Conversely, the high cost of labor (whether free or slave) in the cotton South stymied any movement toward industrialization and urbanization. Finally, labor costs in England during the early decades of the nineteenth century represented an intermediate situation in which the price of both skilled and unskilled labor retarded the diffusion of machine technology but allowed substantial urbanization to occur. What follows is a comparative analysis of the availability and cost of labor in the grain belt of the North, the cotton South, and the English countryside. The analysis includes a review of the litera-

ture on scarce and expensive American labor as a cause of technological change, presentation of an alternative model of development based on low-priced labor, resembling in structure Sir Arthur Lewis's theory of development with an unlimited supply of labor but—unlike Lewis's—incorporating the mechanism of agricultural seasonality and its market imperfections, and, to test this model, an examination of labor costs as determinants in the processes of capital accumulation, urbanization, and industrialization.

NEOCLASSICAL ECONOMISTS HAVE CLAIMED that both labor and capital benefited from American economic development, at least in the northern United States. Labor received increasingly higher wages and larger shares of its products' value added. In other words, wages accounted for a greater percentage of the total value of production exclusive of the costs of materials. The theoretical underpinning for this belief is the contention that both skilled and unskilled labor were scarce and, therefore, commanded premium wages and earnings. In rural areas, the scarcity of labor allegedly encouraged the payment of high wages equal to labor's marginal product, defined here as the value of increased output acquired by the application of additional labor. High farm wages, in turn, meant that, in order to attract rural labor, urban employers had to pay a transfer wage that exceeded the value of urban labor's marginal product. To do this, urban capitalists were compelled to elevate labor's marginal product to the level of wages, and they made this adjustment by adding machine technology. Over the long term, capital investment in mechanization increased output per worker, and labor's share of product value added remained steady. This argument, generally referred to as the "Habakkuk thesis" after its principal proponent, contains a number of logical and evidentiary problems. Although economic historians have raised some questions about it, they have avoided challenging the central proposition of labor scarcity.²

Peter Temin, the most forthright critic of the Habakkuk thesis, has focused on capital and its availability for technological investments in the 1850s and has argued by implication that American labor must have been nearly as cheap as British labor. Temin observed that American and British capital were equally scarce—a fact suggested by the similar level of interest rates. Accordingly, British and American firms, having access to similar technologies, would have industrialized at equal rates if labor costs, like capital costs, were identical. But most American industries used demonstrably more machinery than equivalent British industries.³ Although a logical deduction from Temin's evidence would be that American labor was cheaper than British labor, this possibility has met with considerable resistance on the part of economists. Regarding low-priced American labor as an impossible proposition, they have tried instead to sustain

² The most complete and still the most provocative statement is Habakkuk, *American and British Technology in the Nineteenth Century*.

³ Temin, "Labor Scarcity and the Problem of American Industrial Efficiency in the 1850s," *Journal of Economic History* [hereafter, *JEH*], 26 (1966): 277–98.

Habakkuk's thesis by constructing elegant if implausible models rationalizing the abundance of American capital (hence, its cheapness vis-à-vis labor), proposing dual scarcities of capital and labor, or positing a three-factor model wherein the abundance of American raw materials permitted waste in processing and, hence, the use of inefficient machines and expensive labor.⁴ Temin's critics have one thing in common: they deny out of hand the possibility of an inexpensive labor supply.

Habakkuk's views on technological innovation during the first three decades of the nineteenth century, when American manufacturers began investing capital in the new machine technologies, have also been disputed. As with Temin's critique for the 1850s, the accumulating evidence for this earlier period reveals more similarities in than differences between English and American wages, thus weakening the explanatory power of labor costs. Don Adams's careful compilation of American and English wages for 1790–1830 contradicts Habakkuk's thesis on almost every essential. Specifically, Adams demonstrated that American agricultural wages were comparable to those of England and that the wage differential between skilled and unskilled labor, though similar in both countries, was generally wider in the United States. And Nathan Rosenberg's research on the cotton textile industry in the mid-1820s reveals a parallel pattern. His evidence also shows that wage differentials between the skilled workers (mule spinners and hand-loom weavers) and the unskilled were, if anything, wider in the United States than in England.⁵ Had American capitalists applied Habakkukian logic, they should have used their relatively cheaper unskilled labor more intensively and thus slowed the pace of machine diffusion. Since they did not pursue such a course, their behavior remains unexplained.

The objections raised by Temin, Adams, and Rosenberg have approached a low-cost labor interpretation of the antebellum United States, but each has stopped short of explaining its causes. Equally important, their work has not accounted for industrialization—that is, the United States's preoccupation with machine technology in the years after 1800. The thrust of their criticism has been largely negative: it has obliterated substantial differences in American and English wage rates and seriously undermined Habakkuk's thesis that American labor was expensive, but it has left us without an interpretation of comparative industrialization. Indeed, from the data adduced on comparative wage rates, we might conclude that American labor was as abundant as English labor and, therefore, that we should expect similar factor combinations of labor and capi-

⁴ D. L. Brito and Jeffrey G. Williamson, "Skilled Labor and Nineteenth-Century Anglo-American Managerial Behavior," *Explorations in Economic History* [hereafter, *EEH*], 2d ser., 10 (1972): 235–51; and Paul A. David, "Labor Scarcity and the Problem of Technological Practice and Progress in Nineteenth-Century America," in Paul A. David, ed., *Technical Choice Innovation and Economic Growth: Essays on American and British Experience in the Nineteenth Century* (London, 1975), 19–91. Also see Robert Fogel, "The Specification Problem in Economic History," *JEH*, 27 (1967): 283–308.

⁵ Adams, "Wage Rates in the Early National Period: Philadelphia, 1785–1830," *JEH*, 28 (1968): 404–26, and "Some Evidence on English and American Wage Rates, 1790–1830," *ibid.*, 30 (1970): 499–502; and Rosenberg, "Anglo-American Wage Differences in the 1820s," *ibid.*, 27 (1967): 221–29. For a dissenting view, suggesting narrower skilled-unskilled margins on Pennsylvania iron plantations, see Jeffrey F. Zabler, "Further Evidence on American Wage Differentials, 1800–1830," *EEH*, 2d ser., 10 (1972): 109–17.

tal. If these were the controlling economic realities, then the capital intensity of American industry would remain impervious to rational analysis. Wage rates, however, particularly for agricultural wages that form the foundation of Habakkuk's thesis, are very misleading. A careful examination of American agriculture and its English counterpart shows that the doctrine of American labor scarcity is untenable.

ANTEBELLUM AGRICULTURE WAS SEASONAL. Unlike manufacturing, which customarily operated most of the year, the agricultural season and the demand for labor varied remarkably from one crop region to another. Regional staples determined whether laborers were hired for a few days, a few months, or the entire year. The length of the crop season was critical in establishing the employment possibilities and the earnings of the rural laborer. Thus, it is misleading to use daily or monthly wage rates as proxies for rural earnings when comparing farming systems so radically different from each other as those under discussion, for the convention of calculating farm labor earnings by multiplying monthly wage rates by twelve (or daily wage rates by 311 working days) seriously distorts fundamental differences in contrasting systems. Agricultural labor's earnings are more accurately determined by multiplying the number of days or months a typical laborer worked by the daily or monthly wage rate.⁶ In the Middle Atlantic and Midwestern states, grain farming was the principal business. Yearly contracts for labor were rare, except when farms were opened, sod was busted, and timber was cleared. From the mid-eighteenth century down to 1860, farm labor was seasonal. In wheat areas, farm boys were hired for ten days to two weeks during harvest. Sometimes the urgency to locate labor produced a sense of panic as farmers confronted the potential disaster of having much of their crop over-ripened and dispersed before it could be harvested. Once ripened, wheat required harvesting within a maximum of ten days to two weeks. Notices such as the following, which appeared on the front page of the *Chicago Press and Tribune*, were typical: "Laborers wanted. We learned that farmers just now are greatly in need of laborers to gather their crops. Here is a chance for every idle man in the city. Take almost any road west, south, or north and in a single day, every man who wants work can find it." Similarly, in mixed grain areas, labor customarily worked ten weeks to four months, usually in the period from plowing and corn planting in the spring through corn tillage and on to the July harvest of small grains. Four months constituted the usual term for farm laborers in the grain belt, and during the rest of the year they were underemployed or unemployed.⁷

Similar circumstances prevailed wherever wheat and corn had become major commercial staples. Just before the Civil War, a small farmer explained to Fred-

⁶ For this point, see Carville V. Earle, "A Staple Interpretation of Slavery and Free Labor," *Geographical Review*, 68 (1978): 52-65.

⁷ See Allan G. Bogue, *From Prairie to Corn Belt: Farming on the Illinois and Iowa Prairies in the Nineteenth Century* (Chicago, 1968); David E. Schob, *Hired Hands and Plowboys: Farm Labor in the Midwest, 1815-1860* (Urbana, Ill., 1975); and Paul W. Gates, *The Farmer's Age in Agriculture, 1815-1860* (New York, 1960). Also see other sources cited in Earle, "A Staple Interpretation of Slavery and Free Labor," 52-65.

erick Law Olmsted how this labor system operated. The "poor white people" and "rural mechanics," he related, "hardly ever worked on farms except in harvest, when they usually received a dollar a day, sometimes more. In harvest-time, most of the rural mechanics closed their shops and hired out to the farmers at a dollar a day. . . . At other than harvest-time, the poor white people, who had no trade, would sometimes work for the farmers by the job; not often at any regular agricultural labor, but at getting rails or shingles, or clearing land."⁸ Hence, if labor's earnings were principally confined to several months, then annual earnings obviously amounted to substantially less than the amount that could be earned in other farming systems employing year-round labor.

By contrast, English agriculture in the early nineteenth century was labor intensive with farm laborers employed most of the year in the tasks of marling, manuring, plowing and tilling the arable soil, and tending livestock. The English agricultural historian Eric L. Jones has admirably described the increasing demand for labor on English farms as the agricultural work year was extended:

The seasonal work-rhythm in agriculture was most evident on the larger arable farms. The crop-year means brisk activity sowing the cereals in autumn and spring, frantic haste and urgency at harvest, and comparative somnolence in between. Threshing was virtually the only winter task of any importance. During the eighteenth and nineteenth centuries the starkness of this rhythm was tempered by the new rotations which gained ground. Of these, the Norfolk four-course was the arch-typal but by no means the sole pattern. Its tight sequence of wheat, turnips, barley, clover, then wheat again, its arable flock and its yarded bullocks suggest the directions in which most of the new systems created jobs. Sowing, hoeing and singling the root crops, hurdling the sheep, feeding the fattening bullocks through the winter went some way towards the evolution of a farming system in which work was available, in a series of short bursts, most of the year through.⁹

⁸ Olmsted, *A Journey in the Seaboard Slave States* (New York, 1856), 82-83.

⁹ Jones, *Seasons and Prices: The Role of the Weather in English Agricultural History* (London, 1964), 60-61. In studying the agricultural history of England from the mid-eighteenth to the mid-nineteenth century, certain fundamental factors must be kept in mind. First, between 1750 and 1840 the population of England and Wales more than doubled, while the increase in the home production of cereals, though dramatic, was slightly less than two-fold. Second, because substantial imports from outside the United Kingdom were not possible, the nation remained largely dependent on its indigenous agricultural economy for sustenance. Third, and most important for our argument, the era of parliamentary enclosure, which began in 1750, did not produce a decline in rural population. On this point there is some consensus, and few scholars would seriously quibble with Eric L. Jones, who has claimed that England's agricultural labor supply grew from 1.7 million in 1801 to 2.1 million in 1851. Similarly, geographer J. A. Yelling recently concluded, "What is certain is that the general effect of parliamentary enclosure on village population totals was far from catastrophic; and that although certain aspects of the process favored economy of labour, the broad package of land-use changes that occurred in most parishes continued to support a high level of employment. It would be fair to add that this occurred at a time of rapid upsurge in rural populations when employment was expanding in common-field parishes also, and in neither case is it easy to see precisely how the extra jobs were found." Yelling, *Common Field and Enclosure in England, 1450-1850* (Hamden, Conn., 1977), 226. Yelling's observation is well taken—no one can yet say in any definitive fashion how England's rural population maintained itself. Several excellent studies, however, do suggest how the agricultural work year and its concomitant labor requirements expanded markedly between 1750 and 1840. Two factors are stressed as fundamental in creating the economic foundation that emerged in the English countryside: the physical process involved in enclosure and the introduction of new crops. In two important essays, J. D. Chambers has demonstrated how the enclosure effort dramatically increased the amount of land under cultivation, a development that, he emphasized, required substantial additional work to bring the virgin soil into production. Chambers has also suggested how a pattern of high, year-round employment was instituted by the introduction and spread of turnip cultivation and green fodder crops and the heightened concentration on livestock, all of which "called for labour throughout the year in field, barn and stockyard." Chambers, "Enclosure and Labour Supply in the Industrial Revolution," *Economic History Review* [hereafter, *EHR*], 2d ser., 5 (1953): 319-43, and "The Vale of Trent, 1670-1800," *ibid.*, Supplement 3 (1957): 2-63. In a prize-winning essay examining England's farm economy in this period, C. Peter Timmer has advanced convincing quantitative support for Chambers's general arguments. Specifically, Timmer contended

In some areas of England, laborers worked on lengthy contracts of six months or a year; in others, particularly in the south of England, rural workers stitched together employment from day work, piece work, and harvest work; but, in virtually all of England, the average farm laborer found employment for most, if not all, of the year.¹⁰ Thus, in the 1820s, English farm workers probably earned about \$170 and board for the year, while American laborers in grain-farming districts counted on about \$40, out of which they had to deduct room and board for the off eight months.¹¹ Presumably, many found some additional

that England's augmented agricultural production resulted from the diversification created by growing turnips and grasses and by the accompanying attention paid to husbandry. But he observed that—and this is absolutely critical—the new agricultural form did not include a corresponding expansion in worker output: "Even if a 50 per cent increase in yield was achieved on the new farm," he wrote, "the increase in output *per worker* was nearly nil. The English agricultural revolution increased land, not labor, productivity." In essence, the growth in commodity production resulted from an increase in the amount of arable land and the number of agricultural laborers. To substantiate his interpretation, Timmer provided a comparison of the labor requirements on English farms that did and did not practice the new agriculture. The significance of his conclusion is clear: "The new farm needed two-fifths more labor on a year-round basis, but it did not put any added pressure on labor supplies in the critical harvest period." Timmer, "The Turnip, the New Husbandry, and the English Agricultural Revolution," *Quarterly Journal of Economics*, 83 (1969): 375–95, esp. 392, 394.

¹⁰ From the perspective of the agricultural laborer, of course, the basic importance of the changing economy of the English countryside involved two related considerations: how secure were the prospects for employment and what level of annual income could be expected? These are indeed complex issues. The years 1790–1840 were, as E. J. Hobsbawm and George Rudé described in *Captain Swing* (London, 1969), a time of transition and tension in rural England as the old system of annual labor contracts was replaced by a new one under which labor was hired daily, weekly, or monthly. Examples of both practices abound in the literature of and on the period. In Scotland, Wales, and the north of England both contemporaries, such as James Caird in *English Agriculture in 1850–51* (London, 1852), and modern scholars, such as Christabel S. Orwin and Edith W. Whetham in their *History of British Agriculture, 1846–1914* (London, 1964), have recorded the continuation of annual and semi-annual contracts specifying much of the payment in kind—a situation that may or may not have worked to the laborer's benefit. By contrast, England's richer agricultural regions introduced a bewildering array of new hiring methods. Some workers functioned under the older arrangements. Others, by preference or coercion, labored under a wage-rate system that varied by season and task. Still others worked exclusively on a piece-rate basis. The employment patterns differed not only between regions but within regions and even within counties as well. One investigation completed in 1838 reveals, for example, that in each of Bedfordshire's twenty-six parishes a different wage-rate system existed; G. R. Porter, "Agricultural Queries, with Returns from the County of Bedford," *Journal of the Statistical Society of London* [hereafter, *JSSL*], 1 (May 1838): 89–96. But, again, the important issue to be addressed is what this variegated labor market meant in terms of the incomes of agricultural laborers and the availability of reliable employment. Several conclusions seem clear. First, rural workers adjusted reasonably well and were not impoverished—at least not until the mid-1840s, when the flight from the countryside began. Second, they found virtually year-round work—R. A. C. Parker's study of the wheat, barley, turnip, and livestock farms contains clear evidence that labor was hired for forty-eight weeks of the year; Parker, *Coke of Norfolk: A Financial and Agricultural Study* (Oxford, 1975). Third, and this point is essential to determining annual family income, the hiring of labor was not restricted to the male heads of households; women and children were also employed. The results of this practice, particularly in the major agricultural centers of England (the southern and eastern counties), were substantial, and one investigation of the moderately rich counties of Norfolk and Suffolk in the 1830s underscores its importance. Single men or families where the wife did not work had annual incomes of \$120 to \$130. By contrast, families with one or more children above the age of ten who worked—and in this sample there were a sizable number, 273 of 539, or 50.6 percent—earned from \$140 to \$200. See James Phillips Kay, "Earnings of Agricultural Labourers in Norfolk and Suffolk," *JSSL*, 1 (1838): 179–83. As a writer from Dorsetshire observed somewhat later (and in a far more depressed agricultural period), any discussion of the income of a farm laborer must at minimum take into account, "Whether his children living with him do not earn nearly or entirely their own maintenance"; "Wages in Dorsetshire," *JSSL*, 22 (1859): 521–22. With virtually all available evidence pointing to a lack of extensive emigration or "flight" from the countryside, to year-round employment and satisfactory annual incomes, it is apparent that from 1800 to 1840 the rural English agricultural laboring family had a viable economic position. Also see A. L. Bowley, "The Statistics of Wages in the United Kingdom during the Last Hundred Years—Part I: Agricultural Wages," *Journal of the Royal Statistical Society*, 61 (1898): 702–22.

¹¹ On British labor and wages, see Adams, "Some Evidence on English and American Wage Rates," 505–06; H. C. Carey, *Essay on the Rate of Wages* (New York, 1965), 91; and Eric L. Jones, *Agriculture and the Industrial Revolution* (New York, 1974). On the high incidence of yearly contracts, see Hobsbawm and Rudé, *Captain Swing*.

work; but, clearly, labor in the American grain belt was cheap, and it did not take a very high urban salary to induce these unskilled workers from rural to permanent urban employment. Here was a vast source of inexpensive labor convenient for exploitation by American entrepreneurs.

Urban wages were thus tightly enmeshed with the regional agricultural economy. The key mechanism determining urban wages, as elucidated by development economists, is the transfer wage. Simply put, the transfer wage indicates the wages for unskilled urban labor (or what amounts to the same thing—its cost) for a particular farming region and represents the annual wage that will induce unskilled rural labor to seek urban jobs. That wage may be found by solving the following formula for unskilled urban wages: $W_u - W_r = C_u - C_r$, where W_u is the annual urban wage or earnings, W_r is the amount of rural earnings, and C_u and C_r are the living costs in city and country. When urban earnings were greater than the sum of the living costs in the city and the rural earnings, less, of course, the living costs of the countryside (when, that is, $W_u - W_r > C_u - C_r$), migration from rural to urban jobs became more attractive. Just how great this inequality had to be is problematic, because the nonpecuniary income attached to rural life increased substantially the value of rural wages alone—that is, W_r was usually higher than the cash value of rural earnings by 30 to 40 percent.¹² For purposes of clarity, the transfer wage without nonpecuniary income as part of the calculation will be denoted as W_u and with nonpecuniary income as W_u' .

The transfer wage is a product of labor markets and their imperfections. Although antebellum rural labor markets were competitive with the conditions of supply and demand largely responsible for setting the cost of labor, they were frequently imperfect with either more buyers or more sellers, as Stanley Lebergott has observed.¹³ Imperfections were especially common in seasonal farming districts like the American grain belt. In effect, the labor market was a hybrid, perfectly competitive during the crop season and oligopsonistic or even monopsonistic during the off-season. In the farm season, farmers (buyers) and laborers (sellers) were numerous, demand was inelastic, and daily wages were high. Conversely, the off-season market was glutted with underemployed laborers and few buyers. This hybrid labor market fixed the supply curve of urban labor at the transfer wage. On the demand side, the handful of urban entrepreneurs remaining in the labor market exerted considerable influence on wage determination. These firms paid wages that were not only low but also less than marginal product, thereby causing exploitation in the form of a capitalist's surplus.¹⁴ The capi-

¹² The model is similar to the formula for the slavery-free labor inequality devised by Evsey Domar; see Domar, "The Causes of Slavery or Serfdom: A Hypothesis," *JEH*, 30 (1970): 18–32. On nonpecuniary income, see W. Arthur Lewis, "Reflections on Unlimited Labour," in Luis Eugenio DiMarco, ed., *International Economics and Development: Essays in Honor of Raul Prebisch* (New York, 1972), 75–96; Gerald M. Meier, *Leading Issues in Economic Development* (3d ed., New York, 1976), 158; and especially A. J. Fonseca, *Wage Issues in a Developing Economy: The Indian Experience* (Bombay, 1975), 1–30. Note that the transfer wage operates within both perfect and imperfect labor markets, and its operation affects the incomes of all who seek urban work, native or immigrant.

¹³ Lebergott, *Manpower in Economic Growth: The American Record since 1800* (New York, 1964), 164–90. Also see the informative observations on labor markets in Richard A. Lester, *The Economics of Labor* (New York, 1946), 93–127.

¹⁴ For a graphical representation of this situation, see Graph 1, page 1076, below; and David, "Learning by

talist's surplus equaled the sum of money saved by the employer through the payment of a wage less than that which would be required in a perfectly competitive market—that is, a market where the demand for labor that existed during the crop season prevailed throughout the entire work year.

We recognize that rural labor in the United States rarely worked in a twelve-month employment cycle. The norm was nine to ten months in the South and four to six months in many of the Mid-Atlantic and Midwestern states. Our purpose in using a year-round, perfectly competitive rural labor market as a benchmark is to illustrate the critical interrelationship of rural earnings and urban wage rate determination. More precisely, unskilled urban wages were determined by the earnings of rural labor, which were in turn established by the seasonality of employment and the market imperfections that existed during the off-season. If the crop year were extended, the level of annual income necessary to employ unskilled urban labor rose and, conversely, if the crop year were shortened, the annual wages offered urban laborers declined. The disparity, therefore, between what the rural laborer could have earned if employed for twelve months and his rural earnings for four, six, or ten months—what we have defined as exploitation resulting from seasonality—was critical in establishing the upper bound of the urban wage rate, which, in most cases, was paid on the basis of twelve months of employment. In the long run, it should be noted that exploitation diminished. It did so precisely because, as new firms entered the market to take advantage of low-cost labor, they eventually created a competitive year-round labor market with numerous firms competing for idled rural labor. Or, expressed differently, the level of exploitation dwindled as urban labor markets themselves became perfectly competitive.¹⁵

Within the hybrid labor market established by seasonal agriculture, the transfer wage was the main, though not the exclusive, strategy used by urban firms to attract laborers. It was the preferred strategy for firms that wanted a permanent, year-round labor force, which they could acquire by offering annual urban wages at a level only slightly above that of seasonal rural earnings. Alternatively, firms in seasonal industries favored a second strategy. Operating pri-

Doing and Tariff Protection: A Reconsideration of the Case of the Ante-Bellum United States Cotton Textile Industry," in David, *Technical Choice Innovation and Economic Growth*, 147. David, however, has attributed market imperfections to year-round monopsony of a single firm in an isolated labor market. The seasonal imperfections suggested here are more general in their operation.

¹⁵ Our notion of exploitation as a function of seasonality, since it departs from standard usage in economics, requires brief explanation. We employ the term to focus on the labor market that permitted entrepreneurs to pay low wages because of the unequal demands for labor in the rural and urban sectors. The cost of labor in the urban sector was determined by the work requirements of agriculture. More specifically, the annual incomes earned by rural labor—a product of seasonality—were the prime determinant of urban wages and, therefore, yearly urban earnings. Some neoclassical economists will undoubtedly object to our method of calculating exploitation on the basis of year-round work. Their argument is that, since seasonality was a normal condition during the nineteenth century, labor markets were, therefore, perfectly competitive. Moreover, by denying the impact of seasonality, they contend that neither market imperfections nor exploitation commonly occur in developing staple economies. In contrast, we are obviously convinced that this neoclassical interpretation does not coincide with the reality of labor market functioning during the nineteenth century, when the key determinant was rural incomes rather than "marginalist" economics. Unskilled urban workers were paid not according to their productivity but in accordance with what they could have earned in their chief alternative employment—namely, rural labor. When seasonality truncated these earnings, the result was the structuring of a labor market that served both to exploit labor and to create a capitalist's surplus that was used as a subsidy for industrialization.

marily during the imperfect labor market that prevailed through fall and winter, they could push wages down toward subsistence. A third strategy was noneconomic in nature. For reasons of paternalism or equity, some firms paid wages that were greater than subsistence or transfer wages. Each strategy, nevertheless, involved some degree of exploitation as the following hypothetical examples make clear. Consider the case where farm laborers earned \$10 per month during a four-month crop season. Suppose also that the year-round urban firm attracted rural workers by paying them a transfer wage of \$60. Had perfect labor markets existed throughout the year, the firm would have paid labor \$120. The absolute difference of \$60 was the capitalist's surplus, and the relative difference of 50 percent was the degree of exploitation caused by market imperfections. Consider now the subsistence wage strategy of the firm that hired only temporary labor during the fall and winter seasons. Meat packers, woodlot operators, and iron millers, for example, frequently paid little more than the cost of subsistence in the glutted labor market. Suppose the monthly subsistence wage was \$3, then the capitalist's surplus equaled \$7 per month and exploitation amounted to 70 percent. In theory, firms that paid wages in excess of subsistence or the transfer wage did so for noneconomic reasons. They reduced their potential profit while lowering labor's exploitation. Precisely which strategy firms pursued is a matter for empirical determination. The following discussion focuses on the transfer wage as the main strategy used in the reshaping of the antebellum American economy.¹⁶

IN TERMS OF UNDERSTANDING THE EFFECT of the transfer wage, it is important, at the outset, to underscore the distinctly confined impact exerted by immigration on the operation of the antebellum labor market. During the first half of the nineteenth century, the immigration of some four million people to New England and the Middle Atlantic and North Central states certainly contributed toward expanding the cheap labor pool of those sections. But the major mechanism of seasonality strictly limited the range of potential market consequences. Immigrants went mainly to cities, presumably because of better employment opportunities afforded by year-round work as opposed to seasonal rural labor. Most of them made their original destination an urban center or moved to the city shortly after their arrival in a rural area, thereby increasing in some sections the already ongoing rural to urban movement. This pattern had the effect of depressing the cost of urban labor, although clearly it did not reduce it to the level where rural employment became a viable or attractive alternative. Conversely, and significantly, if immigration had not occurred, urban wages would not have risen appreciably: the determinant of seasonality would have continued to establish the level of compensation that rural employers could afford to

¹⁶ Lester, *The Economics of Labor*, 93-127; and Joan Robinson, *The Economics of Imperfect Competition* (London, 1954). For thinking on the effects of seasonality on economic development in the European context, see Franklin F. Mendels, "Proto-Industrialization: The First Phase of the Industrial Process," *JEH*, 32 (1972): 241-61.



Figure 1: Mid-nineteenth-century representation of the wheat harvest. The laborers in this scene used the scythe, which, though still common in the 1850s, was being replaced by the cradle and the mechanized reaper. Reproduced from *Harper's New Monthly Magazine*, 15 (1857): 307.

extend. The urban entrepreneurs would still have been likely to offer relatively low wages—wages only somewhat higher than total rural income—to induce migration from the countryside. Expressed succinctly, since the cost of rural labor was determined primarily by the forces of seasonality, immigration had minimal effect in lowering the transfer wage by narrowing the differential between the annual earnings of rural and urban labor.¹⁷

The transfer wage during the nineteenth century varied considerably among

¹⁷ A caveat is in order before proceeding to the regional comparisons. Ours is a model of regional wage determination for unskilled men; it should not be confused with a model of migration. A fully specified model of migration obviously must take into account the larger unit of the household, its earnings, and its cost of maintenance. Elsewhere, Carville V. Earle has developed such an economic model of migration that resolves the seeming paradox of migration to the low-wage Midwest rather than to the high-wage South; see Earle, "Interpreting the Size of Cities and City Systems: Agricultural Economy and Urban Labor in the Chicago and Mobile Urban Systems during the 1850s," paper presented at the Social Science History Association Meetings, held in Cambridge, Mass., November 1979.

American regions. The greatest difference existed between the Northern grain belt and the cotton South. For illustrative purposes, the grain-farming regions in northern and central Illinois and the cotton-planting agriculture of Alabama during the 1850s may be compared: Illinois rural earnings reflected the seasonal underemployment of farm labor while Alabama earnings represented year-round rural employment. In Illinois, farmers hired labor for short terms of ten weeks to four months. Hiring peaked in the period from spring plowing and corn planting in April through corn tillage in May and June and the harvest of small grains in July. A typical laborer's season brought wages of \$14 to \$18 per month for four months, or annual earnings of \$56 to \$72. During the remainder of the year, sporadic and unpredictable labor demands lowered wages and increased prospects for unemployment. Rural workers also faced steep living costs. Although they received room and board for the four months they were employed, they "found themselves" paying out about \$48 of their summer earnings in the course of the remaining eight months.¹⁸ Costs of living in the urban areas of the region were higher than rural expenses. Most evidence suggests that it cost a single male \$165 to live in the region's cities. Entering these figures into the formula for ascertaining the transfer wage ($W_u - W_r = C_u - C_r$), the transfer wage based on earnings alone (W_u) was roughly \$173 to \$189 and based on earnings plus nonpecuniary income (W_u') was roughly \$190 to \$211—that is,

$$\begin{aligned} W_u &= \$165 - \$48 + (\$56/\$72) & W_u' &= \$165 - \$48 + 130\%(\$56/\$72) \\ W_u &= \$173/\$189 & W_u' &= \$190/\$211. \end{aligned}$$

Rural to urban migration was theoretically possible, then, when the Midwestern urban wage was between \$190 and \$215.¹⁹

In the South, cotton established a more demanding regimen and a higher transfer wage. The crop's long gestation required sustained attention during the growing season, and this made free labor prohibitively expensive. White labor costing over \$100 per year simply could not compete with slaves costing \$50 to \$60.²⁰ But rural life had attractions for young white men capable of driving slaves and overseeing plantations. These year-round jobs promised rapid capital

¹⁸ Schob, *Hired Hands and Plowboys*, 188; Paul A. David, "The Mechanization of Reaping in the Ante-Bellum Midwest," in Henry Rosovsky, ed., *Industrialization in Two Systems: Essays in Honor of Alexander Gerschenkron by a Group of His Students* (New York, 1966), 3-39; and Earle, "A Staple Interpretation of Slavery and Free Labor," 60.

¹⁹ Figures for living costs are based on those for an adult in Eastern manufacturing cities in 1860 and adjusted for regional differences, since regional costs of living in the Midwest equaled or exceeded those in the Northeast. See *Report of the Special Commissioner of the Revenue for the Year 1868*, 40th Cong., 3d Sess., HR Exec. Doc., no. 16 (Washington, 1869), 122; and Philip R. P. Coelho and James F. Shepherd, "Differences in Regional Prices: The United States, 1851-1880," *JEH*, 34 (1974): 551-91. Nonpecuniary income is not a residual measure; rather, it is based on empirical observation by development economists. The theoretical basis for nonpecuniary income's proportion of rural earnings is less well established; presumably, its level is set by comparative rates of return in rural and urban areas. See Lewis, "Reflections on Unlimited Labour," 75-96; and Meier, *Leading Issues in Economic Development*, 158.

²⁰ On cotton generally, see Lewis C. Gray, *History of Agriculture in the Southern United States to 1860*, 2 vols. (new ed., Gloucester, Mass., 1958). For the seminal work on slave costs, see Alfred H. Conrad and John R. Meyer, "The Economics of Slavery in the Ante-Bellum South," in Robert W. Fogel and Stanley L. Engerman, eds., *The Reinterpretation of American Economic History* (Evanston, Ill., 1971), 342-61.

accumulation and economic mobility. Annual pay was substantial for an overseer in the cotton belt of Alabama and Mississippi, where the average wage was about \$400 and the range from \$200 to \$1,000. Assuming a starting pay of \$300, along with fringe benefits covering living costs ($C_r = 0$), the first-year overseer did well.²¹ The bidding level for urban firms seeking labor was exorbitantly high. Southern merchants and manufacturers had to offer a starting pay of \$300 plus an additional sum of perhaps \$150 to cover living costs in the town or city. These costs placed the transfer wage (W_u) at \$450 and, to overcome the non-pecuniary income attached to rural earnings, the transfer wage (W_u') at \$540.²² Unskilled labor was, therefore, less expensively induced out of the countryside in Illinois than in Alabama, or, to put it in different terms, urban labor was twice as expensive in the South as in the North because of the protracted demands of the cotton economy.

The overseers were not atypical; their considerable earnings accurately reflected the equally good fortune of the lesser planters and nonslaveholding whites during the 1850s. These small planters accounted for over 80 percent of the cotton South's heads of households. Like the large planters, these small producers derived their earnings primarily from growing cotton. Throughout the cotton belt, 75 to 80 percent of the nonslaveholders raised cotton, and, in the Black Belt of Alabama and Mississippi, over 90 percent did so.²³ Almost every white family participated in the gamble of cotton, and conditions favored their success. Although slave prices had spiraled beyond the reach of these small producers, the costs of land, family labor, and capital were minimal, especially when cotton prices pushed above \$0.10 per pound. For labor, the small white planter used his own and that of members of his family. As Frederick Law Olmsted observed, "I have, in fact, seen more white native American women at work in the hottest sunshine in a single month, and that near midsummer, in Mississippi and Alabama than in all my life in the free States, not on account of an emergency, as in harvesting either, but in the regular cultivation of cotton and of corn, chiefly of cotton."²⁴ The small planter obtained land by leasing it or buying it as cheaply as \$2 an acre. For capital, he needed only a one-horse plow, which was primitive in the extreme and of little expense, and a horse or mule, which were expensive but could be purchased on a year's credit, with payments beginning after the sale of the first crop. By driving himself and his relations and by "busting his ass," the aspiring planter raised a good crop of cotton, perhaps as much as ten bales and certainly as much as eight bales. At \$0.10 per pound, his gross revenue ranged roughly from \$320 to \$400.²⁵ Incomes of this magnitude

²¹ William Kauffman Scarborough, *The Overseer: Plantation Management in the Old South* (Baton Rouge, 1966), 3-66.

²² The cost of living in Southern cities is estimated at \$150 by adjusting the cost in Eastern manufacturing cities (\$165) by regional price indices from Coelho and Shepherd, "Differences in Regional Prices," 570.

²³ On the cotton production of nonslaveholders, see Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978), chaps. 2, 3.

²⁴ Olmsted, *A Journey in the Back Country* (New York, 1860), 298.

²⁵ Olmsted is informative on every point made here, but also see Robert R. Russel, "The Effects of Slavery upon Nonslaveholders in the Ante Bellum South," *Agricultural History*, 15 (1941): 112-26; and Charles S. Davis, *Cotton Kingdom in Alabama* (Montgomery, Ala., 1939), 154-55, 67. On mules, see Robert B. Lamb, *The Mule in*



Figure 2: Mid-nineteenth-century representation of cotton picking. Although slaves are depicted here, the task was the same for the small planter and his family. The rarity of illustrations showing whites in the cotton fields is a commentary on the skewed antebellum perception of the cotton South. Reproduced from *Harper's New Monthly Magazine*, 8 (1854): 456.

show quite clearly why overseers' earnings were high. They also explain the attachment of "poor whites" to the country rather than the city.

The transfer wage serves as the theoretical mechanism for wage determination. What is more, the transfer wage established regional bidding floors for unskilled and semi-skilled labor that were low in the Midwest and high in the South. In Chicago, for instance, unskilled labor working for the McCormick Reaper Company in the late 1850s earned \$0.06 per hour for ten hours a day, six days a week, or \$186.60 for the year's work. Indeed, this figure falls within the lower range of the transfer wage and suggests that nonpecuniary income in rural areas was of very little significance during the late 1850s. Certainly the depression of 1857 and the several lean years thereafter made farm labor unattractive and capital accumulation unlikely. Similarly, the Illinois Central Railroad drove wages down to \$0.80 per day (\$248.80 per year), and the company made every effort to reduce wages further at decade's end.²⁶

Southern Agriculture, University of California Publications in Geography, no. 15 (Berkeley and Los Angeles, 1963), 17-29.

²⁶ Robert Ozanne, *Wages in Practice and Theory: McCormick International Harvester, 1860-1960* (Madison, Wisc., 1968), 3-21. The company usually boosted wages in the spring, when in competition with farmers, and

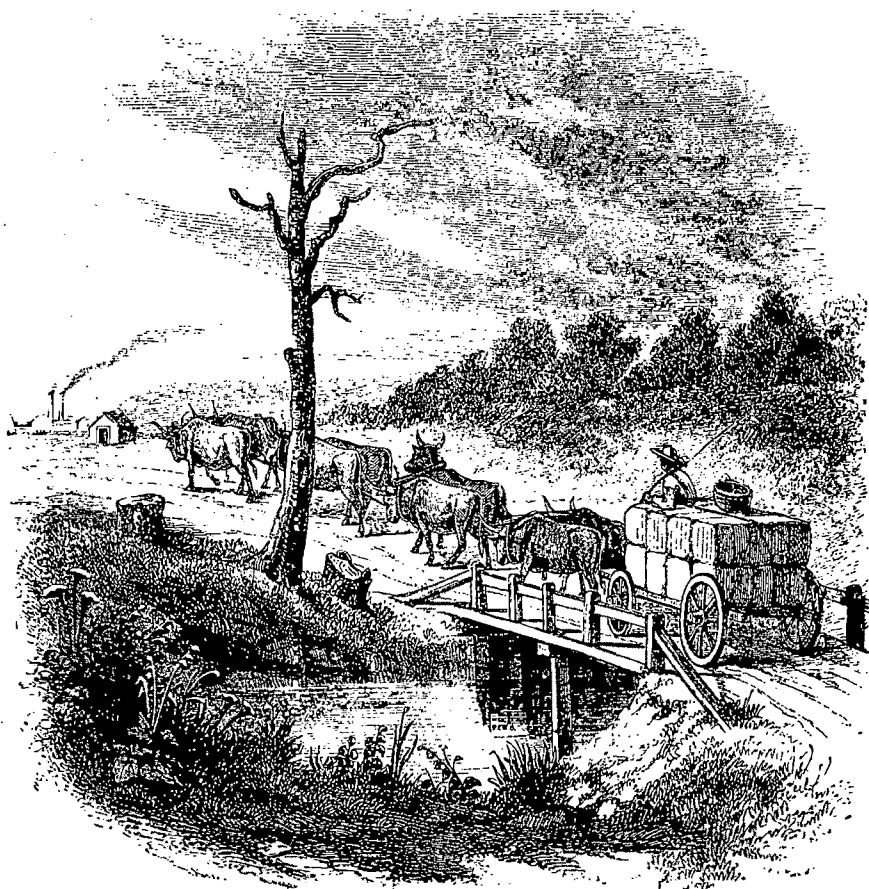


Figure 3: Mid-nineteenth-century representation of cotton hauling from the plantation to the river landing, from which the cotton was shipped by steamboat. Note that the wagon hauled eight bales of cotton, or roughly the output of one worker. Reproduced from *Harper's New Monthly Magazine*, 8 (1854): 460.

The low transfer wage not only in Illinois but throughout the Midwest involved a considerable degree of economic exploitation. Indeed, exploitation was inherent in a markedly seasonal staple economy with imperfect labor markets in the off-season. Compare, for instance, the actual earnings of unskilled urban labor with those earnings that workers would have received under competitive conditions such as those prevailing during the crop season. Monthly wages of \$14 to \$18 from spring planting to summer harvesting would have yielded annual incomes of \$158 to \$216 had employment been year-round. Adding in board at \$6 per month or \$72 per year, workers would have earned \$230 to \$288 in perfectly competitive markets. In practice, entrepreneurs paid somewhat less for year-round labor. The degree of exploitation by our rough measure was 18.9 to 35.2 percent for the McCormick Company and from 0.0 to 13.6 percent for the Illinois Central. The lower level of exploitation on the Illinois Central is attributable, perhaps, to the workers' ability to resist the wage cuts urged by company

dropped wages in the fall after harvest. For other examples of the smallness of unskilled labor's wages, see Bessie Louise Pierce, *A History of Chicago*, volume 2: *From Town to City, 1848-1871* (New York, 1940), 500; and David L. Lightner, *Labor on the Illinois Central Railroad, 1852-1900: The Evolution of an Industrial Environment* (New York, 1977), 22-29, 76-89, esp. 88-89.



Figure 4: Mid-nineteenth-century depiction of the logging industry—log-sawing—typical of Upper Michigan. Reproduced from Eminent Literary Men, *One Hundred Years' Progress of the United States* (Hartford, 1879), 92.

officials. McCormick, the Illinois Central, and other year-round firms were not the sole beneficiaries of low-cost labor. Equally important were those off-season industries that sought temporary labor between fall and spring. At these times, labor was even cheaper, and, theoretically, firms could have driven wages down toward subsistence. Such a strategy was feasible because the labor market contained few buyers and a glut of idle farm hands who drifted about the countryside and into the cities where they hoped to find work.

The sheer dimension of the army of rural laborers re-entering the labor market in late summer and fall has not been fully appreciated. Census takers in 1860 recorded over two hundred and forty thousand farm laborers in the Midwest—a number more than double the population of Chicago at the same date. In grain-growing rural areas, somewhere between 8 and 18 percent of the population were farm laborers, and they made up an even larger share of the labor force.²⁷ In wheat-raising Genoa Township in DeKalb County, Illinois, farm laborers and laborers accounted for 35 percent of the adult male labor force.²⁸ As the crop season ended in July and August, laborers remained in the country, hoping for work as teamsters for the crops they had just harvested, but by fall they began drifting into the cities and towns. They came to the cities, as Horace Greeley later remarked, “under a vague, mistaken impression that there must be work at some rate where so much is being done and so many require service, and squander their means and damage their morals in fruitless quest of what is not there to be had. When Spring at length arrives, they sneak back to the rural

²⁷ Schob, *Hired Hands and Plowboys*, 250–72.

²⁸ U.S. Census Office, Eighth Census of the United States, 1860, Manuscript Census Returns, DeKalb County, Illinois, Population Schedule, National Archives, Washington.

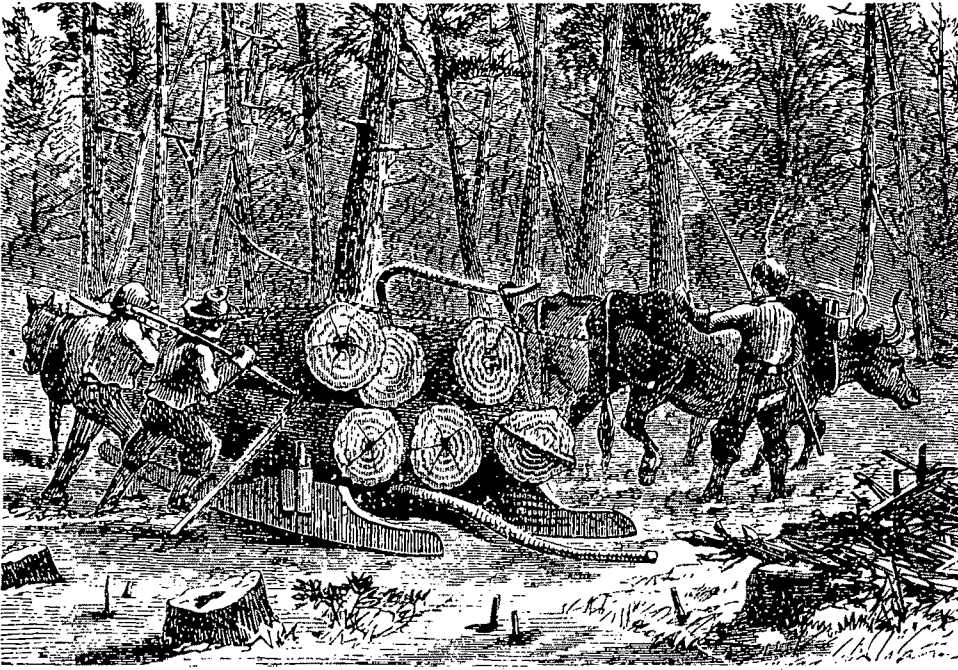


Figure 5: Mid-nineteenth-century representation of the logging industry—hauling logs—typical of Upper Michigan. Reproduced from Eminent Literary Men, *One Hundred Years' Progress of the United States*, 92.

districts, ragged, penniless, debauched, often diseased, and everyway deteriorated by the Winter plunge.”²⁹

Demobilization of the rural labor force was thus an immense social problem for the cities of the Midwest. Cincinnati, for instance, reportedly sheltered seventy-five hundred common laborers, or 6.5 percent of its total population, during the winter of 1851. Earlier in the 1840s, their numbers exceeded even the demand from the rapidly growing winter business of hog-slaughtering and pork-packing. Consequently, the city organized the Cincinnati Fire Wood Company specifically to make work for jobless laborers.³⁰ Chicago had its problems, too, and the *Tribune* expressed relief at the arrival of the 1860 harvest season and advised unemployed laborers to strike out into the countryside to the nearest wheat farm. What was a social problem for the cities was often a financial crisis for the laborer whose grim winter prospects a Wisconsin youth nicely summarized: “I have not found a place for the winter yet for it is rather hard to hire out to work by the month through the winter season Though I have not hired out for the winter I have worked about a month . . . and shall work some more either by the month or chopping some cordwood.”³¹

The glutted labor market was a windfall for firms operating mainly during fall and winter, as exemplified by the lumber industry of Michigan during the 1850s. Several thousand of Michigan’s thirty-five thousand farm hands moved

²⁹ Greeley, *What I Know of Farming: A Series of Brief and Plain Expositions of Practical Agriculture as an Art Based upon Science* (New York, 1871), 303.

³⁰ Schob, *Hired Hands and Plowboys*, 157–58.

³¹ Lucian Enos to his parents, Lafayette County, Wisconsin, October 31, 1843, Wisconsin Territorial Letter Collection, as quoted in Schob, *Hired Hands and Plowboys*, 150.

TABLE I
Wages of "Mechanics and Laborers," North and South

<i>Cities by Region</i>	<i>Painters</i>	<i>Bricklayers</i>	<i>Stone Masons</i>	<i>Carpenters</i>	<i>Plasterers</i>	<i>Laborers</i>
SOUTHERN STATES						
New Orleans	\$2.00-2.50	\$2.50-3.50	\$2.00-3.00	\$2.25-2.50	\$2.25-2.50	\$1.25-1.50
Richmond	1.75-2.25	2.00-3.00	2.00-2.50	1.50-2.00	1.75-2.25	1.00-1.50
Louisville	1.75-2.00	2.50-3.00	1.75-2.00	1.75-2.50	2.00-2.25	1.00-1.25
Galveston	1.75-2.00	2.75-3.00	2.00-3.00	2.00-3.00	1.75-2.25	1.25-1.50
Charleston	1.75-2.00	2.50-3.50	2.00-2.50	2.50-2.75	2.00-2.50	1.00-1.50
Little Rock	2.50-3.50	2.00-3.00	2.00-2.50	2.00-3.00	2.50-3.00	1.00-1.25
Norfolk	1.75-2.00	2.00-2.50	2.25-2.50	1.50-2.00	1.75	1.00-1.25
Memphis	2.00-2.50	2.00-3.00	2.00-2.50	2.25-2.50	1.75-2.50	1.00-1.50
Nashville	2.25-2.50	2.50-3.00	2.00-2.50	2.25-2.50	2.00-2.50	1.00-1.25
NORTHERN STATES						
Chicago	\$1.50-1.75	\$1.75-2.00	\$1.50-2.00	\$1.25-1.75	\$1.50-2.00	\$0.50-1.00
Pittsburgh	1.50-2.00	1.75-2.00	1.50-1.75	1.25-1.75	1.50-1.75	0.75
Cincinnati	1.50-1.75	2.00-2.50	1.25-1.50	1.00-2.00	1.50-1.75	0.75-1.00
Detroit	1.50	2.00	1.50	1.75	1.50	0.88
Columbus, Ohio	1.50	2.00	1.50	1.50-2.00	1.75-2.00	0.75-1.00
Buffalo, N.Y.	1.50-2.00	1.50-2.00	1.50-1.75	1.00-1.50	1.50-1.75	0.50-0.75
Lowell, Mass.	1.00-1.75	1.50-1.75	1.50-2.00	1.25-1.75	1.00-1.50	0.75-1.00
Bangor, Maine	1.50-2.00	1.50-2.00	1.50-2.00	1.50-2.00	1.25-2.00	0.75-1.00
Madison, Wisc.	2.00	2.00	1.50	2.00	2.00	0.50-0.75

SOURCE: *DeBow's Review*, 29 (1860): 381.

into the pineries during the logging season between mid-November and mid-March. The logging firms offered wages that were generally lower than those of competitive markets. Unskilled loggers earned \$10 to \$16 per month instead of \$14 to \$18 per month in wages during the farm season. Adding in the value of winter board raised the woodsman's wages to \$16 to \$22 versus \$20 to \$24 in the crop season. Here, however, exploitation was less than might be expected in a glutted market. Theoretically, if wages had been pushed down to subsistence, roughly equivalent to the \$6 cost of board, exploitation would have amounted to 70 percent; in practice, the rough figures suggest a range from 0 to 25 percent. As the crop season approached, the lumberjacks went back to the farms, but some undoubtedly remained to work as unskilled hands in the saw mills. For those who stayed, wages shot up abruptly to between \$20 and \$26.50. These rates created wages that were virtually identical to those for farm workers at the same time. In sum, winter wages in the Michigan pineries were less than competitive market wages, yet they were also considerably above the subsistence wages that seem to have been offered in Cincinnati in the late 1840s and early 1850s.³²

The Michigan pineries also reveal another general feature of labor costs—namely, the effect on the transfer wage of lengthening the work season. Farm workers who supplemented their work in the crop season with work in pineries, iron furnace collieries, or packing houses raised their rural earnings and thereby pushed the transfer wage upward. In Michigan, the laborer earned \$64 on the farm (\$16 per month for four months) and \$52 in the lumber camps (\$13 per month for four months), while board was covered for all but four months. With these figures in the formula, the transfer wage (W_u) equals \$257: $W_u - \$116 = \$165 - \$24$. Thus the theoretical transfer wage was considerably higher in Michigan than in Illinois. This difference was, in fact, reflected in actual urban wages. The prevailing wage for unskilled urban labor in Detroit, according to *DeBow's Review*, was \$0.875 per day, or \$272 per year in 1860. In Illinois, however, where off-season work opportunities were decidedly limited, daily wages were \$0.60 to \$0.80 per day, or \$186 to \$249 annually.³³ Too much ought not to be made of one case, but at least the evidence is consistent with the theory that implies an increase in the transfer wage as rural earnings are stretched longer over the year. Thus, Illinois seems to have been the lowest labor-cost state in the Midwest, a region where labor was generally inexpensive. (Compare the wages in Chicago with those in other Midwestern cities in Table 1.)

Conversely, the high costs of Southern urban labor reflected the region's higher transfer wage, and observers of the South often commented on "the great deficiency of tradesmen and mechanics" as well as unskilled white workers. The problem of the railroads symbolized the exorbitant cost of Southern free wage

³² For this industry we have drawn extensively from the excellent study by Barbara E. Benson; see her "Logs and Lumber: The Development of the Lumber Industry in Michigan's Lower Peninsula, 1837-1870" (Ph.D. dissertation, Indiana University, 1976).

³³ "Mechanics and Laborers' Wages North and South," *DeBow's Review*, 29 (1860): 381. Also see Table 1, opposite.

labor. In Louisiana, the state's railroad construction projects paid unskilled workers \$26 to \$30 per month (\$312 to \$360 per year), carpenters \$50 to \$60, and supervisors \$100 or higher. Nonetheless, a dearth of labor frustrated that state's efforts and all of the South's railroad building programs. Despite the comparatively high wages offered, they failed to attract the region's native white labor. In some cases these shortages forced builders to forfeit their contracts, while in most instances the firms involved resorted to the expedient of importing large numbers of white workers from the North or from Europe.³⁴

Other industries also illustrate the South's high transfer wage. Steamboat crewmen along Southern rivers, for example, annually earned an average of \$360 in 1850 and perhaps \$430 in 1860. At the same time, white workers on Louisiana's canals earned \$35 a month including board or \$420 per year. By contrast, Midwestern boatmen on the Illinois and Michigan Canal earned from \$112 to \$144 annually in the mid-1850s.³⁵ Moreover, all of Mississippi's fourteen cotton mills were constructed with cheap, imported Northern labor, and the only really profitable cotton factory was operated at the state's penitentiary.³⁶

Travelers justifiably lamented the high costs of labor in Southern cities—a point clearly revealed in a comparative table of wages in Northern and Southern cities published in *DeBow's Review* in 1860 (see Table 1). With scarcely an exception, Southern wages for unskilled and skilled workers were higher than those of Northern cities. In a cotton port such as Galveston, unskilled laborers earned \$1.25 to \$1.50 per day while the reported wages for Chicago were \$0.50 to \$1.00 with most receiving \$0.80 or less, according to the evidence presented above. Expressed as annual income, the laborers of Galveston earned \$388 to \$455, and those of Chicago, \$155 to \$249. Galveston and Chicago represented the extremes of regional differences in urban wages because cotton and grains decisively influenced the level of the transfer wage in Texas and Illinois. Clearly labor was expensive in Southern cities and cheap in Northern cities.

The composition of the Southern free labor pool also underlines the advantageous position of those Southern whites who chose to remain in planting rather than to migrate to the towns and cities. The skilled urban trades, whether in coastal ports or interior towns, were largely dominated by persons of Northern or foreign birth who moved in large numbers into the South's urban centers during the 1850s.³⁷ Similarly, the pool of unskilled free labor largely comprised foreign-born persons, free Negroes, and Northern laborers looking for work in

³⁴ Olmsted, *A Journey in the Seaboard Slave States*, 566; Merl E. Reed, *New Orleans and the Railroads: The Struggle for Commercial Empire, 1830-1860* (Baton Rouge, 1966), 18, 48; and U. B. Phillips, *A History of Transportation in the American Cotton Belt to 1860* (New York, 1908), 126, 160, 245-51.

³⁵ G. W. Morse, "Railroad and Water Communication," *DeBow's Review*, 19 (1855): 193-201; Eric F. Haites et al., *Western River Transportation: The Era of Early Internal Development, 1810-1860* (Baltimore, 1975), 170-77; and Lebergott, *Manpower in Economic Growth*, 325. Wages for steamboat operators are adjusted from 1850 to 1860 levels by following Stanley Lebergott, "Wage Trends, 1800-1900," in William N. Parker, ed., *Trends in the American Economy in the Nineteenth Century*, National Bureau of Economic Research, Studies in Income and Wealth, no. 24 (Princeton, 1960), 449-99, esp. 465.

³⁶ Walter Carey Hearn, "Towns in Antebellum Mississippi" (Ph.D. dissertation, University of Mississippi, 1969), 113-30. The backwardness of Southern manufactures is generally accepted, even in recent revisionist work stressing similarities between Southern and Western manufacturing; see Fred Bateman and Thomas Weiss, "Comparative Regional Development in Antebellum Manufacturing," *JEH*, 35 (1975): 182-208.

³⁷ Ira Berlin and Herbert G. Gutman, "Slaves, Freeworkers, and the Social Order of the Urban South," unpublished paper, Davis Center, Princeton University, 1976.

the winter. Southern cities generally resembled Olmsted's unflattering and characteristically bigoted remarks about Richmond, which housed "a considerable population of foreign origin, generally of the least valuable class; very dirty German Jews, especially, abound, and their characteristic shops . . . are thickly set in the narrowest and meanest streets, which seem to be otherwise inhabited mainly by negroes."³⁸

For would-be Southern entrepreneurs the region's high transfer wage established barriers that were virtually insurmountable. Rural opportunities and year-round employment had so elevated the transfer wage and the bidding floor for unskilled Southern white labor that employers, unwilling to pay out a minimum of \$400 for white workers, increasingly experimented with lower-cost slaves. Yet shifting slaves from the plantation to the factories, canals, and railroads also added costs. A rural slave generally cost his owner \$50 to \$60 per year, while the expense of maintaining an urban slave may have tripled or quadrupled total costs. Slaves were excluded, moreover, from those jobs requiring even minimal reading and reckoning abilities as well as from much factory work, because proslavery ideologues had poisoned entrepreneurial faith in slave competence.³⁹

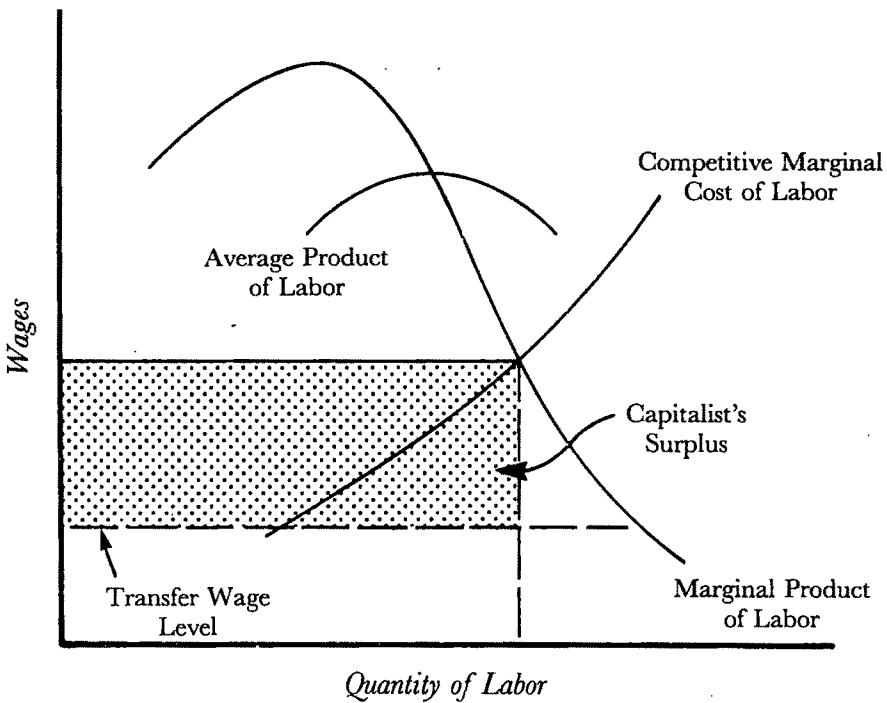
SHARP REGIONAL VARIATIONS IN THE TRANSFER WAGE proved decisive for the course of economic development. By creating a cheap-labor pool hired during the four-month crop season and an imperfect off-season labor market, grain farming established a favorable setting for merchants and manufacturers. Farmers did not have such latitude in raising wages: they were limited by commodity prices, and their demands were confined to a specific crop season, while their competitors hired for a twelve-month period. This condition, which applied with special force to the wheat belt, sometimes created a context at variance with the normal operation of the law of supply and demand; for labor began to grow scarce at the same time that its price, instead of rising, remained low. In the Old Northwest the emerging labor scarcity of the 1850s was countered by mechanizing the small grain harvest, a process that helped hold down and ultimately stabilize rural wages. Urban employers continued to be the chief beneficiaries of low farm wages. These employers at once offered laborers better wages than they could earn on the farm and gained a labor force considerably below labor's average product.

Consider the situation of Midwestern firms. During the off-season they had access to unlimited supplies of cheap labor—that is, the supply curve was in-

³⁸ Olmsted, *A Journey to the Seaboard Slave States*, 51.

³⁹ For a discussion of attempts to adapt slaves to nonagricultural tasks, see Robert S. Starobin, *Industrial Slavery in the Old South* (New York, 1970). Starobin has argued that industrial slaves in small towns were provisioned at about the same levels as plantation slaves, and this implies that their maintenance costs would have been as low as \$20 to \$30 per year. Richard Wade, however, noted that slaves in larger cities had high maintenance costs. *Ibid.*, 146–89; and Wade, *Slavery in the Cities: The South, 1820–1860* (New York, 1972), 134, chaps. 3, 5. Our estimates of slave costs are on the conservative side. Robert Evans, Jr., has shown a Lower South hire rate for 1856–60 of \$197, which with urban labor costs added on amounts to \$362 as the cost of slaves; see his "The Economics of American Negro Slavery," in Universities National Committee Bureau for Economic Research, *Aspects of Labor Economics* (Princeton, 1962), 185–243, esp. 216.

GRAPH 1
Low-Wage Case

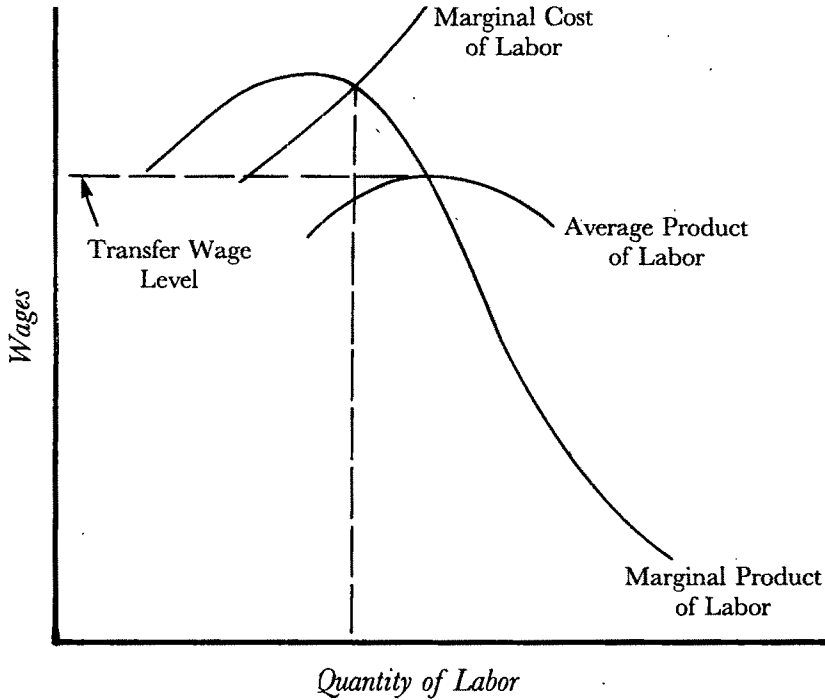


finitely elastic and wages were set at a low level by the transfer wage. These firms exercised a marked influence on wage determination because, given the ability of oligopsonistic buyers of labor to pay wages less than marginal product, the market had become less perfect on the demand side. Labor could thus be hired up to the point where labor's marginal costs equaled its marginal product; however, the annual wages paid to labor, or the transfer wage, fell below this intersection. Such a situation is illustrated in Graph 1, and the shaded area shows the extent of the producer's surplus and exploitation. Sir Arthur Lewis has argued that such a model is applicable to much of the underdeveloped world, but is applying a labor surplus model to the antebellum Midwest justifiable? Yes, provided that the hypothesis positing seasonality as a determinant of both market imperfections and of low transfer wages—or, what is the same thing, of cheap labor—is reliable. The producer's surplus then becomes an important source of internal finance for economic development.⁴⁰

The South was quite different. Urban wages were fixed at higher levels, and the producer's surplus from labor dwindled and perhaps even disappeared. In the case illustrated in Graph 2, wages equal average labor product, and the firm operates inefficiently because marginal product exceeds average product; firms in such situations are reluctant to hire unskilled white labor. One way around this problem—one widely used in the South—was to employ white skilled labor

⁴⁰ Lewis, "Reflections on Unlimited Labour," 75-96; and the excellent summary of Lewis's work in Meier, *Leading Issues in Economic Development*, 157-63.

GRAPH 2
High-Wage Case



in clerical jobs, which elevated the curve of labor's marginal product, thereby placing wages below average product. Under these conditions, industrialization in the South was rare. The halting steps toward manufacturing in the 1850s were aimed not at inducing white labor to the factories but at adapting slave labor to the factory regime.⁴¹

Graphs 1 and 2 indicate the labor situation faced by Midwestern and Southern firms. Midwestern employers had a strong inducement to use cheap labor, thereby earning a producer's surplus. Conversely, Southern firms needed to avoid unskilled labor, except for slaves, and tried instead to raise the marginal productivity of white labor in mercantile and related activities.⁴² The Southern case is nothing more than Habakkuk's argument for the scarcity of American labor. Were Habakkuk correct, the South should have seen the widespread diffusion of machines and industrialization. In fact, that did not happen. The experience of the United States in the antebellum era thus provides very little verification for the thesis of labor scarcity and high wages as the cause of technological change and urban-industrial development. First, the Habakkuk model implies that in the pre-industrial period firms were operating inefficiently

⁴¹ Southern cities were pre-eminently places of commerce, whereas in Northern cities warehousing-storage, transport, and manufacturing rivaled the commercial sector. See Carville V. Earle and Ronald Hoffman, "Regional Staples and Urban Systems in Antebellum America," paper presented at the Regional Economic History Research Center, Eleutherian Mills-Hagley Foundation, September 1978.

⁴² There was, however, considerable debate about the social effects of industrializing slaves; see Starobin, *Industrial Slavery in the Old South*, 190-232.

TABLE 2
Wages, Value Added, and Wage Shares in Manufacturing, 1860

<i>Region & State</i>	<i>Wages in \$</i>	<i>Value Added^a in \$</i>	<i>Wage Share^b %</i>
MIDDLE AMERICAN GRAIN BELT	204,982,000	459,075,000	44.7
Delaware	1,704,000	3,428,000	49.7
Illinois	7,638,000	19,818,000	38.5
Indiana	6,318,000	14,185,000	44.5
Iowa	1,922,000	4,779,000	40.2
Kansas	880,000	2,826,000	31.2
Kentucky	6,021,000	14,015,000	43.0
Michigan	1,080,000	2,424,000	44.6
Minnesota	712,000	1,278,000	55.7
Missouri	6,670,000	15,444,000	43.2
New Jersey	16,277,000	31,363,000	51.9
New York	65,447,000	150,226,000	43.6
Ohio	22,303,000	47,307,000	47.1
Pennsylvania	60,369,000	135,119,000	44.7
Tennessee	3,371,000	7,417,000	45.4
Wisconsin	4,269,000	9,446,000	45.2
SOUTH ATLANTIC AND GULF COAST	32,838,000	66,885,000	49.1
Alabama	2,133,000	4,371,000	48.8
Florida	620,000	1,424,000	43.5
Georgia	2,925,000	6,068,000	48.2
Louisiana	3,955,000	8,277,000	47.8
Maryland	7,191,000	14,383,000	50.0
Mississippi	1,618,000	3,093,000	52.3
North Carolina	2,689,000	5,700,000	47.2
South Carolina	1,380,000	2,966,000	46.5
Texas	1,163,000	2,948,000	39.4
Virginia	8,544,000	17,657,000	48.4
NEW ENGLAND	104,169,000	202,478,000	51.5
Connecticut	19,026,000	37,368,000	50.9
Maine	8,304,000	14,877,000	55.8
Massachusetts	56,963,000	109,869,000	51.9
New Hampshire	8,111,000	15,185,000	53.4
Rhode Island	8,760,000	18,911,000	46.3
Vermont	3,005,000	6,269,000	47.9

^a Value Added is net, derived by deducting raw material costs and capital depreciation, uniformly estimated at 8 percent of capital's value, from the value of output.

^b Wage Share equals Wages divided by Value Added multiplied by 100.

SOURCE: U.S. Census Bureau, *Manufactures of the United States in 1860* (Washington, 1865).

with high-cost labor, but, assuming that entrepreneurs were rational, then they would have avoided such a position. Second, the source of investment capital is problematic in Habakkuk's model given the presence of high interest rates and the absence of a producer's surplus. Third, his theory of technological change fails to explain the impressive shifts into service-sector employment that had, at least since the mid-eighteenth century, proceeded apace, unaided by new technology and increased productivity.

A much stronger case may be made for inexpensive labor subsidizing American economic development—a case that explains both the shifts to capital-intensive manufacturing and to labor-intensive service industries. Cheap labor meant that Midwestern firms hired labor at half the cost of Southern firms. Low Midwestern wages contributed to a producer's surplus, which in turn provided internal capital for firm expansion. As the scale of operation expanded and the labor market remained imperfect, labor's marginal-product curve effectively rose while labor costs remained relatively elastic. These changes augmented the producer's surplus, and that process continued until labor markets became perfect. Thus, labor's share of value added was low in the early stage of market development and improved with the removal of market imperfections.⁴³

Wage shares in American manufacturing support this hypothesis. In New England, for example, textile mill wage shares deteriorated from 1820 to 1840. Wages constituted about 48 percent of value added in the 1820s but only 40 percent between 1830 and 1860.⁴⁴ The manufacturing census of 1860 shows that wage shares of value added were lowest in the grain belt (44.7 percent) from the Mid-Atlantic states through the Midwest. The chief exception was in Minnesota, where the wage shares reflected an immature, frontier economy. Labor's share was higher, as expected, in the Deep South (49.1 percent), particularly in the Gulf and Atlantic Coast states, despite the use of cheap slave labor in factory production. The highest wage share occurred in New England (51.5 percent). This pattern (see Table 2) is not particularly surprising. Labor's share had doubtlessly fallen earlier in the century, a point suggested by the textile industry, and, by 1860, labor supply was both less elastic and better organized. Accordingly, wage shares in New England increased at the expense of the producer's surplus. Rising wage shares are also suggested by Edward C. Budd's examination of factor shares after 1850.⁴⁵

Such changes in wage shares seem to be intimately connected with economic development in its early stages. Lewis has suggested that wage shares traditionally follow a uniform pattern as economic development takes place, with labor's

⁴³ This argument closely resembles Sir Arthur Lewis's model of economic development with unlimited supplies of cheap labor. We remain open, however, on the issue of whether wages rose because of perfect competition or as a result of labor's struggle for an increased share of the product. American rural labor became less elastic as it found off-season employment, thereby raising rural earnings and, hence, the transfer wage. Rural income on the Dakota frontier, for example, rose in the late nineteenth century, and higher labor costs probably hindered the region's urban growth; John C. Hudson, "Migration to an American Frontier," *Annals of the Association of American Geographers*, 56 (1976): 242–65.

⁴⁴ Robert Zevin, "The Growth of Cotton Textile Production after 1815," in Fogel and Engerman, *The Reinterpretation of American Economic History*, 122–47.

⁴⁵ U.S. Census Bureau, *Manufactures of the United States in 1860, Compiled from the Original Returns of the Eighth Census* (Washington, 1865); and Budd, "Factor Shares, 1850–1910," in Parker, *Trends in the American Economy*, 365–98.

share starting small and then growing larger as the process accelerates. Is it coincidence, for example, that the wage share of Illinois in 1860 resembles that of the developing nation of Venezuela in the 1950s?⁴⁶ Lewis's model would suggest not. Rather, his argument posits an evolutionary process wherein labor subsidizes capital investment for a time but then improves its position as labor supply becomes less elastic and, we would add, as seasonal market imperfections are removed. Wage shares as indicators of development, however, must be used cautiously, and, in this regard, middling wage shares are particularly difficult to interpret. They may indicate two different courses of economic development: (1) they may reveal, as Lewis suggests, development through the progressive erosion of the producer's surplus as the labor supply becomes less elastic; or (2) they may indicate a situation of economic stagnation caused by initially high wages and earnings for labor that allow a very negligible producer's surplus and, hence, very little "plowback capital" for development. The second situation arose in the American South, where the long crop season created high rural wages, and has also emerged in underdeveloped countries, where minimum wage legislation or related policies have artificially raised the transfer wage. In both instances, whether in the antebellum South or in contemporary underdeveloped countries, the effect on economic development has been an appreciable retardation of the industrialization process.⁴⁷

In sum, reasonably low-cost labor, as existed in the American Midwest, was indispensable for that region's economic advance, although the position of labor within that process was an unenviable one. In the grain belts of the United States, the exploitation of labor persisted until economic growth established a more competitive market.

LOW-COST LABOR WAS USED in the Midwest in the 1850s as it had been earlier in New England and the Mid-Atlantic states. Equally important, cheap Northern labor provided the stimulus not only to industrial growth but also to the process whereby skilled labor was replaced by a combination of machine technology and unskilled labor. The Philadelphia region in the early nineteenth century illustrates this development. This area combined mixed grain farming, impressive urban growth, and an early start in manufacturing and the factory system.⁴⁸ Here, inexpensive labor was the decisive inducement for the mechanization of cotton textile production.

Between 1800 and 1830 Philadelphia's merchants and manufacturers had solid reasons for using low-cost agricultural labor in urban jobs. By the turn of the century, the city's extensive hinterland had matured into perhaps the pre-eminent mixed-farming region in the nation. High yields of wheat and corn

⁴⁶ Lewis, "Reflections on Unlimited Labour," 89-90.

⁴⁷ Bateman and Weiss, "Comparative Regional Development in Antebellum Manufacturing," 182-208. On underdeveloped countries, see various essays in Meier, *Leading Issues in Economic Development*.

⁴⁸ For two especially relevant studies, see James T. Lemon, *The Best Poor Man's Country: A Geographical Study of Early Southeastern Pennsylvania* (Baltimore, 1972); and Diane Lindstrom, *Economic Development in the Philadelphia Region, 1810-1850* (New York, 1978).

from southeastern Pennsylvania as well as the upper half of the Delmarva Peninsula were channeled into Philadelphia as they had been for much of the eighteenth century. Rural labor was abundant as early as the 1750s. At the end of that decade, laborers made up 15 percent of the taxpayers in Lancaster County, Pennsylvania, and small tenants and sharecroppers added 10 to 15 percent to the available harvest force. These proportions remained the same for at least the next two or three decades. In Chester County through the start of the nineteenth century, laborers (noted in the tax lists as inmates) ranged from 15 to 23 percent of taxable persons, and another 10 percent were tenants. The striking similarity between Pennsylvania and the Midwest in the 1850s extends also to the seasonal work schedule. These farm workers toiled mainly in the harvest. Although some laborers may have worked seven or eight months of the year on diversified farms close to Philadelphia, the overwhelming majority worked mainly between April and July. This was a sore point for the property owners of the town of Lancaster, who complained as early as 1767 that "quitrents could never be collected from the poor of the town, who were idle for 'want of employment,' except during harvest time." It was this mass of unskilled rural labor that permitted a York County farmer to mobilize over one hundred harvesters in 1828.⁴⁹

In the Delmarva portion of the hinterland, the labor pattern was similar. Farm account books there clearly reveal the preference of grain farmers for hiring day laborers frequently, one- or two-month laborers occasionally, and longer-term laborers rarely, if at all. On the mixed-grain, Dixon farm between 1821 and 1824, day laborers were preferred. They constituted seven of the eight laborers hired in 1821, eight of the twelve in 1822, fifteen of the eighteen in 1823, and eight of the ten in 1824. Of the ten workers hired on other terms, six were hired for two months or less, two for eight months, and two for a year. Hired help was required for grain harvesting and haying in late June and July and, except for a hand or two, was unnecessary for the rest of the year. Even the one- and two-month hands were taken on for the June and July work peak.⁵⁰ In short the grain economy in the rural zone around Philadelphia had created by the 1780s, and perhaps earlier, a permanent pool of rural day laborers. Their work was confined mainly to the harvest season (to an even greater degree than Midwestern laborers in the 1850s), and their annual earnings were, of necessity, very small.

Cheap labor became even more attractive for Philadelphians between 1815

⁴⁹ Lemon, *The Best Poor Man's Country*, 12, 96, 234; James T. Lemon and Gary Nash, "The Distribution of Wealth in Eighteenth-Century America: A Century of Change in Chester County, Pennsylvania, 1693-1802," *Journal of Social History*, 2 (1968-69): 1-24; and Stevenson Whitcomb Fletcher, *Pennsylvania Agriculture and Country Life, 1640-1840* (Harrisburg, Pa., 1950), 119-20.

⁵⁰ Julia Ann Dixon, Farmer, *Private Accounts*, Delaware State Archives, Dover, Del. For other useful private accounts, see those of Isaac Gurwell, 1791-1808; Joseph Evans, 1785-1805; and Bennett Downes, 1793-1814, Delaware State Archives, Dover, Del. These combinations of seasonal and annual workers compare favorably with the bonanza farms of the Great Plains after the Civil War. On one of the largest of these farms, which employed up to two hundred and fifty laborers, only 8 percent of the laborers worked for ten to twelve months, while 92 percent worked five months or less. These figures are calculated from data in Fred A. Shannon, *The Farmer's Last Frontier: Agriculture, 1860-1897* (New York, 1945), 159.

and 1830. The region's export staples of wheat, corn, and derivative commodities stagnated following the Napoleonic Wars and the recession of 1819. Rural farm wages and earnings followed the downward course. In 1818, 1826, and 1830—critical years for technological innovation in Philadelphia—farm wages fell from \$11.00 to between \$9.00 and \$9.50 per month. Paralleling the wages of farm hands, the wages of unskilled workers on eastern Pennsylvania iron plantations fell from \$15 to between \$12 and \$13.⁵¹ This synchronous movement of wages suggests that both farm laborers and unskilled workers supplied the same market and thus experienced the same economic pattern of recession and recovery.

The phenomenon of urban-industrial growth during recessions, though paradoxical, is precisely what happened in Philadelphia during the 1820s. Albert Fishlow has noted a similar connection between depression, increased labor inputs, and import substitution in the economic growth of Brazil during the 1930s, and a comparable situation occurred more than a century earlier in the Philadelphia region.⁵² The fall of wages between 1818 and 1826 expanded the demand for unskilled labor at the expense of farm labor. Changes in agriculture and urbanization are certainly compatible with this labor shift. Farm wages fell because of a marked slowdown in the trade of farm products, as evidenced in Philadelphia's grain receipts. Exports tailed off during the 1820s, as did grain, flour, and meal inspections. Wheat, flour, and corn inspections grew at negative or modest annual rates ranging from -0.1 percent to 1.48 percent. As the farm commodity trade declined, so did farm earnings and the level of the transfer wage. Laborers moved to the towns and cities in the region, as Diane Lindstrom has clearly shown. Philadelphia, for example, grew by nearly 38 percent between 1820 and 1830, a decade in which there was little foreign immigration; this growth rate was about double that of its hinterland.⁵³ Laborers were induced to go to the city even though wages were falling rapidly because the compensating feature of city life was the declining cost of living. Wholesale prices had fallen by as much as 80 percent from peaks achieved during the War of 1812, and by the 1820s, prices had fallen to 1790 levels. The recession after the War of 1812 thus meant lower labor costs for urban entrepreneurs and declining living costs for new migrants to the city.⁵⁴

In brief, the recession merely mobilized an already abundant and inexpensive labor force. As farmers retrenched with the slowdown of trade, rural laborers were attracted to cities and towns whence they had often drifted during the off-season. Only this time, many laborers remained permanently, because the city offered steadier employment at slightly higher wages accompanied by declining living costs. Urban work was created by the entrepreneurs of Philadelphia and

⁵¹ Lebergott, *Manpower in Economic Growth*, 539; and Zabler, "Further Evidence on American Wage Differentials," 109-17.

⁵² Fishlow, "Origins and Consequences of Import Substitution in Brazil," in DiMarco, *International Economics and Development*, 311-65.

⁵³ Lindstrom, *Economic Development in the Philadelphia Region*, 170-74. On the importance of rural to urban migration for a later date, see John Modell, "The Peopling of a Working-Class Ward: Reading, Pennsylvania, 1850," *Journal of Social History*, 5 (1971): 71-95.

⁵⁴ Adams, "Wage Rates in the Early National Period," 424.

its region who accurately understood how they might use the cheap, unskilled labor force available to them. Some firms, such as the iron furnaces with their enormous demand for wood cutters, provided winter work for these laborers. More ingeniously, other firms borrowed British technology and, by using the machine, tapped the pool of cheap labor and endowed it with extraordinary skills.⁵⁵

The motive behind capital investment in machines was a simple one—to convert cheap unskilled labor into cheap skilled labor. Habakkuk's argument aside, there is little evidence that entrepreneurs envisioned machines as increasing the productivity of unskilled labor. Quite the opposite, they regarded machines, along with division of labor, as allowing them to maintain roughly the same output while drastically cutting labor costs. In this spirit Eli Whitney claimed that interchangeable parts would "substitute correct and effective operations of machinery for that skill of the artist which is acquired only by long practice and experience; a species of skill which is not possessed in this country to any considerable extent." Whitney did not emphasize the machine's ability to increase physical product but rather how the shortage of skilled labor could be overcome by the combination of machines and unskilled labor. Some years later, James Montgomery succinctly stated entrepreneurial motives from the cost side. "The great object with all manufacturers in this country," he claimed, "is to pay their help just such wages as will be a sufficient inducement for them to remain at the work. Hence the greater the quantity of work produced, the higher the profits, because paid at a lower rate of wages."⁵⁶ Philadelphians understood the language of Whitney and Montgomery, and they put it to practice. By using machines and low-priced labor, they could produce the same output, at a slightly lower cost, as by using skilled high-priced workers. In so doing, they undermined the status of skilled laborers, pushing them down toward the level of the unskilled workers.

One of the great advances in the American industrial revolution was the application of machines in the cotton textile industry. Philadelphia and New England were in the front ranks in adopting this technology during the decade and a half after the War of 1812. In both regions, machine-made cloth of coarse but sturdy quality rapidly displaced the domestic plaids, ginghams, and checks produced by the highly skilled mule spinners and hand-loom weavers. Local yarn mills, supplying local weavers and household looms, gave way to integrated mills combining the new technology of throstle spindles, filling frames, roving machines, and power looms.⁵⁷ David J. Jeremy has detailed a number of basic

⁵⁵ For the story of efforts to secure British textile technology, see David J. Jeremy, "British Textile Technology Transmission to the United States: The Philadelphia Region Experience, 1770–1820," *Business History Review*, 47 (1973): 24–52.

⁵⁶ William P. Blake, "Sketch of the Life of Eli Whitney, the Inventor of the Cotton Gin," *Papers of the New Haven Colony Historical Society*, 5 (New Haven, 1894), 122, as quoted in Habakkuk, *American and British Technology*; 22; and Montgomery, *Practical Detail of the Cotton Manufactures of the United States of America and the State of the Cotton Manufactures of That Country Contrasted with That of Great Britain* (Glasgow, 1840), 97–98.

⁵⁷ Zevin, "The Growth of Cotton Textile Production," 122–47; Caroline F. Ware, *The Early New England Cotton Manufacture: A Case Study in Industrial Beginnings* (1931; reprint ed., New York, 1966), 3–118; William A. Sullivan, *The Industrial Worker in Pennsylvania, 1800–1840* (Harrisburg, Pa., 1935), 17–27; and Jeremy, "British Textile Technology Transmission," 24–52.

organizational and technological innovations that expanded American textile production from 1814 to 1840.⁵⁸ Larger machines were introduced that increased the ratio of spindles to operators from 64 to 166. Similarly, the speeds at which the machines operated were accelerated by over 100 percent, and virtually all stages of production were mechanized, including, by 1840, the application of automatic fault detection systems. In another important decision, the manufacturers narrowed the range of goods they produced for the middle- and lower-quality mass markets. This step simplified operating procedures, shortened the learning period for the untrained worker, and allowed the use of less complex machinery. Finally, as happened at Waltham, manufacturers initiated the process of vertical integration to concentrate "unskilled teenage females as the nucleus of a labor force," whose physical weaknesses and lack of skill were compensated for by "maximum mechanical work." The same substitution occurred in the Philadelphia region; but there unskilled men and boys were the cheap input, and they were employed disproportionately.⁵⁹ James Montgomery, in calculating the results of these changes, estimated in 1840 that a British mill of 128 looms produced 35,000 yards of cloth in two weeks, while its American equivalent made some 51,300 yards in the same period.⁶⁰

THE DECISIVE VICTORY OF THE MACHINE in American textile manufacturing stands in marked contrast to the sluggish diffusion of that technology in Britain. The difference may be explained by contrasting labor supplies. In comparing the economics of using the hand-loom weaver or the power-loom operator in Philadelphia and Britain, two points become apparent. First, the transfer wage was lower in Philadelphia than in Britain, so American costs for unskilled labor and power looms were lower than the costs of hand-loom weavers, whereas British hand-loom weavers were generally cheaper than the new technology. Second, the output of hand and power looms were very much the same, so productivity did not figure as a motive for machine investment.

In the Philadelphia region, cheap labor and textile machinery proved less costly than skilled textile operators, and, accordingly, manufacturers pursuing the "great object" of low wages adopted the new textile technology. Two principal inducements motivated this transition. First, unskilled urban labor was in-

⁵⁸ Jeremy, "Innovation in American Textile Technology during the Early Nineteenth Century," *Technology and Culture*, 14 (1973): 40-76.

⁵⁹ Women were considerably more numerous in the textile industry in New England (New Hampshire, Rhode Island, and Massachusetts) than in the mills located in the grain belt of the Middle Atlantic states (New Jersey, Delaware, Maryland, and the Eastern District of Pennsylvania). The 1822 census of manufactures discloses that women constituted 33.3 percent of all employees in New England mills but only 16 percent in those of the Mid-Atlantic states. Excluding children, who constituted about half of the employees in both regions, women comprised 63.5 percent of the work force in New England, while in the Mid-Atlantic region men made up 65.5 percent of the employees. See U.S. State Department, *Digest of Accounts of Manufacturing Establishments in the United States, and of Their Manufactures* (Washington, 1823). For further detail on New England's industrialization and the role of labor, see Alexander James Field, "Sectoral Shift in Antebellum Massachusetts: A Reconsideration," *EEH*, 15 (1978): 146-71.

⁶⁰ Jeremy, "Innovation in American Textile Technology," 40-76.

expensive because of the low transfer wage occasioned by the sizable mass of farm labor, the duress of the Mid-Atlantic agricultural economy after the War of 1812, and the concomitant decline of rural wages. The second inducement favoring the new technology was the cost of skilled labor. Skilled workers continued to command high wages despite the economic slowdown after 1815, and, as a consequence, the differential between their wages and those of the unskilled widened and reached a maximum in 1822.⁶¹

The low transfer wage in the hinterland of Philadelphia reflected the acute seasonality of the region's rural economy. Rural wages amounted to about \$10 per month while earnings (using an employment estimate generously set at four months) amounted to \$40 per year. Rural subsistence costs in the off-season consumed about \$26 to \$27 of labor's earnings.⁶² Rural life offered little promise, especially in areas wracked by depression, where little land was available and population pressure made land acquisition improbable. In this context, the city offered an attractive alternative. Urban living costs were about \$55 for a single adult male, which implies a transfer wage (W_u) of about \$68 to \$69. The compensation for rural nonpecuniary incomes, however, appears to have been quite large. The intangible income of rural life, judging from the \$110 per year earned by unskilled workers on the Pennsylvania Canal, was about double farm wages.⁶³ A high nonpecuniary income is not unreasonable for a low income, pre-industrial economy; however, its size may be slightly exaggerated by underestimation of off-season employment. Supposing that \$110 indicates the annual wages for which rural labor would have migrated to urban jobs, then we may compare this wage to those prevailing in the textile industry during the 1820s (Table 3).⁶⁴

Consider the firm choosing between the old and new technologies. Its costs for the old technology were the wages paid to skilled hand-loom weavers, whose

⁶¹ Zabler, "Further Evidence on American Wage Differentials," 109-17. That differential, however, remained substantial; see Table 1, page 1072, above.

⁶² Board is estimated at one-third the monthly wage. Lebergott, *Manpower in Economic Growth*, 257-72, esp. 262; and Nicholas Biddle, "Address by Nicholas Biddle, Esq., 15 January 1822," in *Sketch of the History of the Philadelphia Society for Promoting Agriculture* (Philadelphia, 1939), 49-50.

⁶³ M. Carey, *Appeal to the Wealthy of the Land, Ladies as well as Gentlemen, on the Character, Conduct, Situation, and Prospects of Those Whose Sole Dependence for Subsistence Is on the Labour of Their Hands* (3d ed., Philadelphia, 1833), 8-9. Living costs are based on estimates for a family of two adults and two children. We equated two children as one adult and then divided family living costs by three to get an individual laborer's expenses. Although unskilled urban wages were low, steady employment frequently compensated—a point made by James Ronaldson, an employer located in the manufacturing district of Rockdale in southeastern Pennsylvania, who observed in 1832, "I know that, in consequence of our men losing no time from bad weather, want of jobs, and at fifty cents per day, the old men and youths thrive better than on a farm or at laboring work and job work. Indeed, in this establishment, one family whose father is a mason, and another family whose father is a country carpenter, has been supported by them all last winter: the men were out of employment." Ronaldson to Mathew Carey and Clement C. Biddle, in U.S. Department of the Treasury, *Documents Relative to the Manufactures in the United States Collected and Transmitted to the House of Representatives in Compliance with a Resolution of January 19, 1832*, 3 (Washington, 1833): 212-14. For further details on Ronaldson's mill, see Anthony F. C. Wallace, *Rockdale: The Growth of an American Village in the Early Industrial Revolution* (New York, 1978).

⁶⁴ Our estimates suggest that unskilled men and women received comparable annual earnings in the textile industry, and this may account for the disproportionate employment of men in the Mid-Atlantic region. By contrast, in New England the wages of unskilled men seem to have been considerably higher than those of women, thus giving the latter a competitive advantage. For New England, see Thomas Dublin, *Women at Work: The Transformation of Work and Community in Lowell, Massachusetts, 1826-1860* (New York, 1979), 65; and Howard Gitelman, "The Waltham System and the Coming of the Irish," *Labor History*, 8 (1967): 227-53.

TABLE 3
Daily, Weekly, and Annual Wages of Labor in England and the United States,
Old and New Technologies (in Dollars)

<i>Technology & Labor</i>	<i>Daily</i>	<i>England Weekly</i>	<i>Yearly^a</i>	<i>Daily</i>	<i>United States Weekly</i>	<i>Yearly^a</i>
OLD TECHNOLOGY						
Cotton Mule Spinners in Cotton Mills	\$1.02	\$6.12	\$318.24	\$1.24 ^c	\$7.44	\$386.88
Weavers on Hand Looms	0.74	4.44	230.88	0.90	5.40	280.80
NEW TECHNOLOGY						
Boys 10-12 Years Old (Spindles)	\$0.22	\$1.30	\$ 67.60	\$0.25	\$1.50	\$ 78.00
Women in Cotton Mills (Power Looms)	0.33	1.96	101.92	0.42	2.50 ^d	130.00
FARM LABOR						
Board included, expenses deducted	—	—	\$170.00 ^b	—	—	(40 - 26.64) = \$ 13.36

^a Yearly Wage is based on 312 working days.

^b For the yearly farm wages in England, see text, page 1090, below.

^c \$1.24 represents the mean of the range \$1.08 - \$1.40 per day.

^d \$2.50 represents the mean of the range \$2.00 - \$3.00 per week.

SOURCES: Farm labor wages are as estimated in the text, page 1085, above, with board and expenses deducted for periods of unemployment. Otherwise, wages are adapted from Zachariah Allen, *The Science of Mechanics* (Providence, 1829), 347, reprinted in Nathan Rosenberg, "Anglo-American Wage Differences in the 1820's," *Journal of Economic History*, 27 (1967): 221-29, Table 1.

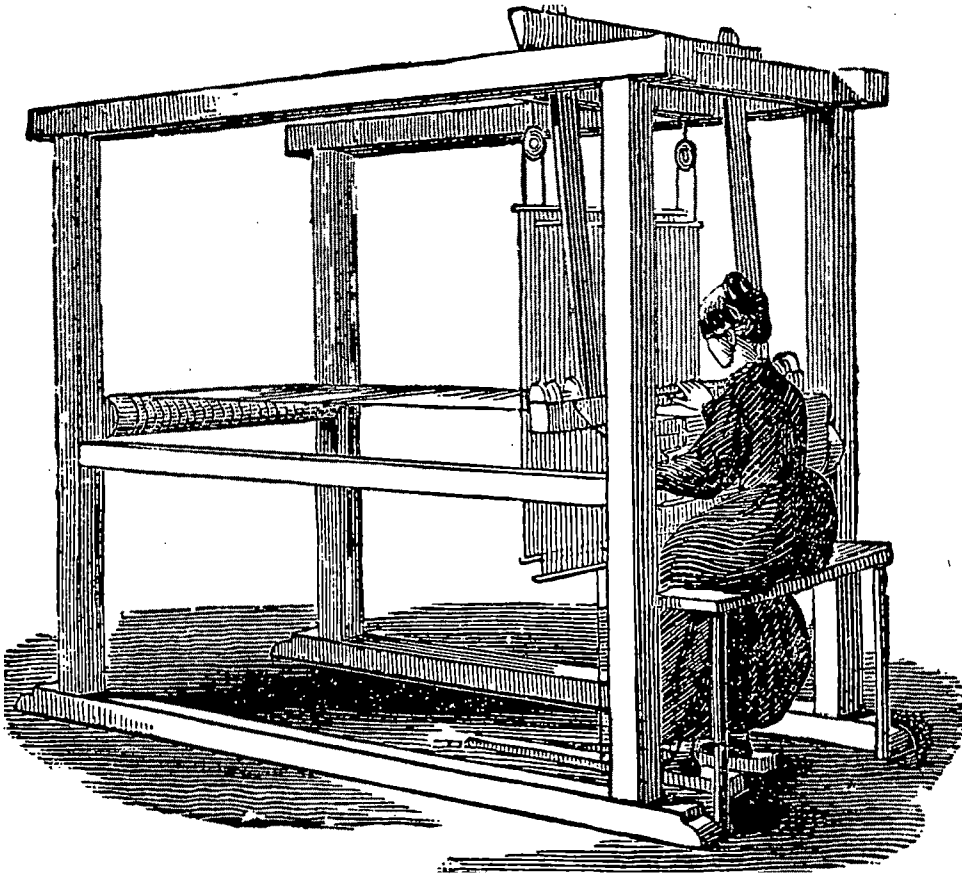


Figure 6: Mid-nineteenth-century representation of hand loom—the old technology of weaving in the cotton industry. Reproduced from Eminent Literary Men, *One Hundred Years' Progress of the United States*, 277.

average earnings were \$280 per year. If a firm wished to replace hand-loom weavers with power looms and unskilled workers and if physical outputs and cloth values were equal and cancelled productivity differences, unskilled labor would have been cheaper, provided machine costs fell under \$170.80, which they usually did. Power looms in the period from 1815 to 1825 varied in price, ranging from the expensive Waltham loom at \$125 to as little as \$70 for other models.⁶⁵ As most operators handled two looms, machine costs were doubled, and total costs for machines and unskilled labor amounted to \$250 to \$360. Loom costs, however, would have depreciated over their useful lives, and, hence, annual costs were considerably below these figures. Paul A. David has shown that looms installed in the 1830s had an average life of thirty-seven years, but this is surely too long for the first power looms installed in the 1810s and 1820s.⁶⁶ The risks of technological obsolescence and rapidly falling machine prices warrant an accelerated depreciation schedule. Five years seems a maximum, given

⁶⁵ Although the argument made here focuses on the introduction of the power loom, a similar case can be made for the rationality of adopting other machines in the new technology in textiles and in other industries as well. George Sweet Gibb, *The Saco-Lowell Shops: Textile Machinery Building in New England, 1813-1949* (Cambridge, Mass., 1950), 39-50, esp. 42.

⁶⁶ David, "The 'Horndahl Effect' in Lowell, 1834-56: A Short-Run Learning Curve for Integrated Cotton Textile Mills," in David, *Technical Choice Innovation and Economic Growth*, 177.

market and technological uncertainties, and such a short useful life has the effect of making the cost of the new technology more expensive.⁶⁷ Annual machine costs, then, amounted to \$28 to \$50, and, adding in labor (\$110), total costs came to between \$138 and \$160. When the hand-loom operator costs \$280 (the weaver bore the cost of his loom), textile manufacturers stood to save \$120 to \$142 per operator by turning to the power loom and low-priced, unskilled labor.

Habakkuk has argued that machines were adopted not to cut costs but to raise labor productivity. No evidence points to gains in physical productivity, however, until long after the introduction of machines, and these gains may be attributed to technical improvements and organizational efficiencies unanticipated by the innovators. New technologies frequently involve no clear gains in productivity. A case in point, for example, is Oliver Evans's grain mill, which increased output by perhaps as little as 10 percent; more importantly, his mill enabled the use of boys and unskilled men, both employable at low wages.

The physical output of the power-loom operators initially seems to have been about the same as the hand-loom weavers. Although the claim has been made that the power-loom operator of the 1830s turned out seven times the cloth produced by the skilled weaver, this estimate presents the power loom in the most favorable light.⁶⁸ Perhaps the most efficient mills of New England about 1830 had achieved this level of advantage. In these elite mills, an operator tending two power looms fed by seventy spindles produced 33,708 yards of cloth (of relatively low thread count) per year. By contrast, expert Philadelphia hand-loom weavers turned out 18 yards per day and, therefore, 5,598 yards during the conventional working year of 311 days.⁶⁹ This six-fold advantage of New England over Philadelphia was unusual in 1830 and grossly out of line for the period from 1815 to 1825, when the new technology was introduced. A more likely figure is based on American cloth output in 1831, and the assumption that there were fifteen spindles feeding each power loom. Each power-loom operator would have turned out only 5,561 yards of cloth—a figure almost identical to that of hand-loom weavers.⁷⁰ At earlier dates, the spindle-to-loom ratio was closer to the lower estimate of cloth output. In 1827, an integrated mill at Manayunk near Philadelphia was set up with a spindle-to-loom ratio of 21.4. Philadelphia mills in 1819 had a ratio of 8.4. In New England the first sale of textile machinery by the Boston Manufacturing Company to the Poignard Plant & Company had a ratio of 6.3; and Boston Manufacturing's sales and

⁶⁷ Rental rates of \$10 to \$15 for looms that sold at \$125 by the Boston Manufacturing Company imply a useful life of eight to twelve and a half years; thus, the estimate of five years certainly gives an upward bias to machine costs. Gibb, *The Saco-Lowell Shops*, 42–43.

⁶⁸ The seven-fold advantage reputedly occurred on British steam looms, with operators tending two looms each; Edward Baines, *History of the Cotton Manufacture in Great Britain* (London, 1835), 239–40.

⁶⁹ The New England estimates are calculated from spindle counts and cloth output figures in David, "Learning by Doing and Tariff Protection," 95–173, and from ranges of spindle-to-loom ratios suggested in Zevin, "The Growth of Cotton Textile Production," 141. For the suggestion of low productivity for power looms in Britain in 1819, see S. J. Chapman, *The Lancashire Cotton Industry* (Manchester, 1904), 31. On hand-loom production in Philadelphia, see Sullivan, *The Industrial Worker in Pennsylvania*, 39.

⁷⁰ David, "Learning by Doing and Tariff Protection," 95–173; and Zevin, "The Growth of Cotton Textile Production," 141.

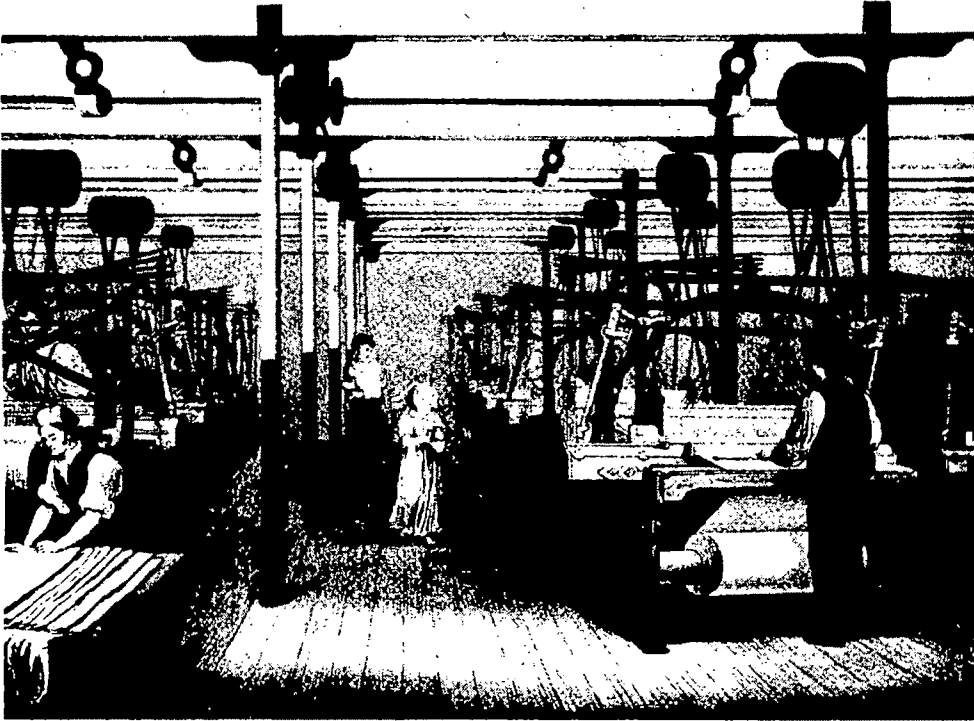


Figure 7: Power-loom weaving in the nineteenth-century textile factory. Lithograph by J. R. Barfoot, reproduced from Anthony F. C. Wallace, *Rockdale: The Growth of an American Village in the Early Industrial Revolution* (New York, 1978), 169, and reproduced courtesy of Alfred A. Knopf, Publishers, and Mabel Brady Garvan Collection, Yale University Art Gallery.

grants of rights to use their equipment between 1817 and 1823 indicate fourteen spindles for every loom. These low ratios of spindles to power looms suggest that the early mills were far less productive than the elite mills of New England in the 1830s and imply a cloth output of about 5,600 yards per power-loom operator. There was, therefore, no immediate appreciable gain in physical productivity by shifting to power looms.⁷¹

Although the outputs of hand and power looms were roughly the same, cloth woven on the hand loom commanded a higher price, and price differences may have narrowed the cost advantage of the new technology. Some idea of the price spread is apparent in wholesale prices of high quality checks and coarse sheeting in Philadelphia. In 1819, hand-loom checks brought a margin of \$0.0212 per yard over power-loom sheetings.⁷² This price spread meant that the hand-loom weaver earned the firm about \$118 more than the power-loom operator; these higher revenues, however, did not offset the low costs of the power loom and its labor. On the cost side, the power loom had an advantage of \$120 to \$142,

⁷¹ Sullivan, *The Industrial Worker in Pennsylvania*, 17–23; Gibb, *The Saco-Lowell Shops*, 40–47; and Jeremy, “British Textile Technology Transmission,” 24–52. For a recent report of similar outputs for English hand and power looms about 1810, see John Stephen Lyons, “The Lancashire Cotton Industry and the Introduction of the Power-Loom, 1815–1850,” *JEH*, 38 (1978): 283–84.

⁷² These price spreads (current prices) give a rough idea of the advantage of hand-loom weavers; but these prices generally represent imported goods, so conclusions on this matter must remain tentative. The Bezanson prices must serve until we have a price series for domestic cloths: Anne Bezanson *et al.*, *Wholesale Prices in Philadelphia, 1784–1861—Part II: Series of Relative Monthly Prices* (Philadelphia, 1937), 30, 196.

which exceeded the higher value of cloth produced by the hand-loom weaver from \$2 to \$24 per operator. The power loom's advantage was even greater when hand-loom weavers turned out less than the eighteen yards per day that was standard for expert weavers.

The case of the power loom underscores the characteristic feature of American capitalism—namely, the use of cheap, instead of expensive, inputs, provided any differences in productivity are overcome. As firms introduced the new technology, their productivity remained stable, but they gained cost advantages by substituting low-cost, unskilled labor for expensive, skilled labor. Machines endowed unskilled workers with the ability to make a simple, coarse fabric. Although worker productivity at a later date underwent vast expansion because of technical and organizational efficiencies, to assert that firms introduced machines for the purpose of elevating labor's physical productivity is to read history backwards. On the other side of the Atlantic, English textile manufacturers were relatively slow in adopting the new technology. Skilled laborers continued running the hand looms and the mules despite the availability of power looms, throstle frames, and related machinery. The explanation for the slow pace of British technological diffusion is merely the reverse of that used for the United States.

In England the old technology remained more profitable than the new. During the mid-1820s, English hand-loom weavers earned \$230, or about \$50 less than American weavers.⁷³ These annual wages could not induce a massive influx of English farm labor into the city. English rural labor, unlike its American counterpart, was expensive; estimates put a laborer's earnings at about \$170 annually, board included.⁷⁴ This high income, in turn, reflected nearly year-round employment. Next to American farming, English farming was much more diversified, integrating crops and livestock in greater variety, placing emphasis on labor intensive activities such as raising potatoes and turnips along with grains, and providing daily attention to livestock. Land improvements—notably draining, marling, manuring, and pasture making—also commanded the laborer's attention during the year. Annual labor contracts were not uncommon, and the seasonal unemployment endemic to American grain farming was generally ab-

⁷³ Rosenberg, "Anglo-American Wage Differences in the 1820s," 221–29.

⁷⁴ Carey, *Essay on the Rate of Wages*, 91; Adams, "Some Evidence on English and American Wage Rates," 505–06; and Hobsbawm and Rudé, *Captain Swing*, 23–55. As noted earlier, rural earnings varied markedly from place to place and among farm laborers' families. Our estimates are, however, consistent with the comprehensive survey of Norfolk and Suffolk laborers in the 1830s undertaken by James Phillips Kay, who demonstrated that the average yearly earnings for laborers in families amounted to \$173. The range varied from single laborers, who earned \$120 per year, to families with several children over ten years of age, whose family income was \$216. Kay's survey of 539 laborers also underlines employment opportunities during the lengthy farm year characteristic of English agriculture. Kay, "Earnings of Agricultural Labourers in Norfolk and Suffolk," *JSSL*, 1 (1838): 179–83. Equally instructive is the importance of marriage and the family. Among the 539 laborers surveyed by Kay, 93.3 percent were married and 81.4 percent had children; *ibid*. If opportunities were so limited in the English countryside, such widespread household and family formation would be much less prevalent, particularly since children constituted a clear economic burden until they reached the age of ten. Such a pattern contrasts sharply with that of the American Midwest at the same time: Schob's survey of townships in 1860 shows that only 25 percent or less of the laborers were married; Schob, *Hired Hands and Plowboys*, 268–72. In short, rural laborers in the American grain belt did not form families in the countryside as did their English counterparts.

sent in the English landscape. Year-long employment often included board, and, hence, subsistence costs were low for the English farm laborer.⁷⁵

The English worker contemplating a move to the city certainly faced living costs as high as those in the United States (\$55). The transfer wage (W_u) equalizing city and country earnings then amounted to an urban wage of \$225; including nonpecuniary income, estimated here as 30 percent of rural earnings, the transfer wage (W_u'), the wage required to move the unskilled laborer permanently from farm to city, becomes \$276:

$$\begin{array}{ll} W_u = C_u - C_r + W_r & W_u' = C_u - C_r + 130\%(W_r) \\ W_u = \$55 - \$0 + \$170 & W_u' = \$55 - \$0 + 130\%(\$170) \\ W_u = \$225 & W_u' = \$276.^{76} \end{array}$$

English manufacturers rightly saw little reason to introduce machines and tap the pool of rural labor, which was more expensive than the costs of hand-loom weavers (\$230) and less productive than skilled weavers. Adding in the costs of power looms made the new technology a losing proposition. Although cotton manufacturers used women on power looms and children on throstle spindles, they could not utilize unskilled rural male labor like their counterparts in the

⁷⁵ On continued use of yearly contracts and the rural hostility aroused in 1830 by attempts to put labor on a wage-rate basis, paying by the day or by the month, see Hobsbawm and Rudé, *Captain Swing*, 23–55. The drive to put English farm labor on a wage-rate basis would have made the English laborer much more like his American counterpart, although the labor intensity of English agriculture would have precluded the long periods of seasonal unemployment that typified American grain-farming regions; J. D. Chambers and G. E. Mingay, *The Agricultural Revolution, 1750–1880* (New York, 1966), esp. 133–34; and Jones, *Agriculture and the Industrial Revolution*, 211–33. Subsistence costs may have been higher in the south of England, where six-month or yearly contracts gave way to day work, piece work, and harvest wages and where the laborer usually provided his own room and board. We may estimate an extreme lower bound for rural net income by using the earnings of a single laborer (\$120) finding his own room and board except during the harvest and haymaking season of eleven weeks; subsistence costs then had to cover the remaining forty-one weeks. Jelinger Cookson Symons estimated that it cost 30 shillings per week to maintain a family of five (including two children over fifteen years of age) residing in or near a country town; Symons, *Art and Artisans at Home and Abroad with Sketches of the Progress of Foreign Manufacturers* (Edinburgh, 1839). Subsistence costs for a single laborer would then be about one-fourth that sum, 7.5 shillings per week, or \$73.80 for forty-one weeks. Net rural earnings would thus amount to \$46.20 (\$120–\$73.80). For Symons's work, see the compilation by Paul Uselding, "Wage and Consumption Levels in England and on the Continent in the 1830s," *Journal of European Economic History*, 4, (1975): 501–13. A higher estimate of perhaps 9 shillings per week seems justified for larger cities; see T. R. Gourvish, "The Cost of Living in Glasgow in the Early Nineteenth Century," *ECHR*, 2d ser., 25 (1972): 65–80. The fact that day workers usually received payments in kind equal to an additional 15 percent of their money earnings is consistent with the argument presented here. See Glenn Hueckel, "English Farming Profits during the Napoleonic Wars, 1793–1815," *EEH*, 13 (1976): 331–45.

⁷⁶ If a lower bound is preferred, we may use Kay's data showing a single laborer's earnings of \$120, from which maintenance costs of \$73.80 are deducted, leaving net rural earnings of \$46.20. Adding nonpecuniary income of \$36 pushes this total up to \$82.20. On the matter of urban living costs, Symons's evidence suggests about 7.5 shillings per week or \$93 per year; adding this to net rural earnings gives a lower bound of the transfer wage at \$175, a figure still considerably above the American level. Also, for wages of unskilled men in the textile industry, Symons indicated a weekly wage of \$4.32, which converts to annual earnings of \$216—a figure close to our theoretical estimate of \$225. Uselding, "Wage and Consumption Levels," 501–13. Symons's evidence further sustains our view of the cheapness of English skilled labor. Comparing England and France on the differential wage between skilled workers in various industries and unskilled men in the textile industry, the English exhibited a lower differential for machine makers, iron founders, and blacksmiths. Thus, it would appear that the low wages paid to skilled laborers would be a central issue in English economic history, yet, aside from the brilliant essay by E. J. Hobsbawm, this problem has received slight attention; see Hobsbawm, "Custom, Wages, and Work-Load in Nineteenth-Century Industry," in his *Labouring Men: Studies in the History of Labour* (New York, 1964), 244–70. On the continuing importance of cheap skilled labor in nineteenth-century industrialization, see C. K. Harley, "Skilled Labour and the Choice of Technique in Edwardian Industry," *EEH*, 9 (1974): 391–414.

American grain belt.⁷⁷ Accordingly, before 1840 the generally beneficent English countryside provided substantial earnings that encouraged rural population growth and discouraged the introduction of the new textile technology. These favorable employment opportunities in rural England were further underscored by the pattern of emigration and immigration. It is important to note that English society sent few emigrants to the low-wage United States. And, for that matter, Irish migrants preferred settlement in high-wage England rather than in the United States.⁷⁸

Labor costs in England and the American grain belt were precisely the reverse of Habakkuk's claim. The Middle Atlantic states contained low-cost, unskilled rural labor and expensive, skilled urban labor; firms there introduced machines to tap the supply of inexpensive labor, to cut costs, and to earn a producer's surplus. In England, skilled labor was comparatively cheap, and firms neglected the new technology unless less expensive child and female labor was available. The subsidy of low-priced labor thus offers a logical and plausible explanation for the introduction of machines; where unskilled labor was moderately expensive, as in England, machine technology advanced but slowly, and,

⁷⁷ The sex composition of the English cotton textile industry in 1835 resembled that of New England but contrasted sharply with the Mid-Atlantic grain belt. Exclusive of children, the work force in England in 1835 consisted of 55.4 percent women; in New England in 1822, 63.5 percent women; and in the Mid-Atlantic states in 1822, just 34.5 percent women; Phyllis Deane and W. A. Cole, *British Economic Growth, 1688-1959: Trends and Structure* (2d ed., Cambridge, 1967), 182-92; and *Digest of Accounts of Manufacturing Establishments*. The English preference for employing women with the new textile technology was thoroughly grounded in wage differences between unskilled men and women. Unskilled English women in textiles earned just 33.3 percent of the wages that their male counterparts earned, whereas women earned 70 percent in France, 57 percent in Switzerland, and 50 percent in Austria and Prussia. Once again, as in the case of cheap skilled English labor, the low wage scale for English women has received surprisingly little attention. For the wage comparisons, see Uselding, "Wage and Consumption Levels," 505; and Ivy Pinchbeck, *Women Workers and the Industrial Revolution, 1750-1850* (London, 1969).

⁷⁸ Various commentators upon our work have suggested that emigration from England contradicts our argument for generally high rural earnings in that nation. In fact, emigration from the English countryside was decidedly limited during the period under discussion—a central point in Arthur Redford's classic study, *Labour Migration in England, 1800-1850*, ed. W. H. Chaloner (3d ed., Manchester, 1976). Scholars who claim that English migration to the United States was large have been misled by the aggregation of English, Scottish, and Irish data. Indeed, the English contribution to American immigration was negligible between 1820 and 1845. During these years, England contributed just 31,922 persons, or 2.7 percent of the immigrants to the United States. Nor does Irish migration to the United States contradict our view. In the period 1820-33, Irish migrants to this country numbered 38,384. In fact, far more Irish went to England, though in the absence of official lists we must rely on Irish population estimates for our figures. As of 1833, native-born Irish accounted for nearly 60,000 people in the Lancashire cities of Manchester and Liverpool with at least 30,000 more in that county's countryside. Another 60,000 lived in London, and many other Irish immigrants were scattered in the smaller cities and towns of England and Scotland. As the author of an 1836 parliamentary report on the state of the Irish poor in Great Britain remarked, "They are to be found in greater or less strength in every manufacturing or commercial town from Aberdeen, Dundee, and Greenock to the central counties of England and the metropolis. There are also many Irish in the Isle of Man, the Island of Guernsey, and at Sheerness. Their roaming and restless habits appear to have carried them to every place where there was any prospect of obtaining profitable employment." House of Commons, *Report on the State of the Irish Poor in Great Britain, Parliamentary Papers*, vol. 34 (London, 1836), 433. To these permanent emigrants should be added the seasonal migrant workers who numbered over 57,000 in 1841 according to official, and probably conservative, estimates. In all, before the famine of the mid-1840s, the Irish moved predominantly to high-wage England rather than to the low-wage United States. See William J. Bromwell, *History of Immigration to the United States Exhibiting the Number, Sex, Age, Occupation, and Country of Birth of Passengers Arriving from Foreign Countries by Sea, 1819 to 1855* (reprint ed., New York, 1969); and *Report on the State of the Irish Poor*, 427-642. Also see Oliver MacDonagh, "The Irish Famine Migration to the United States," *Perspectives in American History*, 10 (1976): 357-446; and E. J. T. Collins, "Migrant Labour in British Agriculture in the Nineteenth Century," *ECHR*, 2d ser., 29 (1976): 38-59, esp. 49-50.

where it was extremely expensive, as in the cotton South, machines did not advance at all.

The immediate consequence of the success of the machine in American textile production was that firms reaped a producer's surplus and labor was exploited in the short term. Granted, rural labor improved its income by migrating permanently to urban jobs: annual incomes of unskilled urban workers were better than they would have been in the country because of steady, year-round employment. In fact, wages are not the main issue. More important is the share of value added going to labor, and on that score it appears that, although wages rose, labor's share of value added fell. The explanation is simple. As George Gibb understood so well, textile firms achieved this result by hiring disproportionately large numbers of low-cost laborers at the relative expense of skilled positions.⁷⁹ Unskilled wages did rise, but they began at such a low base that workers achieved little more than subsistence.

Mathew Carey was particularly outraged by the inadequate incomes earned by workers in Philadelphia during the 1820s and 1830s. Carey understood that these low incomes reflected a structural problem in the economy—what we have labeled as imperfect labor markets occasioned by agricultural seasonality.⁸⁰ In the seasonal farm economy of Pennsylvania, the unskilled laborer was part of a vicious cycle. Idle much of the year in the country, he left for the city and there discovered that his wages and earnings were also small because they were determined by what he had made in the countryside. In the short term, Carey's remedy for the urban poor was the compassionate charity of Philadelphians; in the long run, the vicious cycle was smashed by the emergence of perfectly competitive markets. By 1860, labor's share of value added in manufacturing had risen appreciably in New England and in all probability in the Middle Atlantic states. Thus, development based on cheap labor contained a self-correcting mechanism so that, as more firms entered the labor market, wages were determined increasingly by supply and demand rather than by the transfer wage based on seasonal rural earnings. Simultaneously, the asset of cheap labor passed on to Midwestern wheat and corn states, which rapidly re-enacted the course of events that had occurred a half-century earlier in the Mid-Atlantic states, as exemplified by the case of Philadelphia.

ALTHOUGH IT IS CURRENTLY FASHIONABLE to discount the importance of staple theory, our research has confirmed the central role of these commodities in the processes of economic growth, urbanization, and the transition from pre-industrial to industrial economies.⁸¹ From 1800 to 1860 the character of agriculture

⁷⁹ Gibb observed that the decline in wage shares "did not come about as a result of individual pay cuts, but because of a general shift toward employment of new help at lower rates. . . . This shifting downward of wage rates seems to indicate that machine-building techniques were being systematized to permit the replacement of highly skilled mechanics by lower-paid, more specialized, and less skilled men." Gibb, *The Saco-Lowell Shops*, 54–55.

⁸⁰ Carey, *Appeal to the Wealthy of the Land*, 3–36; and Sullivan, *The Industrial Worker in Pennsylvania*, 29–157.

⁸¹ For an extended examination of the colonial economy, see Carville Earle and Ronald Hoffman, "Staple Crops and Urban Development in the Eighteenth-Century South," *Perspectives in American History*, 10 (1976): 7–78.

and the costs of labor in the United States and England provided the foundation for the development of their respective modern industrial systems. In each country the physical nature of the staples established the levels of labor required for their cultivation and, as a corollary, the incomes that could be earned by rural laborers. Where labor requirements were seasonal, as in the grain belt of the United States, annual rural earnings were low. Therefore, the transfer wage was correspondingly low, and the pool of low-priced labor arising from the staple economy could be used in conjunction with the new technology by urban entrepreneurs seeking to reduce the costs of manufactures. By contrast, the cotton economy of the Southern United States created year-round labor requirements and offered high rural earnings, which would-be urban entrepreneurs were unable to match. Southern cotton thereby retarded the processes of urbanization and industrialization that were made possible by Northern grains. Finally, in England, the intensification of the agricultural schedule did not preclude urbanization, but, by raising the transfer wage and the cost of unskilled urban labor, it removed a critical incentive for investments in machine technology.⁸²

Thus, staple theory offers the most satisfactory interpretation of pre-industrial economic development as a precursor of industrial economies, and it also explains the transition to industrialization. By clarifying the functioning of labor markets and the operation of the transfer wage, staple theory not only accounts for urbanization but also affords a new interpretation of technological change and the origins of industrial economy.

⁸² The structure of the American and British labor markets also had profound effects on the politics of labor and the nature of the working class. The social tensions caused by the displacement of the American skilled artisan by the machine and the unskilled worker were mitigated in England by the relatively high costs of unskilled workers and the slower pace of technical change. Despite repeated efforts at creating a unified American working class, such a coalition was doomed by the wide differential in the wages of the skilled and the unskilled, by the divergence of their economic interests, and by the recurrent threat of displacement by the machine. These deep-seated divisions in the American labor movement repeatedly undermined working-class alliances during the nineteenth and early twentieth centuries. For elaboration, see Sari Bennett and Carville Earle, "The Failure of Socialism in the United States: A Geographical Interpretation," GALI Working Paper, no. 4 (Baltimore, 1980).

The South from Self-Sufficiency to Peonage: An Interpretation

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OF ALL OF THE COMMENTS made by contemporary observers of the antebellum South, the most nearly universal was that Southerners loved their leisure—or, as hostile observers said, they were lazy. “The climate and external appearance of the country conspire to make them indolent, easy, and good-natured,” wrote Andrew Burnaby in 1759; “they seldom show any spirit of enterprise, or expose themselves willingly to fatigue. . . . [They] are content if they can live from day to day.” Later in the eighteenth century Charles Woodmason described back-country Southerners as ignorant and “very Poor—owing to their extreme Indolence.” “They delight in their present low, lazy, sluttish, heathenish, hellish Life, and seem not desirous of changing it. Both Men and Women will do any thing to come at Liquor, Cloaths, furniture, &c. &c. rather than work for it.” In 1810 Charles J. Ingersoll, like many another observer, attributed the “constitutional indolence” of Southerners to the institution of slavery. “Man will not labour,” he claimed, “where he can substitute slaves.” But indolence was not confined to slaveowners. “Among the lower classes of [Southern] society,” Henry C. Knight reported in 1824, “the Canaan richness of the land is productive, among better fruits, of much indolence. . . . Too many, instead of resting on one day in seven, work only on one day in six; and therefore ever remain poor.”¹

Southerners of all social classes would have rejected the naive and culture-bound assumption that people naturally seek to better their condition. In the 1830s Charles F. Hoffman attempted to convince a Southerner of “the superiority which greater industry and acquired knowledge of useful facts gives the northern man.” The Southerner replied, “If the people did not live up to other

We would like to acknowledge the helpful criticism that we received from John Hebron Moore, John D. W. Guice, William N. Parker, and Terry G. Jordan.

¹ Burnaby, *Travels through the Middle Settlements in North-America* . . . (Ithaca, N.Y., 1960), 22, 27; Woodmason, *The Carolina Backcountry on the Eve of the Revolution: The Journal and Other Writings of Charles Woodmason, Anglican Itinerant*, ed. Richard J. Hooker (Chapel Hill, 1953), 52; Ingersoll, as quoted in Warren S. Tryon, ed., *A Mirror for Americans: Life and Manners in the United States, 1790–1870, as Recorded by American Travelers*, 1 (Chicago, 1952): 20; and Knight, *Letters from the South and West* . . . (Boston, 1824), 93. Also see Kenneth R. Wesson, “Travelers’ Accounts of the Southern Character: Antebellum and Early Postbellum Period,” *Southern Studies*, 17 (1978): 305–18.

people's ideas, they lived as well as they wanted to. They didn't want to make slaves of themselves; they were contented with living as their fathers lived before them." And as Frederick Law Olmsted pointed out in the 1850s, "The Southerner has no pleasure in labor. . . . He enjoys life itself. He is content with being. Here is the grand distinction between him and the Northerner; for the Northerner enjoys progress in itself. He finds his happiness in doing. Rest, in itself, is irksome and offensive to him."²

SUCH IMPRESSIONISTIC OBSERVATIONS may be cited endlessly; they may also be borne out by some rough calculations. According to the census of 1850, there were, in round numbers, 569,000 farms in the South, of which about 30 percent held at least one slave. Not all farms with slaves, however, produced enough agricultural staples to qualify for the census bureau's classification of a "plantation": the requisite production was 2,000 pounds of cotton, 3,000 pounds of tobacco, 20,000 pounds of rice, or any amount of sugar cane or hemp. Of the roughly 170,000 farms with slaves, only 101,000 met that qualification. The census classified plantations by principal crop: 74,000 in cotton, 15,700 in tobacco, 8,300 in hemp, 2,700 in sugar cane, and 550 in rice.³

Let us see how much work was needed to constitute a plantation, census-style. As for cotton, the average yield per acre for the South as a whole was about 530 pounds of seed cotton, or about 180 pounds of ginned lint. The average hand could cultivate about ten acres of cotton, meaning about 1,800 pounds of baled cotton, plus enough corn and other provisions for two persons. Since 2,000 pounds of cotton made a "plantation," a slaveless one-family farm could readily qualify—if the head of the household worked at it. Tobacco required slightly more effort. An average hand under normal conditions could work two acres, with a gross yield of 1,600 pounds, besides corn and other necessary provisions. Since 3,000 pounds of tobacco made a "plantation," a slaveless one-family farm with one teen-aged son could readily qualify—if the household worked at it. Rice and sugar plantations were about five times as labor intensive, but, among them, farms producing these crops accounted for only about 3 percent of all plantations.⁴

These general propositions are borne out by the example of Butler County, Alabama, a cotton-producing area for which individual data happen to have been compiled. Of the 553 farms listed by the 1850 census for Butler County, 196 produced enough cotton to meet the census definition of a plantation; 30 of those plantations had no slaves. The widow Sarah Bailey, for example, had seven female children and one white adult male hired hand, but her farm pro-

² Hoffman, *A Winter in the West* . . . , 2 (New York, 1835): 226; and Olmsted, *The Cotton Kingdom*, ed. Arthur M. Schlesinger (New York, 1953), 616.

³ Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978), 34; U.S. Census Office, *Statistical View: A Compendium of the Seventh Census, 1850* (Washington, 1854), 178; and Lewis C. Gray, *History of Agriculture in the Southern United States to 1860* [hereafter, *Southern Agriculture*], 2 vols. (new ed., Gloucester, Mass., 1958), 1: 529.

⁴ Gray, *Southern Agriculture*, 2: 708-09, 776, 912.

duced five 400-pound bales of cotton plus food enough for her family. Dan Dean, a young man in his twenties who farmed one hundred and fifty acres by himself, produced twenty-five bales of cotton. Joshua McCain, a tenant farmer in his forties with a wife, a teen-aged son, and two young daughters, produced forty bales. All three of these farms qualified as plantations.⁵

If plantations required so few laborers and there were only 101,000 of them, one may ask, what were the folks on those other 468,000 farms doing? Certainly they were not working at raising the great Southern agricultural staples, or at least not working hard at it. Robert Perry, another Butler County farmer, may not have been atypical: he owned twenty-three slaves, twelve of whom were adults, yet he produced only four bales of cotton (not enough to qualify as a plantation), along with only 700 bushels of corn, 20 bushels of oats, 100 bushels of sweet potatoes, and 40 pounds of wool. Indeed, fully 35 percent of the county's farm units with two or more adult slaves failed to produce enough to meet the census definition of a plantation.

Now let us see how much work was necessary to grow what those plantations did produce. In 1850, the farms and plantations in the South produced about 862,400,000 pounds of cotton, 185,000,000 pounds of tobacco, 215,300,000 pounds of rice, 236,800,000 pounds of sugar, and 6,700,000 pounds of hemp. The readily available data about the work involved in producing each of these crops suggest that, in round figures, each "hand" could produce 1,800 pounds of ginned cotton, 1,600 pounds of tobacco, 3,600 pounds of rice, 4,000 pounds of sugar, or 6,000 pounds of hemp, in addition to the corn and other provisions necessary for himself and another. Dividing the productivity of an average hand into the South's total production in 1850 yields the total hands necessary to produce each crop: 479,111 hands to produce the cotton, 115,625 to produce the tobacco, 59,806 to produce the rice, 59,200 to produce the sugar, and 1,117 to produce the hemp—a total of 714,859 hands.⁶

To estimate the work load involved in this production, we must define what constituted a hand. Male slaves between fifteen and fifty years of age comprised about a quarter of the slave population, but not all, of course, were field hands. About 5 percent worked in "industry," including those working as craftsmen on plantations. Women and children also worked as field hands on most plantations; they were reckoned as the equivalent of one-half to four-fifths of a hand, depending on the kinds of crops. One Alabama cotton planter in 1846 had thirteen slaves, "for the most part boys and women," whom he figured as "equivalent to about 10 good hands." As a rule of thumb, it seems conservative to follow Lewis C. Gray's assumption: one hand per two slaves. (Cliometricians,

⁵ These data were compiled by Michael Daniel as part of a forthcoming doctoral dissertation at the University of Alabama.

⁶ Gray, *Southern Agriculture*, 2: 708–09, 730–31, 776, 822, 912. And production has been calculated from Donald B. Dodd and Wynelle S. Dodd, *Historical Statistics of the South, 1790–1970* (University, Ala., 1973). Robert W. Fogel and Stanley L. Engerman have estimated that cotton production in 1790 required "the full-time labor of about two thousand hands, a mere one half of one percent of the labor required for cotton in 1850"; Fogel and Engerman, *Time on the Cross: The Economics of American Negro Slavery* (Boston, 1974), 43. They have, therefore, calculated the number of slaves necessary to produce the 1850 cotton crop at four hundred thousand, one-sixth fewer than we have estimated.

incidentally, normally assume a much higher ratio of hands to slaves: Roger L. Ransom and Richard Sutch, for example, figure that almost 70 percent of the whole slave population worked in the fields.⁷) If we assume that two slaves equaled one field hand, the South's 3,200,000 slaves in 1850⁸ means that there were 1,600,000 hands. Let us make the palpably absurd assumption that slaves did *all* of the work involved in *all* of the production of *all* of the South's agricultural staples; in that case, 715,000 hands would have been required. Again, one may ask, what were the other 885,000 field hands doing?

Let us look more closely at the work load involved. We have reasonably reliable estimates of the man-hours (or "hand-hours") required to produce cotton and corn. It happens that those two crops constituted the overwhelming majority, probably nine-tenths, of the entire crop production in the state of Alabama. It took an average of approximately 0.8 hand-hours to produce a pound of cotton and approximately 2.5 hand-hours to produce a bushel of corn. Alabama produced 225,771,600 pounds of cotton and 28,754,048 bushels of corn in 1850. That figures out at 180,617,280 hand-hours to produce the cotton and 71,885,120 hand-hours to produce the corn. Let us again assume that slaves did all of the work. There were 342,844 slaves in the state, or about 171,422 hands. Dividing the hands into the requisite hand-hours, we find that the average hand had to work 1,473 hours per year to produce the cotton and corn crops of Alabama in 1850, assuming no white ever lifted a finger. Converting this figure into ten-hour days, we find that each field hand would have had to work 147 days a year. In Mississippi, the comparable figures are 1,362 hours per year per hand, or 136 ten-hour days a year.⁹

It is revealing, in regard to work load, to compare areas with different population mixes and different kinds of soils. Greene County, Alabama, a rich cotton-growing area with a ratio of slaves to whites of almost two and one-half to one, lay in the heart of the black belt; assuming that only blacks worked there and that only half of the blacks were field hands, we infer that it took 1,045 hours per year per hand to produce the county's output of cotton and corn (104 ten-hour days per year, or two days per week). In Tishomingo County, Mississippi, a relatively poor, upland area with a ratio of seven whites to one slave, it took 2,629 hand-hours to produce the cotton and corn—again assuming that only slaves worked, which in this case seems more than a little extravagant. In Butler County, Alabama, which had medium-quality land and a two-to-one ratio of whites to slaves, the work load for producing the cotton and corn was 1,139 hand-hours if only slaves worked. (The figures do not vary appreciably if calculations are made only for plantations employing slave labor; in that case,

⁷ Gray, *Southern Agriculture*, 1: 542, 544, 549, 2: 664–65; Ransom and Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation* (Cambridge, 1977), 233–34; and Robert S. Starobin, *Industrial Slavery in the Old South* (New York, 1970), 11, 236. Fogel and Engerman have estimated that 1,150,000 of the 2,000,000 slaves on cotton plantations—57.5 percent—were field hands; *Time on the Cross*, 42. Calculating the labor of a female at three-quarters of that of a male results in a ratio of one hand for every two slaves.

⁸ U.S. Census Bureau, *Historical Statistics of the United States: Colonial Times to 1957* (Washington, 1961), 9.

⁹ Per man-hours labor requirements have been derived from U.S. Department of Agriculture, *Progress of Farm Mechanization*, Miscellaneous Publication, no. 630 (Washington, 1947), 3.

the work load on Butler County plantations was 1,109 hours per hand per year.)¹⁰

Several observations should be made about these estimates. First, despite the numerical precision with which they are expressed, they are only estimates, and necessarily rough ones at that. Second, they express *averages*, which encompass but do not delineate the variations of yields and labor requirements from farm to farm. These turned upon such questions as the quality of soil, the intelligence of planters, and how hard (as opposed to how long) field hands worked. But the greatest difficulty with the estimates is that they do not take into account the labor requirements of the other things there were to do on a farm, besides raise market crops. In addition to the growing of food for home consumption, there were animals to tend, fences to build and mend, ditches to dig, wood to chop, land to clear, and various other chores. To be sure, many of the chores on Southern farms and plantations were inherently less time-consuming than on Northern farms; Southerners engaged very little in the labor-intensive dairy business, for instance, and their beef cattle and hogs did not require shelter or feeding in the winter. Travelers' descriptions of the disreputable state of Southern farms, moreover, suggest that routine chores often went undone. That impression is strengthened by the relatively small proportion of improved acreage in the South: of the South's nearly 557,000,000 total acres, fewer than 10 percent were improved in 1850. Only in Maryland, Virginia, Delaware, and Kentucky was more than 20 percent of the land under cultivation. In the other states of the South improved acreage ranged from less than 1 percent in Texas and Florida to nearly 19 percent in South Carolina.¹¹ Southerners were, obviously, far from compulsive in working to clear their land.

Unfortunately, the kinds of documents that would shed light on these matters—plantation records—are intrinsically biased. Southerners who kept detailed records were atypical, and it seems reasonable to assume that those who did so tended to be considerably more work-oriented and profit-oriented than was normal. For instance, there are the conscientiously maintained records of James Mallory, the owner of twenty-two slaves (nine males and thirteen females), half of them between the ages of fifteen and fifty. In 1850 Mallory produced, on his two hundred improved acres in Talladega County, Alabama,

¹⁰ U.S. Census Office, *Seventh Census of the United States, 1850: An Appendix* (Washington, 1853), 421, 430, 431, 447, 457, 458, and Manuscript Census Returns, Seventh Census of the United States, 1850: Butler County, Alabama, Population, Slave, and Agricultural Schedules (microfilm copies, University of Alabama).

¹¹ The figures for butter and cheese consumption reflect the insignificance of dairy production in the South. Per capita consumption of butter in the North in 1850 was, for example, 39 pounds in Vermont, 26 pounds in New York, 22 pounds in New Hampshire, 18 pounds in Michigan, and 17 pounds in Ohio. In Alabama, the per capita consumption of butter in 1850 was 5.2 pounds. And the national per capita consumption of cheese was 4.67 pounds in 1850; in Alabama it was 0.04 pounds. See Sam Bowers Hilliard, *Hog Meat and Hoeecake: Food Supply in the Old South, 1840-1860* (Carbondale, Ill., 1972), 135; Census Bureau, *Historical Statistics of the United States*; and Census Office, Manuscript Census Returns, Seventh Census: Butler County, Alabama. The neighbor of a Yankee woman living in the South "owned fifty cows, she supposed, but rarely had any milk and scarcely ever any butter, simply because his people were too lazy to milk or churn, and he wouldn't take the trouble to make them"; Olmsted, *Cotton Kingdom*, 302-03. Regarding improved land, see Frank L. Owsley, *Plain Folk of the Old South* (Baton Rouge, 1949), 48-49; Thomas C. Donaldson, *The Public Domain: Its History, with Statistics* (Washington, 1884), 28-29; and U.S. Census Office, *Seventh Census of the United States, 1850* (Washington, 1853), Table 55, lxxxii-lxxxiii.

17,600 pounds of cotton, 2,200 bushels of corn, and small quantities of wheat, rye, oats, tobacco, peas, and potatoes. His production of four bales per hand was nearly twice the average for the best cotton lands in southern Alabama and Georgia.¹²

Even plantation records, however, tend to confirm our estimates. It is clear that slaves usually did not work on Sundays, holidays, and days when they were sick. Mallory's slaves had 105 days off, fully or partially, in 1850. Work rules on the Weston plantation in South Carolina specified that, in addition to sick days, slaves would receive more than 60 holidays per year: "No work of any sort or kind is to be permitted to be done by negroes on Good Friday or Christmas day, or on any Sunday. . . . The two days following Christmas day; the first Saturdays after finishing threshing, planting, hoeing, and harvest, are also to be holidays. . . . Only half task is to be done every Saturday, except during planting and harvest. . . . No negro is to be put into a task which . . . [he] cannot finish with tolerable ease."¹³

What is more, plantation records indicate that, in estimating the work load, only a modest allowance needs be made for labor other than that devoted to raising staple crops. For instance, using our formula for determining production we find that half of the slaves on James Mallory's plantation had to work only 207 days in 1849 to produce all the cotton and corn grown on the place. Mallory's detailed journal indicates that he actually kept his slaves busy 260 days during the year—which comes to 53 days, or approximately 25 percent, more than the estimated crop-production work load. If we add 25 percent to the average figure cited earlier, the total work load required of slaves is increased to something in the order of 1,600 to 1,800 hours, or 160 to 180 ten-hour days, per year. By contrast, the most persuasive estimate the authors have seen of the work load on free Northern farms, that made by John F. Olson, places the average Northern agricultural work-year at a minimum of 3,100 hours.¹⁴

¹² James Mallory, Journal, 1843-1877 (original owned by Edgar A. Stewart of Selma, Alabama; microfilm copy, University of Alabama). Average figures for southern Georgia have been calculated from Roland M. Harper, "Development of Agriculture in Lower Georgia from 1850 to 1880," *Georgia Historical Quarterly*, 6 (1922): 111; Alabama averages have been calculated from Census Office, Manuscript Census Returns, Seventh Census.

¹³ John Spencer Bassett, ed., *The Southern Plantation Overseer as Revealed in His Letters* (Westport, Conn., 1968), 23-38. For typical rules and holidays, also see—in addition to such plantation records as those cited in note 14, below—*DeBow's Review*, 21 (1856): 617-20, and 22 (1857): 38-44, 376-81; Ulrich B. Phillips, *American Negro Slavery* (Gloucester, Mass., 1959), 261-90; and Gray, *Southern Agriculture*, 1: 557.

¹⁴ Mallory, Journal, 1843-1877; and Olson, "Clock-Time vs. Real-Time: A Comparison of the Lengths of the Northern and the Southern Agricultural Work-Years" (unpublished essay). Professor Olson, of the University of Connecticut, Storrs, kindly provided us with a copy of his essay; his excellent study is quite the most sophisticated we have seen. Olson has calculated the work day of Northern farmers, on a year-round basis, at 10.1 to 11.4 hours, depending on the kind of farm. (Much higher figures for Northern farm workers are given in David E. Schob, *Hired Hands and Plowboys: Farm Labor in the Midwest, 1815-60* [Urbana, Ill., 1975], 254-55.) Olson has calculated the slave work day at 7.9 to 10.5 hours, depending on crops and seasons. He has estimated that plantation slaves worked more days than we have estimated, figuring 279 days for cotton plantations and an average of 281 days for all kinds of plantations combined. But 279 days is only 19 days, or 7.3 percent, more than is recorded in the Mallory Journal; and, if due allowance is made for the fact that Olson's figures are based exclusively on plantation records (twelve sets) with their inherent bias of showing more than average work, his figures square fairly closely with ours. For similar plantation records, see, for example, William F. Shields, *Plantation Records*, Mississippi Department of Archives and History, Jackson, Miss.; John D. Ashmore, *Plantation Journal*, Southern Historical Collection, University of North Carolina, Chapel Hill,

Our estimates are very different from the estimates other scholars have made. Lewis C. Gray estimated the slaves' work day at 15 to 16 hours during the busy season; Eugene Genovese suggested 12 to 15 hours a day; Robert Fogel and Stanley Engerman say 12.7 to 13.6 hours; and the latest cliometricians in the field, Roger Ransom and Richard Sutch, place the figure at 12 to 14 hours for males, 10 to 13 for women and children. Ransom and Sutch estimate the number of days slaves worked per year at 268 to 289, and estimate the total hours devoted to agriculture per year at 3,055 to 3,965 for adult male slaves and 1,913 to 3,507 hours for women and children.¹⁵

Such estimates are entirely unrealistic. In the first place, they do not square with the almost universal observations of contemporaries. One observer stated that slaves worked "at so slow a rate, the motion would have given a quick-working Yankee convulsions"; another noted that slaves "never worked enough to tire themselves," but merely went "through the motions of labor without putting strength into them"; while a foreigner thought that, in contrast to busy Northern farms, in the South "all mankind appeared comparatively idle."¹⁶ In the second place, had so many hands been working so hard, Southern agricultural production would necessarily have been several times higher than it was. And, most importantly, the cliometricians' figures reveal that these scholars did not grow up in the rural South. Had they done so, they might have noticed that there are not enough hours in the day or days in the year available for the amount of agricultural labor they insist took place. The length of the day in the South ranges from about ten and a quarter hours at the winter solstice to just under fourteen hours at the summer solstice, and on a year-round basis the average day is about twelve hours. There is also winter, when agricultural activities are somewhat limited, and there is rain.¹⁷ A sample of Mallory's journal entries tells the story: January 28, "Ground too wet to plough"; February 3, "A very

N.C.; William J. Dickey, *Diary*, University of Georgia, Athens, Ga.; and John Horry Dent, *Journals*, University of Alabama, Tuscaloosa, Ala. None of these data, incidentally, upsets Fogel and Engerman's contention that slave-plantation labor was more efficient than Northern free agricultural labor; if anything, the fact that slaves worked fewer hours than they reckoned buttresses their case. The economics of scale apply whether laborers are working hard or not. What is more important, the economics of the division of labor apply. The authors of the present essay, having worked on farms, are conscious of the fact that Adam Smith's famous example of the pin factory is applicable to farm work as well as to industrial work.

¹⁵ Gray, *Southern Agriculture*, 1: 557; Fogel and Engerman, *Time on the Cross*, 208; and Ransom and Sutch, *One Kind of Freedom*, 233-36.

¹⁶ *American Agriculturalist*, 9 (1950): 93; Frederick Law Olmsted, *A Journey in the Seaboard Slave States . . .* (New York, 1856), 91; and Basil Hall, *Travels in North America in the Years 1827 and 1828*, 3 (Edinburgh, 1829): 117.

¹⁷ The number of hours in the day at southern latitudes can be calculated from any almanac. Gray's notion of the length of a day may have been influenced by where he taught—at the Universities of Wisconsin and Saskatchewan, in northern latitudes where summer days are much longer, as many as sixteen to nineteen hours. When that is taken into account, Gray's data are not as different from our estimates as might at first appear. He indicated that field hands had two and a half hours off for meals and rest during the course of a work day—which would bring a fourteen-hour day down to eleven and a half hours of work and a thirteen-hour day down to ten and a half. Moreover, Gray stated that on smaller and "noncommercial" units—a description that would, in fact, include the vast majority of farms with slaves—"labor was more casual." *Southern Agriculture*, 1: 577. For examples of work that has taken the weather into account, see Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), 567-68; and Hilliard, *Hog Meat and Hoecake*, 29-31. Neither discussion, however, pays attention to the length of the day. Olson has been realistic about the length of the work day; "Clock-Time vs. Real-Time." Also see Joseph Holt Ingraham, *The South-West, by a Yankee*, 2 (New York, 1835): 276.

[heavy] rain last night. . . . Ploughing kept back"; February 5, "Cold and ground frozen"; February 14, "Snowing very freely"; March 17, "A tremendous rain fell, washing the land very much and greatly retarding planting operations"; March 21, "Rain, Rain, Rain"; March 22, "All business stopped by a heavy rain"; March 26, "Started the ploughs . . . but was stopped by rain"; April 2, "Planting . . . stopped by rain"; April 10, "Rainy and windy, all planting stopped"; April 11, "Too wet to work"; April 17, "Had a very heavy rain, stopped all planting"; April 26, "Stopped from ploughing by rain"; May 8, "Stopped by a heavy rain from work"; May 30, "It is showery, stopped from harvesting"; July 8, "All business stopped about 10 oclock by a heavy rain"; July 11, "All hands driven from the field by a torrent of rain"; and so on.¹⁸ Finally, it should be borne in mind that our estimates are of the maximum hours that field hands had to work to produce the Southern staples. Since whites—operators of those other 468,000 farms mentioned earlier—obviously did some of the farming and since we have probably understated the percentage of slaves who worked the land, the work load of the slaves was obviously less than that indicated.

Now let us estimate how hard those ordinary white Southerners, the "plain folk," worked. For that purpose, let us relieve the slaves of the burden of raising corn entirely and assume that all of it was raised by the sweat of the brow of the plain folk. Corn production in Alabama in 1850 amounted to 67.4 bushels per white person. Since the average hand required 2.5 hours of work to produce a bushel of corn, the per capita work load would have been 168.5 hours per year; if we assume that only half the whites tilled the fields, the work load soars to 337 hours a year—five or six weeks of work.¹⁹

Just how little work it took to subsist, which is about all most white Southerners cared to do, is revealed by the 1850 census data for Butler County, Alabama. Consider, for example, the efforts of four randomly selected non-slaveholding white families whose landholdings varied from more than 500 acres to none at all: Archibald Dickinson and his wife, who had four children living at home, owned 560 acres, 120 of which were improved, valued at \$3,000; the Samuel Farrow family of eight owned 80 acres, 35 of which were improved, worth \$150; John Cleghorn, his wife, and their three children owned 35 acres, only 1 of which was improved, valued at \$40; and the Jesse Armstrong family of eight were tenants who owned no land. All of these families owned livestock, mainly hogs and beef cattle. Dickinson claimed some 200 swine, 25 beef cattle,

¹⁸ Mallory, *Journal*, 1843–1877. The entries in Mallory's *Journal*, however, while representative, actually understate the number of days partially or totally lost to rain. The U.S. Oceanic and Atmospheric Administration has calculated the average number of days with measurable precipitation for fifteen Southern cities: Atlanta, Baltimore, Charlotte, Columbia, Houston, Jackson, Little Rock, Louisville, Memphis, Mobile, Nashville, New Orleans, Norfolk, Raleigh, and Richmond. The number of days with measurable rainfall in these cities ranges from 102 per year in Little Rock to 128 in Atlanta and Baltimore; the average number of days is 113.5, or about 2 days of rain per week. Seasonal variations, however, are considerable, with a range of 6.5 days in October (or about 1 day in 5) to 11.1 days in July (more than 1 in 3). Except for July, precipitation is most frequent from mid-December through mid-April—the time for off-season chores and for spring plowing and planting—when it rains on the average of every third day. These data have been calculated from U.S. Department of Commerce, *Statistical Abstract of the United States* (Washington, 1972), 185.

¹⁹ Labor for corn production has been calculated from U.S. Census Office, *Seventh Census of the United States*, 1850, 421, 431.

and various other animals worth \$700; Farrow's livestock came within \$25 of equaling the value of his farm; Cleghorn's exceeded his farm's value by \$60; and the landless Armstrong possessed animals worth \$200. Dickinson and Farrow grew no cotton at all; Cleghorn and Armstrong made only one bale each, and these along with the animals they slaughtered accounted for nearly all of their income. Dickinson concentrated on corn and livestock; he raised some 1,200 bushels of corn worth about \$750 and slaughtered animals worth another \$100. Farrow also devoted most of his attention to corn and animals, though with more modest results—he produced only 200 bushels of corn worth about \$126 and slaughtered only \$50 worth of animals. Together the twenty-three people of working age in these four families—seven male and two female teenagers, four males in their twenties, three females in their thirties, and four males and three females over forty—produced goods valued at approximately \$1,280: corn worth \$910, slaughtered animals worth \$220, cotton worth \$90, home-made goods worth \$40, and wool worth \$20. The work required to produce these items took about 9,713 hours: 3,630 hours of labor to grow 1,452 bushels of corn; 640 hours to make two bales of cotton; 400 hours to produce home-made goods; 2,778 hours to milk, feed, and care for 15 cows; 2,265 hours to tend, brand, mark, slaughter, or shear 8 horses, 10 oxen, 54 beef cattle, 58 sheep, and 239 hogs. This means that, to achieve the total production of the four families in 1850, each of the twenty-three working-age family members, sharing the labor equally, would have had to work only about 423 hours, or approximately eleven forty-hour weeks, per year.²⁰

CONTEMPORARY OBSERVERS, IN OTHER WORDS, WERE RIGHT when they reported that most antebellum Southerners were contemptuous of work and did as little of it as possible. The reporters were wrong, however, when they declared that, as

²⁰ Information on these and other Butler County families was compiled by Michael Daniel from manuscript census returns. In 1850, the price of cotton averaged 11.7 cents per pound in the New Orleans market; corn averaged 66 cents a bushel in New Orleans and 59 cents a bushel in Virginia. It therefore seems reasonable to use the figure of 63 cents a bushel as perhaps the average inland Alabama price. For sources for the time required to produce corn (2.5 hours per bushel) and cotton (0.8 hours per pound), see note 5, above. The price of common wool, which was 28 cents a pound in Boston in 1849, could not have been more than 20 cents a pound in Alabama, if that much. We have assumed that the time necessary to produce home-manufactured items would correspond to their value; since daily wages in 1850 averaged \$1.00, we have estimated that home-made goods worth, say, \$20.00 would require roughly 20 ten-hour days to produce. Milking and related activities probably took about 30 minutes per cow per day. Perhaps we have allotted too much time to animal husbandry, especially to the care of hogs and beef cattle. The Southern method of tending animals required remarkably little time and effort. Even sheep, as Paul W. Gates has observed, "required less . . . attention than raising dairy cattle." We allotted no extra labor to the raising of sweet potatoes and cowpeas because the amount grown by the families examined here was negligible. They raised some sweet potatoes, but only two families produced as many as usually were grown on half an acre. Cowpeas, simply sown in the corn rows as the corn was "laid by," grew easily in great abundance and crowded out weeds. An antebellum Georgian explained that cowpeas "will yield some six or eight bushels per acre, in addition to the corn, and serve to fatten our stock after we have gathered the corn, and what peas we want." Lewis C. Gray pointed out that in the colonial period, without plows, "one man could produce 100 bushels of corn, 20 bushels of beans and peas, and from 800 to 1,000 pounds of tobacco" and that by 1839 in South Carolina it was customary for a single hand to raise twelve or fifteen acres of cotton and the same amount of corn. None of the Butler County people studied remotely approached that production. Gates, *The Farmer's Age: Agriculture, 1815-1860* (New York, 1960), 221, 225; Gray, *Southern Agriculture*, 1: 27, 450, 2: 469, 812-13, 824, 827, 1027, 1039; and Hilliard, *Hog Meat and Hockcake*, 174, 177.



Figure 1: Log cabins with sandstone chimneys typical of the antebellum period; Colbert County, Alabama. Photograph taken April 13, 1922 by Roland Harper; now part of the William Stanley Hoole Collection, University of Alabama.

a result, Southerners were poor. Obviously the planters were well off; although Fogel and Engerman have been challenged in many particulars—especially by Herbert Gutman, Gavin Wright, and Ransom and Sutch—cliometricians are agreed that slave plantations were profitable. And, although there has been quibbling about the “exploitation rate,” most scholars now concede that the material well-being of the slaves was at least close to being on a par with that of free Northern farmers. Eugene Genovese has declared that the slaves were better off than peasants in most of the rest of the world. Had these scholars pursued the matter more thoroughly, they would have found that American slaves worked far less and lived far better than did the free peasants of France, whose lot in turn was better than that of the serfs of eastern Europe, who in turn were in an agrarian heaven compared to farm workers in Africa and Asia.²¹

But what of those leisurely Southern white plain folk? Superficially, they did seem to have been impoverished, at least in comparison with their hard-working Yankee brethren, for they tended to live in squalor. Olmsted, in a typical passage that can be found with variations in the accounts of numerous other travelers, wrote of an East Texas house that “was more comfortless than nine-tenths of the stables of the North,” a judgment that his graphic description verifies.

²¹ See Fogel and Engerman, *Time on the Cross*; Herbert Gutman, *History and the Numbers Game: A Critique of Time on the Cross* (New York, 1975); Wright, *The Political Economy of the Cotton South*; Ransom and Sutch, *One Kind of Freedom*; and Genovese, *Roll, Jordan, Roll*, 61–64. Ransom and Sutch have been particularly critical of Fogel and Engerman’s calculation of the exploitation rate, placing it much higher; *One Kind of Freedom*, 169, 203–15. On the question of profitability, see Alfred H. Conrad and John H. Meyer, eds., *The Economics of Slavery and Other Studies in Econometric History* (Chicago, 1964); and Hugh Aitken, ed., *Did Slavery Pay?* (Boston, 1971). For an enlightening account of the lot of French peasants, see Eugen Weber, *Peasants into Frenchmen: The Modernization of Rural France, 1870–1914* (Stanford, 1976).



Figure 2: Log house with two sandstone chimneys typical of those occupied by postbellum sharecroppers and tenants; near Kennedy, Lamar County, Alabama. Photograph taken October 18, 1911 by Roland Harper; now part of the Hoole Collection, University of Alabama.

"There were several windows, some of which were boarded over, some had wooden shutters, and some were entirely open. There was not a pane of glass. The doors were closed with difficulty. We could see the stars, as we lay in bed, through the openings of the roof; and on all sides, in the walls of the room, one's arm might be thrust out."²² That description sounds for all the world like the direst poverty; and it is an entirely characteristic representation of the appearance of most antebellum Southern homes. But appearances were deceptive. The "farmer" that Olmsted was describing happened to own a thousand acres of land—none of which he tilled—and more hogs and cattle than he could count. His place was teeming with deer and other game, but "he never shot any; 'twas too much trouble. When he wanted 'fresh,' 'twas easier to go out and stick a hog."²³

That is the key to how the plain folk lived: in the literal sense of the phrase, they lived "high off the hog." Virtually all, even those who owned no land, owned animals. No one actually needed to own land, for the open range prevailed throughout the South. Animals were simply branded or clipped and turned loose to graze the land—anybody's land, for fencing laws prohibited the enclosure of any space not actually under cultivation and required farmers to

²² Olmsted, *Cotton Kingdom*, 304-05.

²³ *Ibid.* On the simplicity of Southern architecture, see James C. Bonner, "Plantation Architecture of the Lower South on the Eve of the Civil War," *Journal of Southern History*, 11 (1945): 370-88; Fred B. Kniffen and Henry H. Glassie, "Building in Wood in the Eastern United States: A Time-Place Perspective," *Geographical Review*, 56 (1966): 40-66; Fred B. Kniffen, *Folk Houses of Louisiana* (Baton Rouge, 1942); Henry Glassie, *Folk Housing in Middle Virginia: Structural Analysis of Historic Artifacts* (Knoxville, 1975); Eugene M. Wilson, *Alabama Folk Houses* (Montgomery, Ala., 1975); and Terry G. Jordan, *Texas Log Buildings: A Folk Architecture* (Austin, 1978).

fence their crops against the animals of others, rather than the other way around.²⁴ When the larder got low, the plain folk stuck another hog. For vegetables, almost no tillage was necessary, since green gardens in the Southern soil and climate, once planted, grew wild, reseeding themselves year after year if they were appropriately neglected, as was also the case with "pumpkins, sweet potatoes, and several other vegetables." In 1854 a startled German, who found the South far different from his own culture, wrote to friends back home:

There are such fine fruits and plants here. The forest is a veritable vineyard, for grapes of all kinds grow in the wild forest as well as such things as are planted in the fine gardens of Germany. All such things grow in the woods here. . . . You can also keep as many cows here as you wish, for feed does not cost a penny. Cattle feed itself in the woods in winter and summer, no cattle here is fed in the barn. Grass grows six to eight feet high in the woods and one person has as much right there as the other. Similarly you can keep as many pigs as you wish, and you need not feed them. The same is true of chickens. . . . We do not want to go on, for we can live here like lords.²⁵

Once a year—in the fall, after the livestock had fattened themselves on acorns and other nuts—herds were rounded up and driven to market as a cash crop. A few weeks of work in the spring, a few weeks in the fall, were all that was required to keep the system going.²⁶

The system made the South lavishly self-sufficient—lavishly, that is, by the plain folk's own preferred standards, which required only an abundance of leisure, tobacco, liquor, and food. That they had three of the four is attested by virtually every travel account. As for food, Sam B. Hilliard has estimated that antebellum Southerners consumed approximately 150 pounds of pork and 50 pounds of beef—and this was lean meat—per capita per year. That is one-third again as much animal protein per capita as was consumed by Americans in 1977. It figures out to 248 grams of *animal* protein per man, woman, and child per day—nearly five times the amount of *total* protein intake recommended for adult males by the Food and Nutrition Board of the National Research Council in 1978.²⁷

Nor was self-sufficiency all. The value of Southern livestock in 1860 was twice that of the year's cotton crop and approximately as much as the value of all Southern crops combined. At first the comparison may seem inappropriate, since only about one-fifth of the animals were slaughtered for market. Another

²⁴ Forrest McDonald and Grady McWhiney, "The Antebellum Southern Herdsman: A Reinterpretation," *Journal of Southern History*, 41 (1975): 147–66.

²⁵ *Visit to Texas: Being the Journal of a Traveller through Those Parts Most Interesting to American Settlers* (New York, 1834), 23–24; and [Joseph Eder] "A Bavarian's Journey to New Orleans and Nacogdoches in 1853–1854," ed. Karl J. R. Arndt, *Louisiana Historical Quarterly*, 23 (1940): 496–97. One of the authors of this essay (McDonald) has observed the phenomenon of vegetables reseeding themselves (or "escaping," as it is technically known) on his own farms in Florida and Alabama.

²⁶ For details on herding, see McDonald and McWhiney, "The Antebellum Southern Herdsman," 160–63 and the sources cited therein. In that article, we assumed that a large percentage of the plain folk were engaged in tillage farming of cotton or other commercial crops, an assumption we now question. Hilliard has made the same assumption; *Hog Meat and Hoeecake*, 151. But Wright's argument to the contrary seems conclusive; *The Political Economy of the Cotton South*, 55–56, including Table 3.4, n. 24.

²⁷ Hilliard, *Hog Meat and Hoeecake*, 105. For modern consumption, real and recommended, see *The World Almanac & Book of Facts*, 1979 (New York, 1979), 161.

three-fifths of the hogs, however, were slaughtered for home consumption, which means that the value of the annual swine "crop" was 80 percent of the total value. Moreover, virtually all of the gross sales of livestock was net profit, whereas the profit margin in crops was relatively slender and uncertain.²⁸

The drives of Southern livestock in enormous herds over long distances were such as to boggle the imagination. The French Broad River area of East Tennessee sent more than 150,000 hogs a year across the mountains into North and South Carolina; 80,000 or more passed through Cumberland Gap each fall; and another 100,000 or so came south annually along established routes from Kentucky and Tennessee to Georgia, Alabama, and Mississippi. The magnitude of the industry may be seen by comparing drives in the Old South with the more celebrated later drives during the heyday of the cowboy. Walter Prescott Webb's classic *The Great Plains* provides a table that shows the number of Texas cattle driven to market in each of the fifteen years from 1866 to 1880. The average annual number was about 280,000, the total during the period 4,223,497. By contrast, during the last fifteen years of the antebellum period, Southerners drove or otherwise marketed an average of 4,468,400 hogs per year; the total during the period was 67,026,000. The marketing of cattle in the antebellum South is more difficult to estimate, but in 1850 a federal agency reported that during the preceding twenty years one small area of the South—some two hundred miles square of piney woods in southern Mississippi, eastern Louisiana, and western Alabama—had raised for market one million cows annually.²⁹

One more important point needs to be made. The leisurely life style of the Southern plain folk was not a by-product of slavery, as many contemporary travelers thought, nor was it an adaptation made by Europeans to the peculiar climatological and geographical conditions of the region. Nor, for that matter, was it a product of a "frontier process." (The South was still frontier country in 1860; only a fraction of its land had been cleared in the two and a half centuries since the white man began to arrive.) Rather, the Southern way was a classical example of what some cultural geographers have called cultural preadaptation or preselection.³⁰ We have not yet completed our massive investigation of the ethnic origins of white Southerners, but our preliminary data indicate that upwards of 70 percent of those whose ethnic background can be ascertained were of Celtic extraction—mainly Welsh, Scots, Irish, and Scotch-Irish—or had originated in the "Celtic frontier," the extreme southwestern, western, and northern

²⁸ McDonald and McWhiney, "The Antebellum Southern Herdsman," 147; and Hilliard, *Hog Meat and Hoecake*, 129. Ransom and Sutch have estimated the slaughter rate of hogs at 80 percent; *One Kind of Freedom*, 151, 346–47 n. 12. Also see Richard Sutch, "The Treatment Received by American Slaves: A Critical Review of the Evidence Presented in *Time on the Cross*," *Explorations in Economic History*, 12 (1975): 369–70. Since the rate of slaughter for market was about 20 percent, it follows that about one-fourth of the hogs slaughtered were killed for market.

²⁹ Webb, *The Great Plains* (Boston, 1931), 223; Dodd and Dodd, *Historical Statistics of the South*, 2–61; and U.S. Patent Office, *Report of the Commissioner of Patents for the Year 1850*; part 2: *Agriculture* (Washington, 1851), 260.

³⁰ Acreage has been calculated from Dodd and Dodd, *Historical Statistics of the South*, 2–61. On cultural preadaptation, see Milton Newton, "Cultural Preadaptation and the Upland South," *Geoscience and Man*, 5 (1974): 143–54; Fred B. Kniffen, "Folk Housing: Key to Diffusion," *Annals of the Association of American Geographers*, 55 (1965): 549–77; and Kniffen and Glassie, "Building in Wood in the Eastern United States," 40–66.

parts of England.³¹ To be sure, the tidewater plantation country of the Atlantic seaboard was predominately English, but elsewhere in the South Anglo-Americans were distinctly in the minority; and there were various German,³² French,

³¹ For the "Celtic fringe," see J. G. A. Pocock, "British History: A Plea for a New Subject," *New Zealand Journal of History*, 8 (1974): 6. Pocock has not included the English border country as part of the "Celtic fringe," but others have. On the distinctiveness and Celticness of the English North Country, see Denis Hay, "England, Scotland, and Europe: The Problem of the Frontier," *Transactions of the Royal Historical Society*, 25 (1975): 77-91; on that of the southwest, see A. L. Rowse, ed., *The West in English History* (London, 1949); and, on that of the Welsh border, see Howell T. Evans, *Welsh Drovers: Wales and Monmouthshire* (Cardiff, 1938); and H. R. Ranking, "Cattle Drovers from Wales to England," *Journal of the Ministry of Agriculture*, 62 (1955): 218-21. In using the term "Celtic," we do not mean to suggest a common genetic pool, for the peoples under discussion were clearly of different genetic mixtures. The Welsh are obviously of different genetic stock from the Irish, for instance, and Highland Scots had different bloodlines from Lowlanders. Rather, we are speaking of peoples who shared a common cultural heritage—customary lifestyles, attitudes, and ways of doing things. Even in that sense, of course, the various peoples we treat as Celtic were far from identical. But after a great deal of study we have concluded that it is legitimate to consider them as a single general cultural group, different from the English—much in the same way that Western culture is seen as distinct from Islamic culture, while recognizing that Italians and Swedes differ from one another even as do Libyans and Turks. A more accurate phraseology than Celtic, in the sense we are using the term, would be "people from the British Isles who were historically and culturally non-English"—but somehow that phrase seems less catchy. In a previous article, following the "Report of the Committee on Linguistic and National Stocks in the Population of the United States," which appeared in the *Annual Report of the American Historical Association* in 1931, we reported that Scots and Scotch-Irishmen "constituted just under a fourth of the total white population of the South" in 1790; "The Antebellum Southern Herdsman," 156 n. Since writing that article, we have uncovered a number of serious methodological errors in the 1931 report, and, as a consequence, we have re-examined the entire subject. It turns out that, in 1790, persons of Celtic extraction ranged from 38.7 percent of the white population in Maryland to 53.4 percent in South Carolina, as against 47.4 percent English in Maryland and less than 37 percent English in South Carolina. In the Southern uplands, from which most nineteenth-century inhabitants of the interior South derived, Celts in 1790 constituted as much as 90 percent of the population. By contrast, in New England in 1790 between 70 and 80 percent of the population was English. These data and the analysis on which they are based are reported more fully in Forrest McDonald and Ellen Shapiro McDonald, "The Ethnic Origins of the American People, 1790," *William and Mary Quarterly*, 3d ser., 37 (1980): 179-99. For the past two years, we have also been engaged in a large-scale name analysis of the entire antebellum South; our tentative figure of upwards of 70 percent is derived from this study. Our calculations indicate that there were some Scotch-Irish in upper New England; but open-range husbandry was not available to them. Even in the seventeenth century, common grazing rights were restricted to original town proprietors and their heirs or assigns; and, by the time Scotch-Irishmen arrived, the commons had been substantially closed. See Percy Wells Bidwell and John I. Falconer, *History of Agriculture in the Northern United States, 1620-1860* (New York, 1941), 21-23, 56-58. Also see the Microfilm Collection of Town Records, New Hampshire State Library, Concord, N.H. Although the rural Virginia "oot" for "out" is clearly of Scottish origin, some studies of southwestern Virginia dialects would seem to contradict our findings, since they indicate that those Virginians (as well as rural folk in parts of Tennessee, Georgia, Alabama, and Mississippi) continued to speak with a seventeenth-century English accent well into the twentieth century. The "English" accent, however, is that of the West Country—Celtic Cornwall and the "Celtic frontier" shires of Devon, Somerset, Dorset, and Wilts. See Bennett Wood Green, *Word Book of Virginia Folk Speech* (Richmond, 1912); Cleanth Brooks, *The Relation of the Alabama-Georgia Dialect to the Provincial Dialects of Great Britain* (Baton Rouge, 1935); and John Eacott Manahan, "The Cavalier Remounted: A Study of the Origins of Virginia's Population, 1607-1700" (Ph.D. dissertation, University of Virginia, 1946), 182-85, Appendix 17.

³² Germans in the colonial South tended to live in separate enclaves, and, being industrious farmers, they cleared the land by cutting down trees, burning them, and pulling "out the stumps by the roots." This practice was in marked contrast to the "shiftlessness of the Scotch-Irish," who girdled trees, let them bleed to death and fall or be blown down, and simply planted around the stumps. Carl Bridenbaugh, *Myths and Realities: Societies of the Colonial South* (Baton Rouge, 1952), 135, 140-41. In comparison to the Celts, Germans operated farms with "greater intensity, productivity, and locational stability." German farmers in Texas were "more active in market gardening" and "in supplying vegetables and fruit," and "they devoted more attention to wine-making and the production of white potatoes. In both western and eastern settlements, the Germans showed greater interest than southerners in cultivation of small grains. . . . By the same token, the Germans . . . operated on a higher level of commercialization than the Americans before the Civil War." In 1850, the average German farm in eastern Texas was half the size and contained livestock valued at less than a third as much as that of the average non-German farm. Although Germans frequently adopted Southern ways, "the open range was an entirely new economic undertaking for the Germans, alien to their European agricultural heritage." Terry G. Jordan, *German Seed in Texas Soil: Immigrant Farmers in Nineteenth-Century Texas* (Austin, 1966),

and Hispanic³³ settlements in the region, but these groups either lived in isolated enclaves or were absorbed into the dominant Celto-American culture. By contrast, the Northern United States, except for the recently arrived Irish and for the Scotch-Irish in western Pennsylvania and in "Copperhead country" along the Ohio River, were peopled mainly by English and Germans.

As J. G. A. Pocock has put it, the Celtic peoples are "no more English than Britain is European." They and Englishmen on the Celtic frontier shared many cultural characteristics, the most relevant for present purposes being a long-standing tradition of open-range pastoralism and an accompanying disdain for tillage agriculture, especially for labor-intensive cultivation. As early as 1185 Gerald of Wales reported, after a tour of Ireland, that "the pastoral way of life, the tending of herds, did not require the same physical toil involved in arable farming, and therefore the Irish were thought to be indolent." Gerald found essentially the same to be true of Wales: "It must always be borne in mind that

192-94, 98, 100, 85. Germans were almost the only people in the South whom Frederick Law Olmsted understood and admired; their neat houses and diligent merchants, mechanics, and farmers reminded him of New England. After enjoying a German inn, Olmsted remarked, "Nothing so pleasant as that in Texas before, hardly in the South." Olmsted, *A Journey through Texas* (reprint ed., New York, 1969), 140-47, 149, 167, 178-90, 235-37.

³³ Livestock laws and herding practices in Louisiana under the French and Spanish were quite different from those of the Celtic South. Apparently the early settlers of Louisiana were unskilled as herdsmen; in 1713, the colony was forced to import "pigs or salt bacon" in "sufficient quantity to last the garrison for six months." Tenants on Bienville's lands were given "provisions for one year, a cow in calf, two hogs, four chickens with a cock, and the necessary utensils and agricultural implements. . . . The cow must be returned within three years, and of all the cattle raised in excess of the first twelve head, Bienville was to receive half. For the two hogs furnished, he demanded a fat hog every second year, and for the four chickens and the cock six fat hens or capons were demanded every year." This demand for fat hogs and chickens suggests that the animals were penned up. In any event, hogs seem not to have been plentiful in colonial Louisiana. An estate recorded in 1745 listed "a number of horned cattle, oxen, cows, bulls and heifers," but no hogs; another estate recorded in 1761 enumerated 348 animals of various kinds, but not a single hog. There was no open range in Louisiana before it became part of the United States. Testimony in 1749 reveals that shepherds controlled the daily movement of both sheep and hogs. A contract "for the purpose of raising livestock and poultry" in 1763 specified the building of barns for hogs and sheep and the enclosing of land, the fence for which the "two contracting parties" would put up "at joint expense" in order "to prevent the cattle from straying off." A case of cattle damaging crops brought before the Superior Council of Louisiana in 1763 shows that animals were required by law to be fenced into enclosures throughout most of the year. A letter to the Council of West Florida complains that a buyer of land had forfeited his right to it because he failed to put "Cattle thereon" and to build "inclosures." And, a decree of 1795 states, "No live stock is permitted to run at large, between the 15th March & the 15 November, and every Inhabitant is at liberty to kill . . . the Domestic Animals that shall trespass upon the Crops." Lauren C. Post, "The Domestic Animals and Plants of French Louisiana as Mentioned in the Literature with Reference to Sources, Varieties, and Uses," *Louisiana Historical Quarterly*, 16 (1933): 565; Albert Godfred Sanders, "Documents Concerning the Crozat Regime in Louisiana, 1712-1717," *ibid.*, 307; Kathryn C. Briede, "A History of the City of Lafayette," *ibid.*, 20 (1937): 900-01; Heloise H. Cruzat *et al.*, eds., "Records of the Superior Council of Louisiana," June-July 1745, March-April 1762, January-December 1749, October 1763, and January-March 1763, *ibid.*, 14 (1931): 246, 23 (1940): 942-44, 20 (1937): 488, 26 (1943): 205-06, and 24 (1941): 819-20; and James A. Padgett, ed., "Minutes of the Council of West Florida, April 3-July 22, 1769," *ibid.*, 23 (1940): 374, and "A Decree for Louisiana Issued by the Baron of Carondelet, June 1, 1795," *ibid.*, 20 (1937): 597. Methods of animal husbandry changed completely after Louisiana became a state. The Police Jury in parish after parish altered the laws and legalized the open range. In 1813, for example, St. Helena Parish not only allowed animals to run at large but required "Each inhabitant or planter" to construct a stout fence five and a half feet high around his crops to protect them from the hogs and cattle that freely roamed the parish. The open-range system in Sabine Parish soon produced so many hogs "that the owner who had failed to mark his hogs was frequently unable to identify them," and each year men went out to hunt hogs "with dogs and guns." Robert Dabney Calhoun, "The Origin and Early Development of County-Parish Government in Louisiana (1805-1845)," *Louisiana Historical Quarterly*, 18 (1935): 93, 120; Walter Prichard, ed., "Minutes of the Police Jury of St. Helena Parish, August 16-19, 1813," *ibid.*, 23 (1940): 416-17; and John B. Belisle, *History of Sabine Parish, Louisiana* (n.p., 1912), 97.

the Welsh are not being enervated by daily toil," he reported three years later; "the whole population lives almost entirely on . . . the produce of their herds. . . . They eat plenty of meat, but little bread. They pay no attention to commerce, shipping or industry."³⁴

This leisurely pastoral way of life perdured and remained characteristic of all the Celtic portions of the British Isles. The Exchequer Rolls for 1378 show that nearly 45,000 hides were exported from Scotland that year, indicating that animal husbandry was the basis of the kingdom's economy. Estienne Perlin, a mid-sixteenth-century traveler to Scotland, spoke of the abundance of cattle (a term that comprehends sheep, horses, and swine as well as bovine animals). Late in the century, according to A. R. B. Haldane in his monumental study of droving in Scotland, Bishop Leslie noted the huge herds of every manner of livestock that, "through a certane wyldnes of nature," foraged the open range "lyke wylde hartes, wandiring out of ordour." And, despite increased Anglicization of the Lowlands after the Act of Union in 1707, something resembling open-range animal husbandry remained the way of life for most of the Scottish plain folk well into the eighteenth century; indeed, as Haldane has made clear, one principal effect of the Act of Union was to open southeastern English markets for long trail drives of Scottish animals. As for the Scottish plain folk themselves, Thomas Pennant described them pithily in 1769; they were, he said, "indolent to a high degree, unless roused to war, or to any animating amusement."³⁵

The Celts had sometimes thrived, sometimes been impoverished; but they were amazingly resistant to changing their ways, and particularly to making the painful wrench from being animal-raisers to being mere tillers of the soil. The Irish, for instance, held crop-growing in such contempt that, despite centuries of oppressive English landlordism, it was not until the 1780s that planting began to be common in Ireland—and then with the potato, a crop whose labor requirements are minimal. Thomas Croker's description of the Irish peasants, written in 1824, strikingly resembles Olmsted's descriptions of Southern Americans. "The cabins of the peasantry," Croker recorded, "teem[ed] with a numerous population of children, pigs, and poultry." Living conditions, he continued, were appalling; "an adequate idea of the wretchedness of the habitations can scarcely be formed from description. . . . The floor is bare earth, so uneven that

³⁴ Pocock, "British History: A Plea for a New Subject," 7; Brian de Breffny, ed., *The Irish World: The Art and Culture of the Irish People* (New York, 1977), 86; and Gerald of Wales, *The Journey through Wales and the Description of Wales, 1188* (New York, 1978), 270, 233. To appreciate the differences between Ireland and Wales on the one hand and England on the other, see Warren O. Ault, *Open-Field Farming in Medieval England* (London, 1972), *passim*. For the best works on the Celts, see Anne Ross, *Everyday Life of the Pagan Celts* (London, 1970); T. G. E. Powell, *The Celts* (New York, 1958); Gerhard Herm, *Celts: The People Who Came out of the Darkness* (New York, 1977); Alwyn Rees and Brinley Rees, *Celtic Heritage: Ancient Tradition in Ireland and Wales* (London, 1961); Thomas Crofton Croker, *Researches in the South of Ireland . . .* (New York, 1969); Archibald R. B. Haldane, *The Drove Roads of Scotland* (London, 1952); Thomas Pennant, *A Tour in Scotland, 1769* (5th ed., London, 1790); L. M. Cullen, *An Economic History of Ireland since 1660* (New York, 1972); and K. H. Connell, *Irish Peasant Society* (New York, 1968). Also see Henry Jones Ford, *The Scotch-Irish in America* (Princeton, 1915), 1–208; Ian C. C. Graham, *Colonists from Scotland: Emigration to North America, 1707–1783* (Ithaca, N.Y., 1956); and James G. Leyburn, *The Scotch-Irish: A Social History* (Chapel Hill, 1962), including the bibliography, 354–61.

³⁵ Haldane, *Drove Roads of Scotland*, 7, *passim*; Peter Hume Brown, *Early Travellers in Scotland* (Edinburgh, 1891), 74; and Pennant, *Tour through Scotland*, 214.

the four legs of a chair are seldom of use at one time, and baskets and utensils lie around in an indiscriminate litter; a pig, the wealth of an Irish peasant, roams about with conscious importance." Pigs, indeed, were everywhere; "the Irish possessed abundance and were insatiably fond" of pork. But the ordinary Irishman "has no idea of planting a bush, or fruit tree, or of raising a flower; and his ignorance renders him too stubborn to make trial of improved agricultural implements."³⁶

When the culturally preadapted Celtic peoples migrated to the Southern uplands of British North America—which they did on a grand scale during the hundred years after 1715—they found geographical and political conditions ideal for the flourishing of their style of life. In sum, the opulently easy society of the Southern plain folk on the eve of the Civil War represented the culmination of many centuries of Celtic traditions. And these same Celtic traditions might explain why so many slaveowners did not push their slaves to work harder: maximization of profits and of one's labor supply was alien to the culture and, in fact, had never been common in the Celtic areas of the British Isles.

NOW LET US SHIFT TO THE POSTBELLUM SOUTH. If antebellum observers were agreed that Southerners were leisurely or lazy, postbellum observers were agreed that most Southerners lived in wretched misery. "After the war," a freedman recalled, "Old Man Gordon took me and some of the others out to Mississippi. I stayed in peonage out there for 'bout forty years." Peonage, however, was not confined to blacks: tenancy and sharecropping reduced most white farmers to a system of virtual peonage as well. "Whilst houses, fences, and everything have gone and are going to ruin and decay," noted a late nineteenth-century observer, "the poor farmer can only get advances to make cotton. [There are] no fences, no hogs, no cattle, no agriculture, no nothing. Bald, barren, uncultivated, and washed spots are seen everywhere. . . . The products of the soil . . . do not sustain and support the population. . . . Not one in a hundred makes a crop now without mortgaging for his year's support and supplies."³⁷

Moreover, things grew steadily worse, decade by dreary decade. The lucky ones were those who escaped to the mills where they worked long hours for a pittance. "Soon as the mill opened," recalled one man, "my father and all the younguns that was old enough commenced to work. I got ten cents a day for working from six o'clock in the morning to five minutes until seven o'clock in the evening." Only a few were that lucky: burdened by debts, tenants were essentially fixed to the soil. When they left, it was at the landowner's bidding, not their own. Here is a woman describing her childhood: "All of our folks before us was tenant farmers and that's all we've ever done. If you know anything about tenant farming you know they do without everything all the year hoping to

³⁶ Breffny, *The Irish World*, 182–83; and Croker, *Researches in the South of Ireland*, 61, 128–29, 269, 103.

³⁷ B. A. Botkin, ed., *Lay My Burden Down: A Folk History of Slavery* (Chicago, 1945), 248; and Ethel M. Armes, *The Story of Coal and Iron in Alabama* (Birmingham, Ala., 1910), 269–70.



Figure 3: Cotton sharecropping family after the turn of the century.
Photograph reproduced courtesy of the Farm Bureau, U.S. Department of Agriculture.

have something in the fall. Well, it's very little they ever have."³⁸ And James Agee, in 1936, described the circumstances of an Alabama farmer he called George Gudger:

Gudger has no home, no land, no mule; none of the more important farming implements. He must get all these of his landlord, [who,] for his share of the corn and cotton, also advances him rations money during four months of the year, March through June, and his fertilizer.

Gudger pays him back with his labor and with the labor of his family.

At the end of the season he pays him back further: with half his corn; with half his cotton; with half his cottonseed. Out of his own half of these crops he also pays him back the rations money, plus interest, and his share of the fertilizer, plus interest, and such other debts, plus interest, as he may have incurred.

What is left, once doctors' bills and other debts have been deducted, is his year's earnings.

Gudger is a straight half-cropper, or sharecropper.

Very few tenants, Agee explained, paid cash rent. Most followed some variant of sharecropping, with its characteristic life cycle:

From March through June, while the cotton is being cultivated, they live on the rations money.

From July through to late August, while the cotton is making, they live however they can.

From late August through October or into November, during the picking and ginning season, they live on the money from their share of the cotton seed.

³⁸ Federal Writers' Project, *These Are Our Lives* (New York, 1975), 181.



Figure 4: Interior of a typical house occupied by postbellum sharecroppers and tenants. Photograph reproduced courtesy of the Farm Bureau, U.S. Department of Agriculture.

From then on until March, they live on whatever they have earned in the year; or however they can.

During six to seven months of each year, then—that is, during exactly such time as their labor with the cotton is of absolute necessity to the landlord—they can be sure of whatever living is possible in rations advances and in cottonseed money.

During five to six months of the year, of which three are the hardest months of any year, with the worst of weather, the least adequacy of shelter, the worst and least of food, the worst of health, quite normal and inevitable, they can count on nothing except they may hope least of all for any help from their landlords.³⁹

The decline into peonage can be shown with figures. During the late antebellum period, perhaps 80 percent or more of the farms in the Lower South were operated by owners. During the postbellum period this figure declined steadily until, in 1930, more than one million white families and nearly seven hundred thousand black families were tenants. In that year only 37 percent of Southern farms were fully owned by their operators, and most of those were heavily mortgaged. In 1910 the number of whites who “owned” more than five tenant families, which would be more than twenty people, was nearly the same as the number who had owned twenty or more slaves in 1860. In the Deep South peonage was even more obvious. Farms operated by owners in Alabama and Mississippi declined from 54 percent in 1880 to 27 percent in 1930.⁴⁰

³⁹ Agee and Walker Evans, *Let Us Now Praise Famous Men* (New York, 1966), 105–06.

⁴⁰ Owsley, *Plain Folk of the Old South*, 9; Herbert Weaver, *Mississippi Farmers, 1850–1960* (Nashville, Tenn., 1945), 63–84; Fairfax Publishers, *The Statistical History of the United States* (Stanford, Conn., 1965), 278; and U.S. Census Bureau, *Report on the Production of Agriculture . . . Tenth Census, 1880* (Washington, 1883), 64, 100, and *Fifteenth Census of the United States, 1930: Agriculture*, volume 2, part 2: *The Southern States* (Washington, 1932), 972, 1050.

What happened can be seen by an analysis of six counties: Greene and Pickens in Alabama, and Noxubee, Monroe, Tishomingo, and Itawamba in Mississippi. In the antebellum period and throughout the postbellum period Greene and Noxubee counties were populated overwhelmingly by blacks. The average white population for one hundred years was less than 20 percent of the total. On the other extreme, Tishomingo and Itawamba counties averaged white populations of more than 89 percent for the same period. The population of Pickens and Monroe counties has been mixed, averaging slightly less than 50 percent white.⁴¹ Each of the counties in each of these categories—black, white, and mixed—had been self-sufficient before the Civil War, and each experienced a systematic decline in production and in the ability of its citizens to feed themselves adequately in the postbellum period. Before the Civil War the people in these counties owned more than 2.1 hogs per person and raised an average of 48.5 bushels of corn per person; by 1930 the figures had declined to 0.4 of a hog per person and 22.8 bushels of corn per person. In other words, per capital hog production fell by 80 percent, and per capita corn production by more than 50 percent. From being net exporters of food these counties became importers of food.⁴²

Meanwhile, the farms in these counties were getting smaller and cotton production was declining drastically. The average acreage per farm in the black counties decreased from 650 to 53, in the predominately white counties from 328 to 70, and in the mixed counties from 429 to 67. And cotton production fell from 83,174,800 pounds in 1860 to 51,023,000 in 1930. Per capita cotton production decreased from 607 pounds per person in 1860 to 362 pounds per person in 1930, when 71 percent of the farms in these counties were operated by tenants.⁴³ Obviously, more and more people were working harder and harder to scratch out a living of an ever declining quality. By 1944, according to the Department of Agriculture, the gross production of each Southern farm worker was only 56 percent of the national average, and only 38 percent as much as the average Midwestern and Western farm worker produced. Moreover, the average Southern farm worker cultivated only 15.4 acres of crops compared to the national average of 37.4 acres per farm worker.⁴⁴

⁴¹ U.S. Census Bureau, *Seventh Census of the United States, 1850: An Appendix*, 421, 447, *Population of the United States in 1860, Compiled from the Original Returns of the Eighth Census* (Washington, 1864), 8, 270, *Report on the Population of the United States at the Eleventh Census, 1890*, pt. 1 (Washington, 1895): 416, 417–18, *Negro Population, 1790–1915* (Washington, 1918), 642, 656–57, *Population by Counties and Minor Civil Divisions, 1910, 1900, 1890* (Washington, 1912), 9, 13–14, 276, 278, 279, *Negroes in the United States, 1920–1932* (Washington, 1935), 685, 687, 758, 759, 760, *Fifteenth Census of the United States, 1930: Population*, 3, pt. 1 (Washington, 1932): 94, 96, 1312, 1313, 1315, *U.S. Census of Population, 1960: Number of Inhabitants, Alabama* (Washington, 1961), 63, 65, and *Census of Population, 1960, volume 1: Characteristics of the Population*, part 26: *Mississippi* (Washington, 1961), 90, 91, 93.

⁴² U.S. Census Office, *Statistical View of the United States . . . Compendium of the Seventh Census . . . , 1850* (Washington, 1850), 194, 196, 197, 260, 262, 263, *Population of The United States in 1860*, 8, 270, *Agriculture of the United States in 1860, Compiled from the Original Returns of the Eighth Census* (Washington, 1864), 3, 85, and *Fifteenth Census of the United States, 1930: Agriculture*, 1001, 1003, 1007, 1009, 1078, 1079, 1080, 1081, 1085, 1087, 1089.

⁴³ U.S. Census Office, *Population of the United States in 1860*, 8, 270, *Agriculture of the United States in 1860, Compiled from the Original Returns of the Eighth Census* (Washington, 1864), 2, 84, 193, 206, 3, 85, *Fifteenth Census of the United States, 1930: Population*, 94, 96, 1312, 1313, 1315, and *Fifteenth Census of the United States, 1930: Agriculture*, 972–77, 1007, 1009, 1050–55, 1085, 1087, 1089.

⁴⁴ Department of Agriculture, *Progress of Farm Mechanization*, 12.

"How did we get caught?" cried James Agee in an anguished passage. "Why is it things always seem to go against us? Why is it there can't ever be any pleasure in living? I'm so tired it don't seem like I could ever get rest enough. . . . In what way were we trapped?"⁴⁵

LET US ATTEMPT TO EXPLAIN how they got "trapped." Three months after Appomattox, an influential journal proposed that Northerners colonize and Yankeeize the South—"in short, . . . turn the slothful, shiftless Southern world upside down." What actually happened to Southerners is what had happened to their Celtic ancestors when the English subdued them—that is, the ultimate outcome of the Civil War was like that of wars of feudal conquest, resulting in making the great landlord class total masters inside the South and in making them vassals to overlords outside the region. In specific terms, the deposed and discredited planter class continued after the war to own a disproportionate share of the land: in 1870 the wealthiest 5 percent owned over two-fifths of the land and the wealthiest 20 percent owned nearly three-quarters of the land. Soon after the war, the former slaves more or less forced their former masters to adopt the sharecropping system. The freedmen were thus trapped very early: to them, as to Janis Joplin's Bobby McGee, freedom became "just another word for nothing left to lose."⁴⁶

The entrapment of the white plain folk was a longer and more complex process. The first step was the large-scale destruction of their capital in the form of livestock as a direct or indirect consequence of the war. Advancing Union and retreating Confederate forces stole or impressed nearly everything edible in their paths; and, in the turbulence during the years following Appomattox, roving bands of freedmen wantonly stole and slaughtered many more animals. Only in areas where there had been relatively little fighting or relatively few slaves were there increases in the number of hogs between 1860 and 1880. In eight of the former slave states the hog population declined by a total of 2.6 million; in the "Five Cotton States" isolated by Ransom and Sutch, per capita ownership of swine fell from 1.64 in 1860 to 0.88 in 1880. Damaging as this was to the plain folks' traditional way of life, however, it need not have been fatal, and it was not: perhaps two-thirds of them still owned their own land in 1880.⁴⁷

⁴⁵ Agee and Evans, *Let Us Now Praise Famous Men*, 74–75.

⁴⁶ *Nation*, 1 (1865): 67; Ransom and Sutch have provided comparative data on the concentration of land-ownership for 1860 and 1870; *One Kind of Freedom*, 78–80. Also see Jonathan M. Wiener, "Planter Persistence and Social Change: Alabama, 1850–1950," *Journal of Interdisciplinary History*, 7 (1976): 235–60. The lyric to Bobby McGee was composed by Kris Kristofferson, Columbia KC 30322. The economic history of the post-bellum South has been studied by a number of cliometricians besides Ransom and Sutch, including Robert Higgs, Stephen J. DeCanio, and Joseph D. Reid, Jr. For an excellent summary and critique of these efforts, see Harold D. Woodman, "Sequel to Slavery: The New History Views the Postbellum South," *Journal of Southern History*, 43 (1977): 523–44. None of the works of these scholars takes into account the animal-raising tradition in the antebellum South.

⁴⁷ Grady McWhiney, "The Revolution in Nineteenth-Century Alabama Agriculture," *Alabama Review*, 31 (1978): 21–24 and the sources cited therein; McDonald and McWhiney, "The Antebellum Southern Herdsman," 163–65 and the sources cited therein; and Ransom and Sutch, *One Kind of Freedom*, 152, Table 8.1. Land ownership is difficult to ascertain because the census does not tabulate farms by race and form of tenure. Ac-

But the trap was already being fashioned. Part of it lay in the undermining of their market as a by-product of the new relationships between the great landlords and their tenants and between the landlords and their own overlords. It was in the interest of landlords to discourage tenants from raising foodstuffs for their own consumption, partly because such production was unprofitable to the landlords and partly because in their new capacity as storekeepers, the landlords had an interest in selling food to the croppers. The landlord-storekeeper might have acquired his foodstuffs, or at least his meat, from the plain folk, except that his dependence upon Northern credit made him beholden to his outside suppliers. Given that dependent relationship, given the rise of the corn-hog-feeder-lot industry on the prairie plains (with its fatter, lower-priced, and less nutritious pork), and given the rise of the railroads (with their inherently cheaper rates for long hauls), the landlord-storekeeper found it more advantageous to import foodstuffs than to buy them locally.⁴⁸

The other part of the trap lay in undermining the plain folks' capacity to produce and transport their stock. The method was complex in detail but simple in principle: the fencing laws were changed to end the open-range system. There is evidence that the great planters had long sought to abolish the open range but, before the war, had lacked the political clout. After the war they gained the power to do what they wished, partly through the votes of their black sharecroppers and partly through the corrupt influence of their well-heeled Northern connections. The history of the land laws has not been studied in every state, but Crawford King has made a careful study of the laws in Alabama and has sampled enough of those elsewhere to conclude tentatively that Alabama was more or less typical of the entire South. Beginning in 1866, Alabama adopted a succession of local option laws, whereby a majority of voters in a county could end the open range. Unsurprisingly, black belt counties closed the range first, but the movement steadily spread until, by 1890, half of the counties in the state had eliminated the open range. In time it would be abolished entirely.⁴⁹

That was devastating to the herders for two essentially unrelated reasons. The first is that it seriously impaired the plain folks' traditional "transportation" system, the trail drive. The state became checkerboarded with closed-range areas; long drives became progressively more difficult and finally impossible. Meanwhile, the economics of running a railroad, which made interstate shipment cheaper than shipment inside the state, effectively closed the new method of transportation to Alabama herdsmen.⁵⁰

cording to the *Statistical History*, some 64 percent of all Southern farms were operated by owners or part-owners; *Statistical History of the United States*, 278. In the census of 1900, the first year in which tenure is tabulated by race, 36 percent of the whites and 75 percent of the blacks were tenants. Extrapolating backward, it would seem that as many as three-quarters of the whites still owned their own farms in 1880. Ransom and Sutch, however, using a sample of eleven thousand farms, have calculated that 41 percent of the white small family farms (fifty acres or less in crops) in 1880 were operated by tenants; *One Kind of Freedom*, 84, 283-91.

⁴⁸ For a good description of this part of the "trap of debt-peonage," see Ransom and Sutch, *One Kind of Freedom*, 156-70. On the effects of the corn-hog-feeder-lot industry and the railroads, see McWhiney, "The Revolution in Nineteenth-Century Alabama Agriculture," 24-29.

⁴⁹ King, "Alabama Fencing and Stock Laws: The End of the Open Range," paper read at the Annual Meeting of the Alabama Historical Association, held in Troy, 1979.

⁵⁰ McWhiney, "The Revolution in Nineteenth-Century Alabama Agriculture," 29-30.

The second reason has to do with the nature of the beast. The pig is "thermodynamically ill-adapted" to hot climates; he "is primarily, a creature of forests and shaded riverbanks." He could thrive in the Southern uplands or in the lowland canebrakes as long as he could run free, but he could not survive if penned up. According to the Agricultural Research Council Institute of Animal Physiology at Cambridge, England, "adult pigs will die if exposed to direct sunlight and air temperatures over 98°" Fahrenheit.

To compensate for its lack of protective hair and its inability to sweat, the pig must dampen its skin with external moisture. It prefers to do this by wallowing in fresh clean mud, but it will cover its skin with its own urine and feces if nothing else is available. Below 84° F., pigs kept in pens deposit their excreta away from their sleeping and feeding areas, while above 84° F. they begin to excrete indiscriminately throughout the pen. The higher the temperature, the "dirtier" they become.⁵¹

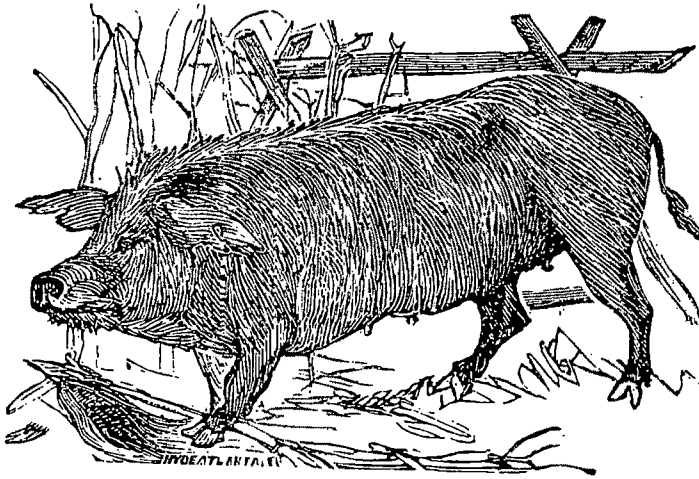


Figure 5: Line drawing of a "common Georgia hog: low-grade sow." Reproduced from Thomas P. Janes, *A Manual on the Hog* (Atlanta, 1977), 31.

In other words, to pen up Southern range hogs was to condemn them to filth and disease and, in due course, to veritable extinction. It was also to condemn the Southern people to disease: their new diet of fat, pen-fed pork and milled corn, imported from outside the South, contributed directly to the spread of pellagra and other nutrition deficiency diseases.⁵² What was more, to deprive the plain folk of their pigs was to condemn them to take up a life—as commercial plow-farmers—for which their entire heritage and traditional ways not only failed to prepare them but also effectively doomed them. Every year more and more of them became cultivators, in which capacity—given their habits, given their steadily deteriorating health, and given the postbellum marketing system,

⁵¹ Marvin Harris, *Cows, Pigs, Wars, and Witches* (New York, 1978), 34–36.

⁵² McWhiney, "The Revolution in Nineteenth-Century Alabama Agriculture," 27–29; Wilbur O. Atwater and Charles D. Woods, *Dietary Studies with Reference to the Food of the Negro in Alabama in 1895 and 1896* (Washington, 1897), 64–69; and Joseph Goldberger, "Pellagra," in Adelia M. Beeuwkes *et al.*, comps., *Essays on the History of Nutrition and Dietetics* (Chicago, 1967), 103–06. Atwater and Woods studied only blacks, but their observations were equally applicable to white sharecroppers as the twentieth century wore on.

especially that of cotton—they had scarcely a chance of succeeding. As they failed, they went into debt; when they went into debt, they lost their lands; when they lost their lands, their only recourse was labor in the textile mills or the lumber camps, or—what was the lot of most—the peonage of the tenant and cropping system.

And thus the gigantic trap slowly, steadily, inexorably closed upon them, until almost no one in the South remained free.

How Different from Each Other Were the Antebellum North and South?

EDWARD PESSEN

HOW DIFFERENT FROM EACH OTHER were the North and South before the Civil War? Recent work by historians of antebellum America throws interesting new light on this old question. Since some of these studies deal with individual communities,¹ others with single themes of antebellum life, they are in a sense Pirandelloan pieces of evidence in search of an overarching synthesis that will relate them to one another and to earlier findings and interpretations. My modest hope is that the discussion that follows will be useful to historians in pursuit of such a synthesis.

The terms "North" and "South" are, of course, figures of speech that distort and oversimplify a complex reality, implying homogeneity in geographical sections that, in fact, were highly variegated. Each section embraced a variety of regions and communities that were dissimilar in climatic, topographical, demographic, and social characteristics. If, as Bennett H. Wall has written, "there never has been the 'one' South described by many historians,"² neither has there been the one North. Historians who have compared the antebellum South and North without referring to the diversity of each have not necessarily been un-

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¹I first seriously thought of comparing the North and South after reading three papers by young historians on which I was asked to comment at the meeting of the Southern Historical Association held in Atlanta in November 1976. Whitman H. Ridgway's "The Decline of the Post-Revolutionary Establishment: Maryland Community Elites in the First Party Era," J. Mills Thornton III's "The Growth of Elitism in Alabama Politics, 1840-1860," and Donald De Baas's "Political Elitism in Antebellum Georgia" revealed striking similarities between politics in these Southern communities and in the Northern communities that I had been examining.

²Wall, "An Epitaph for Slavery," *Louisiana History*, 16 (1975): 233. Useful recent statements on Southern diversity include D. W. Meinig, "The Continuous Shaping of America: A Prospectus for Geographers and Historians," *AHR*, 83 (1978): 1195; Sam Bowers Hilliard, *Hog Meat and Hoeecake: Food Supply in the Old South, 1840-1860* (Carbondale, Ill., 1972); Ralph A. Wooster, *Politicians, Planters, and Plain Folk: Courthouse and Statehouse in the Upper South, 1850-1860* (Knoxville, 1975); Randolph B. Campbell and Richard G. Lowe, *Wealth and Power in Antebellum Texas* (College Station, Texas, 1977); and Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978).

aware of this diversity. Their premise, in speaking of the North and South, is that the Mason-Dixon line divided two distinctive civilizations, the basic similarities within each of which transcended its internal differences.

The modern discussion is a continuation of a scholarly controversy that has engaged some of the giants of the American historical profession. Charles A. Beard, Ulrich B. Phillips, Allan Nevins, David M. Potter, C. Vann Woodward,³ and other scholars of stature have been drawn to the theme because it is inextricably related to perhaps the most fascinating of all questions in American history: the causes of the Civil War.⁴ Many historians attribute that "irrepressible conflict" to the fundamental differences between the two civilizations that were parties to it.⁵ Even those scholars who have played down the role of sectional differences in bringing on the war have found themselves unable to avoid comparing the ways of life and thought of the two belligerents.⁶

Unsurprisingly, the discussion has produced a variety of interpretations. Some scholars have emphasized the similarities of the North and South, a much greater number have stressed their dissimilarities, and others have judiciously alluded to their significant likenesses—"commonalities," in Potter's terminology—and unlikenesses.⁷ The greater popularity, among scholars and laity alike, of

³ Beard, *The Rise of American Civilization* (New York, 1927); Phillips, *Life and Labor in the Old South* (Boston, 1929); Nevins, *Ordeal of the Union*, 6 vols. (New York, 1947), esp. vol. 2: *A House Dividing, 1852-1857*; Potter, *The South and the Sectional Conflict* (Baton Rouge, 1968), and *The Impending Crisis, 1848-1861*, ed. Don E. Fehrenbacher (New York, 1976); and C. Vann Woodward, "The Irony of Southern History," *Journal of Southern History* [hereafter, *JSH*], 19 (1953): 3-19, and *American Counterpoint: Slavery and Racism in the North-South Dialogue* (Boston, 1971).

⁴ Book-length discussions of the causes of the Civil War include Edwin C. Rozwenc, ed., *The Causes of the American Civil War* (Lexington, Mass., 1972); Thomas J. Pressly, *Americans Interpret Their Civil War* (Princeton, 1954); Howard K. Beale, *What Historians Have Said about the Causes of the Civil War*, Theory and Practice in Historical Study, Social Sciences Research Council (New York, 1946); and Lee Benson, "Explanations of American Civil War Causation: A Critical Assessment and a Modest Proposal to Reorient and Reorganize the Social Sciences," in his *Toward the Scientific Study of History: Selected Essays* (Philadelphia, 1972), 223-333.

⁵ According to Allan Nevins, the sectional "schism in culture" created an atmosphere in which "emotions grew feverish, in which every episode became a crisis, every jar a shock"; *Ordeal of the Union*, 2: 554. In Eugene D. Genovese's version, the South's "special civilization built on the relationship of master to slave" was "at the root of the conflict with the North"; Genovese, *The Political Economy of Slavery: Studies in the Economy and the Society of the Slave South* (New York, 1965), 35. Carl N. Degler has recently written that, "because of those differences" between the sections, "which clustered around the existence of slavery in the South, eleven states broke away to form the Confederate States of America"; Degler, *Place over Time: The Continuity of Southern Distinctiveness* (Baton Rouge, 1977). Also see Arthur C. Cole, *The Irrepressible Conflict, 1850-1865* (New York, 1934); and Heywood Fleisig, "Slavery, the Supply of Agricultural Labor, and the Industrialization of the South," *Journal of Economic History* [hereafter, *JEH*], 36 (1976): 592. For my own thoughts concerning the relationship between sectional differences and the outbreak of the war, see pages 1148-49, below.

⁶ For critics of what David Potter described as the "school of historical thought" that "sees the [Civil War] as a clash of profoundly dissimilar cultures," whose people ostensibly "were at odds because they lived in different cultural worlds," see Potter, *The Impending Crisis, 1848-1861*, 30-31, and *The South and Sectional Conflict*, 76-78; James G. Randall, "The Blundering Generation," *Mississippi Valley Historical Review*, 27 (1940): 3-28; and Francis B. Simkins, *The Everlasting South* (Baton Rouge, 1963), 38.

⁷ Carl N. Degler's work contains interpretations that emphasize both differences and similarities between North and South; Degler, *The Other South: Southern Dissenters in the Nineteenth Century* (New York, 1974), and *Place over Time*. To the writings of Potter and Woodward, about which the same can be said, can be added James G. Randall and David Donald, *The Civil War and Reconstruction* (Boston, 1969); T. Harry Williams, *Romance and Realism in Southern Politics* (Athens, Ga., 1961); Charles G. Sellers, ed., *The Southerner as American* (Chapel Hill, 1960); and Grady McWhiney, *Southerners and Other Americans* (New York, 1973). A particularly strong, if not entirely persuasive, assertion of the essential similarity of the two sections over the entire course of American history is Howard Zinn, *The Southern Mystique* (New York, 1964), a work that is present-minded in the extreme. Thomas P. Govan has answered his title question pithily and bitingly in the negative; Govan, "Was the Old South Different?" *JSH*, 21 (1955): 447-55. Works that emphasize sectional differences include Nevins, *Ordeal of*

comparisons that emphasize differences is doubtless due, in part, to the fact that the war heightened our perceptions of those supposedly irreconcilable differences and, in part, to the fact that several dissimilarities were so striking, so unarguable, so obviously significant. While much of the scholarly controversy has concerned subtle sectional distinctions, whether in values, ideals, or other complex intangibles that might be read one way or the other, depending on the predilections of the interpreter, other disparities transcend subjectivity, based as they are on hard, quantifiable evidence.

Here were two sections containing roughly equal areas for human settlement. Yet on the eve of the Civil War the population of the North was more than 50 percent greater than that of the South. The most dramatic disparity concerned racial balance: roughly one-quarter of a million Northern blacks comprised slightly more than 1 percent of the Northern population; the more than four million blacks in the South constituted one-third of the Southern population. And almost 95 percent of Southern blacks were slaves. Although the value of agricultural products in the two sections was almost equal, Northern superiority in manufactures, railroad mileage, and commercial profits was overwhelming, far surpassing the Northern advantage in population. Similarly, Northern urban development outdistanced Southern, whether measured by the number of cities or by the size and proportions of the population within them.⁸ What did these and other, harder to measure, differences signify? To what extent were they balanced out by important sectional similarities? These are among the questions this essay will consider.

In comparing the great antebellum sections, it is useful to remember that all powerful, complex, and viable contemporaneous societies are likely to converge or be similar in some respects, dissimilar in others. It would be lovely were we able to estimate precisely the relative significance of the various criteria of comparison, the points at which similarities or differences become critical, and the nature of the balance between likenesses and unlikenesses that would justify appraising two societies as "essentially" different or similar. Alas, we cannot. A society or civilization is a complex Gestalt. The subtle reciprocity binding together

the Union; Phillips, *Life and Labor in the Old South*; Simkins, *The Everlasting South*; Charles S. Sydnor, *The Development of Southern Sectionalism* (Baton Rouge, 1948); Avery O. Craven, *The Growth of Southern Nationalism, 1848-1861* (Baton Rouge, 1953), and *The Coming of the Civil War* (2d ed., Chicago, 1957); Clement Eaton, *Freedom of Thought in the Old South* (Durham, 1940), and *The Civilization of the Old South: Writings of Clement Eaton*, ed. Albert D. Kirwan (Lexington, Ky., 1968); Frank E. Vandiver, ed., *The Idea of the South: Pursuit of a Central Theme* (Chicago, 1964); W. J. Cash, *The Mind of the South* (New York, 1941); William R. Taylor, *Cavalier and Yankee: The Old South and the American National Character* (New York, 1961); Forrest McDonald, "The Ethnic Factor in Alabama History: A Neglected Dimension," *Alabama Review*, 36 (1978): 256-65; Raimondi Luraghi, *The Rise and Fall of the Plantation South* (New York, 1978); Eugene D. Genovese, *The World the Slaveholders Made: Two Essays in Interpretation* (New York, 1969), and *The Political Economy of Slavery*; and Eugene D. Genovese and Elizabeth Fox-Genovese, "The Slave Economies in Political Perspective," *Journal of American History* [hereafter, *JAH*], 66 (1979): 7-23.

⁸ These well-known facts are drawn from the U.S. census reports from 1830 to 1860. In a provocative recent essay Leonard P. Curry has argued for the rough equality of antebellum urban development in the North and South. Curry has demonstrated that a large proportion of the South's relatively small antebellum population increase occurred in Southern cities. But, in his ingenious manipulation of demographic data, he failed to consider that a large proportion of a small absolute figure is itself a small absolute figure. Curry's argument for the qualitative similarity of cities in all sections is more successful. Curry, "Urbanization and Urbanism in the Old South: A Comparative View," *JSH*, 40 (1974): 43-60.

its elements cannot be understood by mechanically attempting to weigh the significance of each of these elements and then adding up the total. The impossibility of contriving a simplistic calculus for measuring societies does not, of course, mean that a sensible comparison is impossible. It means only that such a comparison will inevitably be subjective and serve, at best, as a point of departure to those who evaluate the evidence differently.

A comprehensive comparison of the two sections would overlook nothing, not even the weather, which, according to Phillips, "has been the chief agency in making the South distinctive."⁹ In the space available here I shall focus on what our sociological friends might call three social indicators: (1) the economy, (2) the social structure, and (3) politics and power. In selecting these matters for examination, I do not mean to suggest that they are more important than values, ideals, the life of the mind, or any number of other features of antebellum life. Tangible phenomena may be easier to measure than intangible, but they offer no better clue to the essential character of a place and a people. I emphasize economic, social, and political themes because all of them are clearly important, the evidence on them is substantial, and each has recently been re-examined to interesting effect.

THE ECONOMIC PRACTICES OF EACH SECTION—one hesitates to call them economic "systems" in the face of the contradictory and largely planless if not improvisatory nature of these practices—were similarly complex. Northerners and Southerners alike made their living primarily in agriculture. Guided by the unique weather and the unequal length of the growing seasons in their sections, Northern and Southern farmers increasingly specialized, but in dissimilar crops. Tobacco and, above all, rice, sugar, and cotton were largely unknown to the North. Yet in the South, as in the North, farmers—whether large or small—sought and, for the most part achieved, self-sufficiency. They produced more grains and corn than anything else and in both sections raised and kept domestic animals roughly equal in quantity and, it has recently been claimed, comparable in quality.¹⁰ In view of the regularity with which Northern farmers brushed aside the lonely voices in their midst who urged subordination of profits to the "long-range needs of the soil," their money-mindedness in planting wheat (their own great dollar earner) year after year, and their unsentimental readiness to dispose of "family land" so long as the price was right, what Stanley L.

⁹ Phillips, *Life and Labor in the Old South*, 3. I hope to pursue this comprehensive investigation in the future.

¹⁰ Forrest McDonald and Grady McWhiney, "The Antebellum Southern Herdsman: A Reinterpretation," *JSH*, 41 (1975): 147-66; Lewis Cecil Gray, *History of Agriculture in the Southern United States to 1860*, 2 vols. (Washington, 1933); Percy W. Bidwell and John I. Falconer, *History of Agriculture in the Northern United States, 1620-1860* (Washington, 1925); Paul W. Gates, *The Farmer's Age: Agriculture, 1815-1860* (New York, 1962); Albert Fishlow, "Antebellum Interregional Trade Reconsidered," *American Economic Review*, 54 (1964): 352-64; Robert E. Gallman, "Self-Sufficiency in the Cotton Economy of the Antebellum South," *Agricultural History*, 44 (1970): 5-24; Raymond C. Battalio and John Kagel, "The Structure of Antebellum Southern Agriculture: South Carolina, A Case Study," *ibid.*, 25-38; and William H. Hutchinson and Samuel H. Williamson, "Self-Sufficiency of the Antebellum South: Estimates of the Food Supply," *JEH*, 31 (1971): 591-612.

Engerman has said about Southern planters seems to apply equally well to Northern agriculturalists: they were certainly not "non-calculating individuals not concerned with money."¹¹

The enduring popularity of *Gone with the Wind* suggests that the American popular mind continues to believe that the Old South was a land of large plantations populated by masters both honorable and courtly, cruel and sinful, by Southern belles "beautiful, graceful . . . , bewitching in coquetry, yet strangely steadfast," by loyal, lovable, comic, but sometimes surly Negroes, and by white trash or "po' buckra." American historians have, however, known for a least half a century that the plantation legend "is one of great inaccuracy"—false to the character of Southern society, to the diversity of Southern whites, and to the realities of black life.¹² Great plantations centering on splendid mansions did exist in the Old South but not in very great numbers.¹³

The most distinctive feature of the antebellum Southern economy, as of Southern life as a whole, was, of course, its "peculiar institution." Slavery had not been unknown in the North, flourishing through much of the seventeenth and eighteenth centuries and persisting in New Jersey until 1846. But it had involved relatively few blacks and had had slight effect on Northern life and thought.¹⁴ Northern public opinion, better represented by the authors of the Federal Constitution in 1787 and the Missouri Compromise in 1820 than by the abolitionists of the antebellum decades, accepted slavery, approved of doing business with those who controlled it, abhorred its black victims, and loathed Northern whites who agitated against it. Northern acquiescence in Southern slavery does not erase this most crucial difference between the sections, but it does argue for the complementarity and economic interdependence of North and South.¹⁵

The profitability and other economic implications of antebellum slavery have become the subjects of intense recent debate, stimulating the development of

¹¹ Engerman, "A Reconsideration of Southern Economic Growth, 1770-1860," *Agricultural History*, 49 (1975): 343-61.

¹² See Francis Pendleton Gaines, *The Southern Plantation: A Study in the Development and Accuracy of a Tradition* (New York, 1924). For some interesting work on the psychological role of the plantation myth or legend, see George B. Tindall, "Mythology: A New Frontier in Southern History," in Vandiver, *The Idea of the South*, 1-15; Williams, *Romance and Realism in Southern Politics*; Taylor, *Cavalier and Yankee*; C. Hugh Holman, "The Southerner as American Writer," in Sellers, *The Southerner as American*, 180-99; and Cash, *The Mind of the South*.

¹³ Lee Soltow, *Men and Wealth in the United States, 1850-1870* (New Haven, 1975), 133, 142; and Fleisig, "Slavery, the Supply of Agricultural Labor, and the Industrialization of the South," 585-87.

¹⁴ Arthur Zilversmit, *The First Emancipation: The Abolition of Slavery in the North* (Chicago, 1967); Edgar J. McManus, *Black Bondage in the North* (Syracuse, 1973); and Ira Berlin, "Time, Space, and the Evolution of Afro-American Society in British Mainland North America," *AHR*, 85 (1980): 44-78. For the argument that Northern rejection of slavery had been dictated almost entirely by cost considerations, see Carville V. Earle, "A Staple Interpretation of Slavery and Free Labor," *Geographical Review*, 68 (1978): 51-65.

¹⁵ Glover Moore, *The Missouri Compromise, 1819-1821* (Lexington, Ky., 1953); Philip S. Foner, *Business and Slavery: The New York Merchants and the Irrepressible Conflict* (Chapel Hill, 1941); Kinley J. Brauer, *Cotton versus Conscience: Massachusetts Whig Politics and Southwestern Expansion, 1843-1848* (Lexington, Ky., 1967); Leonard L. Richards, *"Gentlemen of Property and Standing": Anti-Abolition Mobs in Jacksonian America* (New York, 1970); and Brian J. Danforth, "The Influence of Socioeconomic Factors upon Political Behavior: A Quantitative Look at New York City Merchants, 1828-1844" (Ph.D. dissertation, New York University, 1974). Danforth has persuasively argued that many Northern businessmen were ready to subordinate all other political considerations to their lucrative cotton trade with the South.

cliometrics or the new economic history.¹⁶ Since slavery was more than a labor system, historians have also searchingly investigated its noneconomic implications for both blacks and whites.¹⁷ A fair reading of the recent evidence and argument is that, while more slaves by far worked as field hands, slaves also performed with great efficiency a great variety of other jobs, many of them skilled, allowing for significant economic differentiation within the slave community.¹⁸ And, as exemplary workers and as costly and valuable properties, skilled slaves were ordinarily spared gratuitous maltreatment or deprivation.¹⁹ Despite the inevitable brutality of the system, slaves appear to have managed to maintain the integrity of their personalities, customs, values, and family ties.

Several trade unionists in the antebellum North agreed with slavery's apologists that not only the working and living conditions but in some respects the "liberty" enjoyed by Northern hirelings compared unfavorably with the situation of slaves.²⁰ These were patently self-serving arguments, designed to put the lot of the Northern worker in the worst possible light. The fact remains that the economic gap between enslaved black and free white workers in antebellum South and North was narrower than historians once thought. Evidence bearing on the conditions of white Northern as well as black Southern labor demonstrates that during the middle decades of the nineteenth century the real wages of Northern workingmen declined and their living conditions remained bleak, their job security was reduced, their skills were increasingly devalued, and in many respects their lives became more insecure and precarious.²¹

¹⁶ A sampling of the recent literature includes Alfred H. Conrad and John R. Meyer, "The Economics of Slavery in the Ante Bellum South," *Journal of Political Economy*, 66 (1953): 95-130; Harold D. Woodman, "The Profitability of Slavery: A Historical Perennial," *JSH*, 29 (1963): 303-25; Yasukichi Yasubi, "The Profitability and Viability of Plantation Slavery in the United States," *Economic Studies Quarterly*, 12 (1961): 60-67; Alfred H. Conrad *et al.*, "Slavery as an Obstacle to Economic Growth in the United States: A Panel Discussion," *JEH*, 27 (1967): 518-60; Robert W. Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 2 vols. (Boston, 1974); Paul A. David and Peter Temin, "Slavery: The Progressive Institution," *JEH*, 34 (1974): 739-83; Richard Sutch, *The Treatment Received by American Slaves: A Critical Review of the Evidence Presented in Time on the Cross* (Berkeley and Los Angeles, 1975); Herbert G. Gutman, *Slavery and the Numbers Game: A Critique of Time on the Cross* (Urbana, Ill., 1975); and Robert W. Fogel and Stanley L. Engerman, "Explaining the Relative Efficiency of Slave Agriculture in the Antebellum South," *American Economic Review*, 67 (1977): 275-96.

¹⁷ Stanley M. Elkins provocatively compared the personalities and psyches of slaves to those of European concentration camp inmates in a work that has inspired a massive and increasingly critical discussion of its thesis; Elkins, *Slavery: A Problem in American Institutional Life* (Chicago, 1959). The following publications, all of them severely critical of Elkins's argument, make important contributions of their own: John W. Blassingame, *The Slave Community: Plantation Life in the Antebellum South* (New York, 1972); George P. Rawick, *From Sundown to Sunup: The Making of the Black Community* (Westport, Conn., 1972); Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slave Owners Made* (New York, 1974); Herbert G. Gutman, *The Black Family in Slavery and Freedom, 1750-1925* (New York, 1976); and Lawrence W. Levine, *Black Culture and Black Consciousness: Afro-American Folk Thought from Slavery to Freedom* (New York, 1977).

¹⁸ On this point, see Robert S. Starobin, *Industrial Slavery in the Old South* (New York, 1970); Charles B. Dew, "Disciplining Slave Ironworkers in the Antebellum South: Coercion, Conciliation, and Accommodation," *AHR*, 79 (1974): 393-418; and Wall, "An Epitaph for Slavery," 239.

¹⁹ In *Time on the Cross*, Fogel and Engerman have argued cliometrically and, as their critics have pointed out, unpersuasively that slaves were rarely whipped and were comparatively well fed. For contrasting viewpoints on the quality of the slaves' diet or, more precisely, on the quality of the pork eaten by Southerners, both white and black, see Kenneth F. Kiple and Virginia A. Kiple, "Black Tongue and Black Men: Pellagra and Slavery in the Antebellum South," *JSH*, 43 (1977): 411-28; and Grady McWhiney, "The Revolution in Nineteenth-Century Alabama Agriculture," *Alabama Review*, 31 (1978): 3-32.

²⁰ John Finch, *Rise and Progress of the General Trades' Union of the City of New York* (New York, 1833); and the *New York Working Man's Advocate*, March-April 1844.

²¹ Antebellum labor spokesmen in the North made precisely these charges; see Edward Pessen, *Most Uncommon Jacksonians: The Radical Leaders of the Early Labor Movement* (Albany, N.Y., 1967), pt. 2. Diverse modern evi-

At mid-century industrial workers in the South as in the North worked primarily in small shops and households rather than in factories. Trade unionists in Baltimore, Louisville, St. Louis, and New Orleans were with few exceptions skilled and semi-skilled white artisans, precisely as they were in Philadelphia, New York, Boston, and Pittsburgh. In Southern as in Northern towns and cities, the least skilled and prestigious jobs were those done preponderantly by Catholic immigrants rather than by older Protestant, ethnic groups.²² Significantly, the South attracted far fewer of the antebellum era's "new immigrants"—that is, Germans and Irish—than did the North. For all of their smaller numbers in the South, European immigrants played an economic and social role there that was not dissimilar to what it was in the North. Diverse measurable evidence indicates that the pattern of immigrant life in the United States was national, rather than distinctly regional, in character. A similar point can be made about Southern urbanism and manufacturing—namely, quantitative distinctiveness (or deficiency), qualitative similarity to the North. Although the value of Southern manufactured products was usually less than one-fifth of the national total during the antebellum decades, the South was hardly a region devoid of industrial production. Articulate Southerners "crusade[d] to bring the cotton mills to the cotton fields," and, whether due to their exhortations or to the play of market forces, the amount of capital the slave states invested in cotton manufacturing doubled between 1840 and 1860, surpassing their rate of population growth.²³ Because the South nevertheless lagged far behind the Northeast in manufacturing, one influential school of historians has described the antebellum economy—and, for that matter, Southern society as a whole—as noncapitalist, prebourgeois, or "seigneurial."²⁴

Some historians have criticized Southern deficiencies in commerce, finance, transportation, and manufacturing as manifestations of economic wrongheadedness and irrationality and have attributed to these deficiencies the South's defeat in the Civil War. A number of modern economic historians, cliometricians for the most part, have interpreted the evidence somewhat differently. Invoking the old argument of "comparative advantage," they have noted that heavy investment in cotton, the nation's great dollar earner in international trade, was hardly irrational, since it enabled the South to equal the national rate of profit during the era. Southerners who did invest in Southern factories

dence sustains labor's complaints to a surprising degree. See Stanley Lebergott, *Manpower in Economic Growth* (New York, 1964); Walter B. Smith, "Wage Rates on the Erie Canal, 1828-1861," *JEH*, 23 (1963): 298/311; Bruce Laurie, "Nothing on Compulsion: Life-Styles of Philadelphia Artisans, 1820-1850," *Labor History*, 15 (1974): 337-76; Herbert G. Gutman, "Work, Culture, and Society in Industrializing America, 1815-1919," *AHR*, 78 (1973): 531-88; and Alan Dawley, *Class and Community: The Industrial Revolution in Lynn* (Cambridge, Mass., 1976).

²² This point is the thesis of a paper on antebellum Southern free labor by Herbert G. Gutman and Ira Berlin that Gutman presented to the Columbia University Seminar on the City in the spring of 1976. For the North, see Theodore Hershberg *et al.*, "Occupation and Ethnicity in Five Nineteenth-Century Cities: A Collaborative Inquiry," *Historical Methods Newsletter*, 7 (1974): 174-216; and Kathleen Neils Conzen, *Immigrant Milwaukee, 1836-1860: Accommodation and Community in a Frontier City* (Cambridge, Mass., 1976).

²³ Herbert Collins, "The Southern Industrial Gospel before 1860," *JSH*, 12 (1946): 386-402; and Starobin, *Industrial Slavery in the Old South*, 13.

²⁴ See, in particular, Genovese, *The Political Economy of Slavery*, and *The World the Slaveholders Made*; Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966); and Luraghi, *The Rise and Fall of the Plantation South*.

got a return that compared favorably with industrial profits elsewhere. (Why, ask the critics, didn't they invest more of their capital that way?) If Southern manufacturing was outdistanced by that in the Northeast, it compared favorably with industrial production in the Northwest and, for that matter, in Continental Europe in the mid-nineteenth century. If the South suffered inordinately in the wake of the financial panics of 1837 and 1839, it was, as Reginald C. McGrane noted long ago, precisely because the South had speculated excessively in transportation projects and land acquisition as well as other investments. The South's "unusually favorable system of navigable streams and rivers" has been cited to explain its lag in railroads. Yet in the 1840s Southern railroads "equalled or exceeded the national average capitalization per mile." The views of many scholars are expressed in Gavin Wright's recent observation that "before the War the South was wealthy, prosperous, expanding geographically, and gaining economically at rates that compared favorably to those of the rest of the country."²⁵

Antebellum Northern investors, like their counterparts in the South and in Europe, put their money into American products, industrial and agricultural, solid and flimsy, drawn almost entirely by the profit margin likely to result from their investment. Investors in all latitudes appear to have been indifferent to possible long-range consequences of their financial transactions, acting rather on the principle that the "rational" investment was the one likely to pay off. That the railroads, the diversified industry, and the commercial superiority of the North turned out to have important military implications in the 1860s could hardly have been anticipated by earlier profit-seekers. When the commercial magnates known as the Boston Associates invested heavily in factories built in the new suburbs of Boston, they hardly had in mind outfitting Union troops a generation later; they were much more concerned about maintaining close ties with Southern cotton magnates on whose raw materials they were so heavily dependent. There is something bizarre in historians, more than a century after the event, scrutinizing the economic behavior of antebellum capitalists and subjecting that behavior to unrealistic tests of rationality and farsightedness that these men themselves would have found farfetched.

To argue, however, as several historians have, that a substantial Southern

²⁵ Wright, *The Political Economy of the Cotton South*, 89. The other assertions in this paragraph are based on Paul A. David, "The Growth of Real Product in the United States before 1840: New Evidence, Controlled Conjectures," *JEH*, 27 (1967): 151-95; Fogel and Engerman, *Time on the Cross*, 1: 254-55; Fred Bateman and Thomas Weiss, "Comparative Regional Development in Antebellum Manufacturing," *JEH*, 35 (1975): 182-208; Fred Bateman *et al.*, "Profitability in Southern Manufacturing: Estimates for 1860," *Explorations in Economic History*, 12 (1975): 211-12, 226-30; James Roger Sharp, *The Jacksonians versus the Banks: Politics in the States after the Panic of 1837* (New York, 1970), 28; McGrane, *The Panic of 1837: Some Financial Problems of the Jacksonian Era* (Chicago, 1924); James A. Ward, "A New Look at Antebellum Southern Railroad Development," *JSH*, 39 (1973): 413, 419-20; Lewis E. Atherton, *The Southern Country Store, 1800-1860* (Baton Rouge, 1949); Merl E. Reed, *New Orleans and the Railroads: The Struggle for Commercial Empire, 1830-1860* (Baton Rouge, 1966); Fred Bateman and Thomas Weiss, "Manufacturing in the Antebellum South," in Paul Uselding, ed., *Research in Economic History: An Annual Compilation of Research*, 1 (Greenwich, Conn., 1976): 1-44; Fleisig, "Slavery, the Supply of Agricultural Labor, and the Industrialization of the South," 592; and Fred Bateman *et al.*, "The Participation of Planters in Manufacturing in the Antebellum South," *Agricultural History*, 48 (1974): 277-97. Blaine A. Brownell has argued that Southern industrialism was expanding and that there were "remarkable structural similarities in Southern and Northern industry on the eve of the Civil War." But his argument largely rests on the impressionistic observations of European visitors. Brownell, "Urbanization in the South: A Unique Experience?" *Mississippi Quarterly*, 26 (1973): 105-20.

lag—whether in railroad mileage or urban growth—is not as great when it is measured in *per capita* rather than absolute terms²⁶ explains away rather than explains these fundamental sectional differences. For it can reasonably be maintained that the antebellum South's comparatively small white population (which accounted for its high *per capita* rates) was not due to historical accident but to significant features, if not failings, in Southern civilization. That all differences between two communities indubitably have a historical explanation—be it the smaller population, the hotter climate, or the prevalence of enslaved blacks—in no sense detracts from the significance of those differences. The burden of my argument is not that antebellum economic developments in the states south of the Potomac were almost exactly like, let alone a mirror image of, those in the states north of the river but rather that the economies were similar in significant ways that are often taken for granted, as, for example, in the similar operation of the profit motive or the similarity of the laws of inheritance in the two sections. And even where, as in industrial production and labor systems, the South and North differed most glaringly, modern evidence has reduced and placed in a somewhat different perspective the gulf between them. As for the recent suggestion that the South was not capitalistic, I shall defer comment until I have first dealt with social and political matters, since capitalism concerns more than economic arrangements alone.

HISTORIANS HAVE LONG KNOWN that a society's social structure offers an important clue to its character. The kind of social classes that exist, the gulf between them, their roles in society, the ease or difficulty of access to higher from lower rungs on the social ladder, and the relationships between the classes tell as much about a civilization as do any other phenomena.²⁷ What distinguishes modern from earlier historians in their treatment of social class is the extent to which they have borrowed from social scientists both in theorizing about class and in the methodology used for measurement. Employing these new approaches, historians have drastically modified earlier notions of antebellum society.

The ancient belief that the white antebellum South consisted of two classes, wealthy planters at the top and a great mass of poor whites below, may continue to command some popular acceptance. That belief has been so long dead among historians, however, that as early as 1946 Fabian Linden could remark that "the debunking of the 'two class' fallacy" had "become the tedious cliché."²⁸ For, beginning in 1940 and continuing steadily thereafter, Frank L. Ows-

²⁶ Fogel and Engerman, *Time on the Cross*, 1: 254; and Curry, "Urbanization and Urbanism in the Old South," 43-60.

²⁷ For a discussion of the variety of ways of defining class, see Harold Hodges, *Social Stratification: Class in America* (Cambridge, Mass., 1969); Leonard Reissman, *Class in American Society* (New York, 1967); W. Lloyd Warner with Marchia Meeker and Kenneth Eells, *Social Class in America: A Manual of Procedure for the Measurement of Social Status* (New York, 1960); Bernard Barber, *Social Stratification: A Comparative Analysis of Structure and Process* (New York, 1957); Reinhard Bendix and Seymour Martin Lipset, eds., *Class, Status, and Power: Social Stratification in Comparative Perspective* (New York, 1966); and Edward Pessen, *Riches, Class, and Power before the Civil War* (Lexington, Mass., 1973), 165-247.

²⁸ Linden, "Economic Democracy in the Slave South: An Appraisal of Some Recent Views," *Journal of Negro History*, 31 (1946): 187.



Figure 1: D'Evereux, located in Natchez, Mississippi, is one of the best examples of Greek revival architecture in the South; it was built in 1840, and James Hardie was the architect. Photograph reproduced courtesy of the author-photographer from G. E. Kidder Smith, *A Pictorial History of Architecture in America*, 1 (New York: American Heritage Publishing Company, Inc., 1976): 367.

ley and a group of scholars influenced by his work utilized hitherto neglected primary sources to reveal that the most typical white Southerners by far were small farmers working the modest acreage they owned with few, if any, slaves.²⁹

The too neat portrait that the Owsley school drew of the white Southern social structure was quite similar to the picture of *Northern* society accepted by historians less than a generation ago.³⁰ The white population was ostensibly composed primarily of the great "middling orders," hard-working, proud, and not unprosperous farmers for the most part, whose chance to rise even higher so-

²⁹ Ulrich B. Phillips and Lewis Cecil Gray early in this century portrayed a Southern economy and society composed of diverse elements; *Life and Labor in the Old South*, 339-40; and *History of Agriculture in the Southern United States to 1860*, *passim*. But Frank L. Owsley and his disciples effectively dealt the death blow to the two-class concept. Representative writings by the "Owsley School" include Frank L. Owsley and Harriet C. Owsley, "The Economic Basis of Society in the Late Ante-Bellum South," *JSH*, 6 (1940): 24-45; Harry L. Coles, "Some Notes on Slaveownership and Landownership in Louisiana, 1850-1860," *ibid.*, 9 (1943): 381-94; Frank L. Owsley, *Plain Folk of the Old South* (Baton Rouge, 1949); Blanche H. Clark, *The Tennessee Yeoman, 1840-1860* (Nashville, 1942); Chase C. Mooney, "Some Institutional and Statistical Aspects of Slavery in Tennessee," *Tennessee Historical Quarterly*, 50 (1942): 195-228; and Herbert Weaver, *Mississippi Farmers, 1850-1860* (Nashville, Tenn., 1945).

³⁰ In David Potter's phrase, the Owsley School had "delineated the structure of antebellum society in terms in which large slaveholders and plain farmers were practically indistinguishable"; *The South and Sectional Conflict*, 14. For a devastating critique of the Owsley approach, see Linden, "Economic Democracy in the Slave South," 140-89. For an equally critical recent statement, see Wright, "'Economic Democracy' and the Concentration of Agricultural Wealth in the Cotton South, 1850-1860," *Agricultural History*, 44 (1970): 63-94. Also see Eaton, *The Growth of Southern Civilization*, 154-60; and Campbell and Lowe, *Wealth and Power in Antebellum Texas*. For a rare—rare, that is, in this scholarly era—opposing viewpoint, see Eugene D. Genovese, "Yeoman Farmers in the Slaveholders' Democracy," *Agricultural History*, 49 (1975): 341.



Figure 2: Rose Hill, located in Geneva, New York, is a fine example of Northern Greek revival architecture; William K. Strong, a retired New York City merchant, built the mansion in 1839 on land purchased from the Rose family. Photograph reproduced courtesy of the author-photographer from Kidder Smith, *A Pictorial History of Architecture in America*, 188.

cially matched the opportunities an increasingly democratic society gave them to exert political influence and power. Small groups of rich men—great planters in the one clime and merchants and industrialists in the other—occupied the highest social plateau; professionals who served the rich were slightly above the middle, which was occupied by small business people and independent farmers, skilled artisans, and clerks; and below them stood industrial and landless agricultural laborers. Since class is determined not by bread alone, blacks—whether slave or free and regardless of how much individuals among them had managed to accumulate—were universally relegated to the lowest levels of the social structure, scorned even by white vagrants and frequently unemployed workers, urban and rural, who constituted America's equivalent of a propertyless proletariat.³¹

The achievement of recent research is its transformation of what was a rather blurred image of social groups, whose membership and possessions were both

³¹ For the Southern class structure, see Frank Huffman, Jr., "Town and Country in the South, 1850–1880: A Comparison of Urban and Rural Social Structures," *South Atlantic Quarterly*, 76 (1977): 366–81; Owsley, *Plain Folk of the Old South*; Craven, *The Coming of the Civil War*, 27–31; James C. Bonner, "Profile of a Late Antebellum Community," *AHR*, 49 (1943–44): 663–80; D. L. A. Hackett, "The Social Structure of Jacksonian Louisiana," *Louisiana Studies*, 12 (1973): 324–53; McDonald and McWhiney, "The Antebellum Southern Herdsman"; and Roger W. Shugg, *Origins of Class Struggle in Louisiana* (University, La., 1939). For a devastating critical analysis of Shugg's research, see Joseph G. Tregle, Jr., "Another Look at Shugg's Louisiana," *Louisiana History*, 17 (1976): 245–81. And, for a modest, lovely book about merchants and their place in the Southern community, see Atherton, *The Southern Country Store, 1800–1860*.

unclear, into a more sharply focused picture. By digging deeper, particularly in nineteenth-century data on wealth and property, historians have come close to knowing the numbers of families belonging to different wealth strata and the amount of wealth these families owned. The beauty of the new evidence on who and how many owned what and how much is that in the antebellum era wealth appears to have been the surest sign of social, as well as of economic, position. Antebellum wealth was almost invariably made in socially acceptable ways. Modern scholars have found that "the social divisions of antebellum America were essentially wealth-holding categories." The upper class did not comprise so much the families who "controlled the means of production" as it did the families who "controlled the vast wealth created largely through the exchange of goods produced."³² Degree of wealth was the surest sign of the quality of housing, furnishings, and household goods a family could afford, of its style of living and uses of leisure, and of the social circle within which it moved and its individual members married. Gathering from the manuscript census schedules, probate inventories, and tax assessors' reports statistically valid samples or, in some cases, evidence on every family in the community under study, modern scholars have been able to arrange the antebellum Southern and Northern populations on a wealth-holding scale. While it is close to a statistical inevitability that the distribution of wealth in the South and North would not be precisely the same, the most striking feature of the evidence is how similarly wealth was distributed—or maldistributed—in the two sections.

On the eve of the Civil War one-half of the free adult males in both the South and the North held less than 1 percent of the real and personal property. In contrast, the richest 1 percent owned 27 percent of the wealth. Turning from the remarkable similarity in sectional patterns of wealthholding at the bottom and the very top, the richest 5 to 10 percent of propertyowners controlled a somewhat greater share of the South's wealth, while what might be called the upper middle deciles (those below the top tenth) held a slightly smaller share in the North. The South also came close to monopolizing wealthy counties, the per capita wealth of which was \$4,000 or more and, despite its smaller population, the South, according to the 1860 census, contained almost two-thirds of those persons in the nation whose worth was at least \$110,000. According to Lee Soltow, the leading student of this evidence, these sectional disparities "could be attributed almost entirely to slave values. . . . If one could eliminate slave market value from the distribution of wealth in 1860 . . . , the inequality levels in the North and South were similar."³³

In view of the centrality of slavery to the antebellum South, it is idle to speak of "eliminating the market value" of slaves from the sectional comparison. Northern free labor, rural and industrial, also represented a form of "sectional wealth," if a much overlooked form. Although as individual human beings they

³² Wright, *The Political Economy of the Cotton South*, 37; and Edward Pessen, "The Social Configuration of the Antebellum City: An Historical and Theoretical Inquiry," *Journal of Urban History*, 2 (1976): 278-79.

³³ Soltow, *Men and Wealth in the United States, 1850-1870* (New Haven, 1975), 99, 101, 133-34, 149, 152, 158. But see Wright, *The Political Economy of the Cotton South*, 25-27.

did not add to their own private wealth or to the wealth of the employers they served, their labor created wealth for themselves and for these same capitalists at rates of productivity that, I believe, even Robert W. Fogel and Stanley L. Engerman would concede compared favorably with the rates of the most efficient slaves. In other words, the North had access to a form of wealth, free labor, that was roughly as valuable per capita as was slave wealth, however absent this Northern wealth was from the reports prepared by census takers and assessors. Given the known habits of these officials to overlook small property holdings—precisely the kind of holdings that would have been owned by Northern working people—and to accept as true the lies people swore to as to their worth,³⁴ it is likely that the fairly substantial cumulative wealth owned by small farmers and modest wage earners was almost entirely omitted from the wealth equation. Such groups were far more numerous in the North than in the South. Had slaves been treated as part of the potential property-owning Southern population to which they actually belonged, instead of being treated as property pure and simple, the total wealth of the antebellum South would have been diminished by several billion dollars: the product of multiplying the number of slaves by the average market price of almost \$1,000 per slave.³⁵ The addition of nearly four million very poor black people to the number of potential propertyowners in the South would have increased its rate of inequality (and the Gini coefficient of concentration that measures it), although not everywhere to the same extent.³⁵

Wealth in both sections was distributed more equally—perhaps the more apt phrase is less unequally—in the countryside than in towns and cities. While the rural North has been less intensively investigated than its Southern counterpart, enough research has been completed to disclose that the North was hardly a haven of egalitarian distribution of property. Rural Wisconsin (which had a Gini coefficient of inequality as high as that of antebellum Texas), the Michigan frontier, and northwestern New York State were centers of inequality and pov-

³⁴ See Edward Pessen, "The Egalitarian Myth and the American Social Reality: Wealth, Mobility, and Equality in the 'Era of the Common Man,'" *AHR*, 76 (1971): 995, 998, and "The Wealthiest New Yorkers of the Jacksonian Era: A New List," *New-York Historical Society Quarterly*, 54 (1970): 148-52; Soltow, *Men and Wealth in the United States, 1850-1870*, *passim*; and Richard K. Jones, "Stonington Borough: A Connecticut Seaport in the Nineteenth Century" (Ph.D. dissertation, City University of New York, 1976), 424-25. Jones has shown that in dozens of cases the market value of probated estates was two to four times the value that had only recently been attributed to these same estates by assessors.

³⁵ Randolph B. Campbell and Richard G. Lowe's stimulating and excellent "counterfactual exercise" in assessing the effect on the antebellum South's distribution of wealth of "freeing the slaves" has the one omission that I can detect of failing to restore—and with interest—the capital that Southerners had invested in slave purchases; *Wealth and Power in Antebellum Texas*, 55, 135, 146-53. The same theme has also engaged Gavin Wright, Robert Fogel and Stanley Engerman, Lee Soltow, and Robert Gallman. Also see Engerman, "Some Considerations Relating to Property Rights in Man," *JEH*, 33 (1973): 43-65.

³⁶ This is, at least, the conclusion of Campbell and Lowe, who have found that "freeing" slaves and thus diminishing the wealth of rich propertyowners is balanced out by the substantial enlargement of potential propertyowners, in the persons of very poor blacks. In antebellum Charleston, by contrast, transforming slaves from property into (potentially property-owning) people would have had a marked effect on wealth distribution, increasing the proportion of the propertyless by almost a third and substantially expanding the percentages of wealth owned by the richest 1 percent and 10 percent. The explanation of this maldistribution is the very large proportion of blacks in Charleston, where they comprised more than 50 percent of the population. See Michael P. Johnson, "Wealth and Class in Charleston in 1860," paper presented at the Citadel Conference on the South, held in Charleston, April 19-21, 1979, p. 5.

erty. At mid-century, the proportion of white men who owned land in any amount was substantially lower in the Northwest than in the South. The percentage of free males owning land in the North as a whole was slightly smaller than in the South. Owing to the absence of slaves and to the relative paucity of very large farms, wealth was somewhat less unequally distributed in the rural North than in the South.³⁷

In investigating the distribution of wealth in the antebellum rural South, scholars have probed data on different states, counties, and regions. The patterns throughout are remarkably similar, whether for wealth in general, land and real estate, or personal and slave property. Accentuating the maldistribution of landed wealth—whether in Alabama, Mississippi, Louisiana, Texas, the “cotton South,” or the agricultural South as a whole—was a fact of life that the Owsley school neglected: the dollar value per acre of large farms owned by slave-owning planters was substantially greater than the value per acre of the small farm. And yet, regardless of the nature of the soil or the proportion of large farms in a given region, the rates of wealth concentration were remarkably similar as well as constant during the decades before the war. Paralleling the recent finding that in antebellum Texas, no matter what the differences were “in climate, soil, and extent of settlement, the most striking fact is . . . the high degree of concentration in wealthholding across all the regions,” another recent study reports no great differences in “the degree of inequality” between the cotton South and the other “major agricultural regions” of grain, tobacco, sugar, and rice production in 1860.³⁸

The distribution of slave wealth closely followed the pattern of other forms of Southern wealth.³⁹ During the decade before the war, slaveownership was confined to between 20 and 25 percent of white families, and maldistribution of this form of property was the rule within the slave-owning population. Half of all slaveowners owned five or fewer slaves, with only one-tenth owning the twenty or more slaves that by Ulrich B. Phillips’s definition made them “planters.” Less than one-half of 1 percent owned one hundred or more slaves. As with other forms of wealth, the concentration of slave wealth increased slightly between 1850 and 1860.⁴⁰

³⁷ Lee Soltow, *Patterns of Wealthholding in Wisconsin since 1850* (Madison, Wisc., 1971), 78, 79, 88, and *Men and Wealth in the United States, 1850–1870*, 41; Campbell and Lowe, *Wealth and Power in Antebellum Texas*, 128–29; George Blackburn and Sherman L. Richards, Jr., “A Demographic History of the West: Manistee County, Michigan, 1860,” *JAH*, 57 (1970): 613, 618; and Wendell Tripp, ed., *Through “Poverty’s Vale”: A Hardscrabble Boyhood in Upstate New York, 1832–1867* (Syracuse, 1974).

³⁸ Campbell and Lowe, *Wealth and Power in Antebellum Texas*, 35–42, 45, 55, 129–30, 135; Albert Niemi, Jr., “Inequality in the Distribution of Slave Wealth: The Cotton South and Other Southern Agricultural Regions,” *JEH*, 37 (1977): 747–53; Linden, “Economic Democracy in the Slave South,” 150–51, 157–59, 161–64, 170–72; Bonner, “Profile of a Late Ante-Bellum Community,” 673, 675; Soltow, *Men and Wealth in the United States, 1850–1870*, *passim*; and Gavin Wright, “‘Economic Democracy’ and the Concentration of Agricultural Wealth in the Cotton South, 1850–1860,” 74. For Wright’s minor revisions of the wealth estimates for 1850, see his more recent *Political Economy of the Cotton South*, 31. Huffman’s “Town and Country in the South” is indeed the “hurried survey” its author conceded it to be.

³⁹ According to Albert Niemi, “inequality in slave holdings is a good proxy for inequality in total wealth” throughout the antebellum South; “Inequality in the Distribution of Slave Wealth.”

⁴⁰ Campbell and Lowe, *Wealth and Power in Antebellum Texas*, 43–44. Also see Wright, *The Political Economy of the Cotton South*, 34–35; Linden, “Economic Democracy in the Slave South,” 150–52, 166; and Soltow, *Men and Wealth in the United States, 1850–1870*, 134, 142.

While the South had long lagged behind the North in urban development, recent scholarship has unearthed evidence that Southern cities grew at a remarkable rate during the antebellum decades.⁴¹ If the Southern rate of urban expansion still did not match the Northern quantitatively, Southern cities, old and new, were qualitatively not unlike their Northern counterparts.⁴² Antebellum cities in all latitudes were amazingly similar in the roles they played in the political, administrative, financial, economic, artistic, and intellectual affairs of their regions. Antebellum cities were also alike in the types of men who ran them, in the underlying social philosophies guiding those men, and in their "social configurations."⁴³ Not the least of the similarities of cities in both great sections was in their distribution of wealth.

Three things can be said about the distribution of wealth in the towns and cities of the Old South. Property ownership was even more concentrated there than in rural areas. Riches became more unequally distributed with the passage of time, with the proportion of the propertyless increasing sharply between 1850 and 1860. There was an increase too in the proportion of urban wealth owned by the largest wealthholders—at least for the dozen communities measured to date.⁴⁴ And the patterns of wealth distribution in Southern cities were very much like those that obtained in the North.

The pattern of wealth distribution in Providence and Newport (Rhode Island), Pelham and Ware (Massachusetts), Newark, Pittsburgh, Cleveland, Mil-

⁴¹ Curry, "Urbanization and Urbanism in the Old South"; Huffman, "Town and Country in the South"; Lyle W. Dorsett and Albert H. Shaffer, "Was the Antebellum South Antiurban? A Suggestion," *JSH*, 38 (1972): 93-100; Brownell, "Urbanization in the South"; Richard J. Hopkins, "Are Southern Cities Unique? Persistence as a Clue," *Mississippi Quarterly*, 26 (1973): 121-44; James M. Russell, "Atlanta, Gate City of the South, 1847 to 1885" (Ph.D. dissertation, Princeton University, 1972); and David R. Goldfield, "Pursuing the American Dream: Cities in the Old South," in Blaine A. Brownell and David R. Goldfield, eds., *The City in Southern History: The Growth of Urban Civilization in the South* (Port Washington, 1977), 52-91, 198-204.

⁴² Curry has offered an ingenious but unpersuasive argument designed to show that the urban proportion of the antebellum South's total population growth compared favorably with the Northern figure. For my critique, see note 8, above. The fact remains, however, that, from a starting point at the beginning of the century that was well behind the North, the South, by a number of significant measures, did come close to matching Northern urban growth during the antebellum decades.

⁴³ Pessen, "The Social Configuration of the Antebellum City," 267-306; D. Clayton James, *Antebellum Natchez* (Baton Rouge, 1968); Kenneth W. Wheeler, *To Wear a City's Crown: The Beginnings of Urban Growth in Texas, 1836-1865* (Cambridge, Mass., 1968); Richard C. Wade, *The Urban Frontier* (Cambridge, Mass., 1959); Robert C. Reinders, *End of an Era: New Orleans, 1850-1860* (New Orleans, 1964); Michael H. Frisch, *Town into City: Springfield, Massachusetts, and the Meaning of Community, 1840-1880* (Cambridge, Mass., 1972); Sam Bass Warner, Jr., *The Private City: Philadelphia in Three Periods of Its Growth* (Philadelphia, 1968); Stuart M. Blumin, *The Urban Threshold: Growth and Change in a Nineteenth-Century American Community* (Chicago, 1976); Richard S. Alcorn, "Leadership and Stability in Mid-Nineteenth-Century America: A Case Study of an Illinois Town," *JAH*, 61 (1974): 685-702; Harold Hurst, "The Elite Class of Newport, Rhode Island, 1830-1860" (Ph.D. dissertation, New York University, 1975); Pessen, *Riches, Class, and Power before the Civil War*; and Robert Doherty, *Society and Power: Five New England Towns, 1800-1860* (Amherst, Mass., 1977). In preparation is a comparative study of antebellum Charleston and Boston by Jane H. Pease and William H. Pease that, from the glimpses of it I have seen, promises to be a fascinating piece of work pointing in a direction that differs from the one I have followed. The Peases presented a stimulating paper, "Social Structure and the Potential for Urban Change: Boston and Charleston in the 1830's," at the Ninety-Third Annual Meeting of the American Historical Association, held in San Francisco, December 28-30, 1978.

⁴⁴ Susan Jackson, "Movin' On: Mobility through Houston in the 1850's," *Southwestern Historical Quarterly*, 81 (1978): 260; Jackson, "The People of Houston in the 1850's"; Campbell and Lowe, *Wealth and Power in Antebellum Texas, passim*; Robert E. Gallman, "Trends in the Size Distribution of Wealth in the Nineteenth Century: Some Speculations," in Lee Soltow, ed., *Six Papers on the Size Distribution of Wealth and Income* (New York, 1969); Johnson, "Wealth and Class in Charleston in 1860," 4, 12-13; Wheeler, *To Wear a City's Crown*, 31, 79, 110, 114, 131-32; and James, *Antebellum Natchez*, 136-37, 153-59, 165-66, 169-70, 178.

wauke, the great cities on the Northeastern seaboard, and a dozen other Northern urban centers was impressively consistent and glaringly unequal. The sharp maldistribution of the 1820s and 1830s became more widely skewed with the passage of time (the Gini coefficients of inequality for 1860 matched those prevalent in the South). On the eve of the Civil War, the wealth of most cities, while greatly augmented, was "less widely dispersed than it had been earlier"; the propertyless groups in Stonington (Connecticut) and Chicago, for example, comprised between two-thirds and three-fourths of all households by the outbreak of the war.⁴⁵

Nor do sectional rates of vertical mobility appear to have been much different. In 1856 Cassius M. Clay told an Ohio audience that "the northern laboring man could, and frequently did, rise above the condition [into] which he was born to the first rank of society and wealth," but he "never knew such an instance in the South."⁴⁶ Recently unearthed evidence on the social origins of the men in the "first rank" does not sustain Clay's surmise, so popular with contemporary yeasayers. In the South, "increasing barriers to slaveownership resulting from higher slave prices and the growing concentration of wealth" left "lesser planters," not to mention laboring men, with their "aspiration thwarted."⁴⁷ And in the North—whether in Wayne County (Michigan), Newport, Stonington, small towns in Massachusetts, Chicago, and Brooklyn, or the great cities of New York, Boston, and Philadelphia—eminent and rich men of humble birth were a rarity. Evidence on the more likely movement from a lower social position to an adjacent one, rather than to the very top, remains in pitifully short supply. In antebellum Philadelphia, small New England counties, and rural Georgia, even the modest movement from one plebian level to another appears to have seldom occurred.⁴⁸

⁴⁵ Doherty, *Society and Power: Five New England Towns*, 48; Hurst, "The Elite Class of Newport, Rhode Island," 58, 65, 67, 76, 77; Jones, "Stonington Borough," 351–52; Stuart M. Blumin, "Mobility and Change in Antebellum Philadelphia," in Stephan Thernstrom and Richard Sennett, eds., *Nineteenth-Century Cities: Essays in the New Urban History* (New Haven, 1969), 204; Pessen, "The Egalitarian Myth and the American Social Reality," 1019–27; Craig Buettinger, "Economic Inequality in Early Chicago, 1840–1850," *Journal of Social History*, 11 (1978): 414; and Lee Soltow, "The Wealth, Income, and Social Class of Men in Large Northern Cities of the United States in 1860," in James D. Smith, ed., *The Personal Distribution of Income and Wealth* (New York, 1975), 235–41. I have omitted a discussion of comparative sectional income because of the paucity, spottiness, and unreliability of aggregate income estimates for this period. For a devastating critique of the weaknesses in the 1840 census, in Richard A. Easterlin's study based on it, "Interregional Differences in Per Capita Income, Population, and Total Income, 1840 to 1950," in *Trends in the American Economy in the Nineteenth Century: Studies in Income and Wealth*, vol. 24 (Princeton, 1960), and in the numerous studies that in turn rely on Easterlin's conjectures, see Gerald Gunderson, "Southern Ante-Bellum Income Reconsidered," *Explorations in Economic History*, 10 (1973): 151–76.

⁴⁶ Clay, as quoted in Eric Foner, *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party before the Civil War* (New York, 1970), 48. Also see Eaton, *The Growth of Southern Civilization*, 152–53; Simkins, *The Everlasting South*, 38–39; and Owsley, *Plain Folk of the Old South*, 133–34.

⁴⁷ Wright, *The Political Economy of the Cotton South*, 35–37; James L. Roark, *Masters without Slaves: Southern Planters in the Civil War and Reconstruction* (New York, 1977), 8; Shugg, *Origins of Class Struggle in Louisiana*, 33; Bonner, "Profile of a Late Ante-Bellum Community," 673, 675, 679; and Michael P. Johnson, "Planters and Patriarchy: Charleston, 1800–1860," *JSH*, 46 (1980): 47.

⁴⁸ Alexandra McCoy, "The Political Affiliations of American Economic Elites: Wayne County, Michigan, 1844–1860, as a Test Case" (Ph.D. dissertation, Wayne State University, 1965); Jones, "Stonington Borough"; Hurst, "The Elite Class of Newport, Rhode Island," 47–53; Danforth, "The Influence of Socioeconomic Factors upon Political Behavior"; Doherty, *Society and Power: Five New England Towns*, 61–62, 69; Stuart Mack Blumin, "Mobility in a Nineteenth-Century American City: Philadelphia, 1820–1860" (Ph.D. dissertation, University of Pennsylvania, 1968); Peter R. Knights, *The Plain People of Boston, 1830–1860: A Study in City Growth* (New York, 1971), chap. 5; Pessen, *Riches, Class, and Power before the Civil War*, pt. 2; and Buettinger, "Economic Inequality in Early Chicago," 416–17.

Throwing important, if indirect, light on the relatively slight opportunities for upward social and economic movement antebellum America offered to poor or economically marginal men is the era's high rate of physical or geographical mobility. In rural as well as urban communities, in large cities and small, and on both sides of the Mason-Dixon line, armies of footloose Americans were on the move, following trails never dreamed of in the Turner thesis. One-half of the residents, primarily the poorer and propertyless, left those communities from one decade to another in their search for a more acceptable living.⁴⁹ I have no doubt that future research will yet disclose that, during what was a period of economic expansion in both sections, significant numbers of Americans improved their lot, even if modestly.⁵⁰ To date, however, the data reveal equally slight rates of social mobility and high rates of geographical mobility on both sides of the Mason-Dixon line.

Carl Degler has recently observed that Southern society "differed from northern in that the social hierarchy culminated in the planter, not the industrialist."⁵¹ At mid-century, great Northern fortunes, in fact, owed more to commerce and finance than to manufacturing. What is perhaps more important is that a sharply differentiated social hierarchy obtained in both sections. In Degler's phrase, planter status was "the ideal to which other white southerners aspired." A good case can be made for the equally magnetic attraction that exalted merchant status had for Northerners. If the fragmentary evidence on Virginia, Georgia, and the Carolinas, which Jane H. Pease has so effectively exploited, is any indication, then great planters lived less sybaritically and consumed less conspicuously than historians have previously thought. If Philip Hone's marvelous diary—two dozen full-to-the-brim volumes of life among the swells during the antebellum decades—has broader implication, then the Northeastern social and economic elite commanded a lifestyle of an elegance and costliness that, among other things, proved irresistably attractive to the aristocratic Southerners who graced Hone's table, pursued diversion with other members of Hone's set, and married into its families—the Gardiners, Coolidges, Coldens, Bayards, Gouverneurs, and Kortrights.⁵²

That the social structures of the antebellum South and North were in some important respects similar does not, of course, make them carbon copies of one another. In this as in other respects the chief difference between the sections was that one of them harbored a huge class of enslaved blacks. John C. Calhoun,

⁴⁹ Knights, *The Plain People of Boston, 1830-1860*, 76, 108-09, 115-18; Doherty, *Society and Power: Five New England Towns*, 3; Don H. Doyle, "The Social Order of a Frontier Town: Jacksonville, Illinois, 1825-1870," paper presented to the Annual Meeting of the Organization of American Historians, held in Denver, April 18, 1974, p. 2; Huffman, "Town and Country in the South," 372-73; Linden, "Economic Democracy in the Slave South," 178, 180-81; Hopkins, "Are Southern Cities Unique?" 139; Michael B. Katz *et al.*, "Migration and the Social Order in Erie County, New York, 1855," *Journal of Interdisciplinary History*, 8 (1978): 669; Goldfield, "Pursuing the American Urban Dream," 60; and Jackson, "Movin' On: Mobility through Houston in the 1850's," 251, 269, 282.

⁵⁰ Edward Pessen, "Social Mobility in American History: Some Brief Reflections," *JSH*, 45 (1979): 165-84.

⁵¹ Degler, *Place over Time*, 56.

⁵² Pease, "A Note on Patterns of Conspicuous Consumption among Seaboard Planters, 1820-1860," *JSH*, 35 (1969): 331-93; and Edward Pessen, "Philip Hone's Set: The Social World of the New York City Elite in the 'Age of Egalitarianism,'" *New-York Historical Society Quarterly*, 56 (1972): 285-308, "The Lifestyle of the Antebellum Urban Elite," *Mid-America*, 55 (1973): 163-83, and "The Marital Theory and Practice of the Antebellum Urban Elite," *New York History*, 53 (1972): 389-410.

James H. Hammond, George Fitzhugh, and other influential Southern champions of white supremacy never ceased reminding their antebellum audiences, therefore, that in the South "the two great divisions of society [were] not the rich and the poor, but white and black, and all the former, the poor as well as the rich, belong to the upper classes."⁵³ Several historians have recently agreed that great planters and small white farmers in the South shared common interests, for all the disparity in their condition.⁵⁴ The interests of the different social classes will be considered in the discussion of influence and power that follows. Whatever these interests may have been, Southern whites, rural and urban, lived as did Northerners—in a stratified society marked by great inequalities in status, material condition, and opportunity.

INFLUENCE, POWER, AND, ABOVE ALL, POLITICS in antebellum America have been the subjects of massive recent research. Most discussions of antebellum politics have stressed differences between the major parties. The literature takes on new meaning peculiarly germane to this discussion when it is recast and its focus shifted to a comparison of politics in the North and South. Politics, as Samuel Johnson once observed, often touches human beings but lightly. A recent study of antebellum North Carolina reports that its political system, which was indifferent to pressing problems, was only saved from "violent explosions" by "its own practical insignificance."⁵⁵ That people may be indifferent to the politics of their time, perhaps sensibly so, does not render politics insignificant to the historian. In retreating from history as past politics, some of us appear to have taken up a history of nonpolitics. This is silly. For how the political system works, whether for good or for ill, is as important a clue to the character of a civilization as any other.

By mid-century the American political system was everywhere formally democratic. Notorious exceptions to and limitations on democracy persisted, but they persisted in both North and South and for largely the same reasons. If blacks could not vote in the Old South, with rare exceptions neither could they vote in the Old North, where they were barred by statute, subterfuge, custom, and intimidation.⁵⁶ The South initiated the movement to limit the powers and terms of office of the judiciary and substitute popular elections for the appointment of judges. When Fletcher M. Green reminded us a generation ago that antebellum Southern states created new, and modified old, constitutions that were

⁵³ Calhoun, Speech before the U.S. Senate, as quoted in Nevins, *Ordeal of the Union*, 1: 419.

⁵⁴ Genovese, "Yeoman Farmers in a Slaveholders' Democracy," 338; Degler, *Place over Time*, 80–81; Wright, *The Political Economy of the Cotton South*, 42; and Otto H. Olsen, "Historians and the Extent of Slaveownership in the Southern United States," *Civil War History*, 18 (1972): 101–16.

⁵⁵ Harry Legare Watson II, "'Bitter Combinations of the Neighborhood': The Second American Party System in Cumberland County, North Carolina" (Ph.D. dissertation, Northwestern University, 1976), 65.

⁵⁶ The best comprehensive overview of the political and other deprivations suffered by Northern free blacks is Leon Litwack, *North of Slavery* (Chicago, 1971). Invaluable on Northern racial attitudes is George M. Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817–1914* (New York, 1971). As is still true in the twentieth century, apportionment discriminated in favor of rural over urban areas; see Michael P. Johnson, *Toward a Patriarchal Republic: The Secession of Georgia* (Baton Rouge, 1977), 87–88.

fully as democratic as those in Northern states, he concluded that by this "progressive expansion in the application of the doctrine of political equality . . . , the aristocratic planter class had been shorn of its political power." Power, he claimed, had now been transferred to "the great mass of whites."⁵⁷ As Green's critics were quick to point out, popular suffrage and theoretical rights to hold office are not synonymous with popular power.⁵⁸ Yet these are not empty or hollow rights. That they have often been made so testifies not to their insignificance but rather to the importance of the larger context in which democratic political gains are registered. It remains neither a small matter nor a small similarity that on the constitutional level the antebellum North and South were similarly democratic and republican.⁵⁹

At least as important as a society's system for selecting political officeholders is the kind of men who are regularly selected and their characteristic performance in office. In collecting evidence on political figures, scholars have sought to measure the measurable—above all, the social and economic characteristics of officeholders and party leaders. I think it safe to assume that historians performing these chores have the wit to know that an individual of whatever background is perfectly capable of transcending it. Their unspoken working assumption is one that has been known since before Aristotle: the material and social circumstances of men in power may throw some light on their motives and behavior, taking on added significance when these circumstances are uniform or close to uniform. That Charles A. Beard's mechanistic overemphasis of these points may have given them a bad name does not detract from their usefulness.

Abundant data have been accumulated on the occupations, wealth and property ownership, church affiliations, education, and other social indicators not only of antebellum officeholders in several dozen cities equally divided between South and North and in counties in every Southern state but also of state officials in all of the Southern and most of the Northern states and of Congressmen from most of the states in the Union. The resultant picture inevitably is not uniform. Humble county and town officials, for example, were less likely to be drawn from the highest levels of wealth and from the most prestigious occupations than were men who occupied more exalted state and federal positions.⁶⁰ Aldermen and councilmen usually did not match the mayor either in wealth or in family prestige. But the relatively slight social and economic differences found between men at different levels of government or between men nominated by the parties that dominated American politics from the 1830s to the

⁵⁷ Green, "Democracy in the Old South," *JSH*, 12 (1946): 3–23. Also see David Donald, "The Confederate as a Fighting Man," *ibid.*, 25 (1959): 178–93; and, for a valuable discussion of the movement to democratize and limit the powers of the judiciary, see Maxwell Bloomfield, *American Lawyers in a Changing Society* (Cambridge, Mass., 1976).

⁵⁸ In Clement Eaton's phrase, "the existence of a democratic political machinery is . . . no guarantee that democracy will prevail in the functioning of government"; *The Civilization of the Old South*, 298–99. Also see Campbell and Lowe, *Wealth and Power in Antebellum Texas*, 3, 109.

⁵⁹ For an interesting argument that stresses the importance of the political ideology of republicanism to the antebellum South, see Michael Holt, *The Political Crisis of the 1850s* (New York, 1978).

⁶⁰ See particularly the two definitive studies by Ralph A. Wooster: *The People in Power: Courthouse and Statehouse in the Lower South, 1850–1860* (Knoxville, 1969), and *Politicians, Planters, and Plain Folk: Courthouse and Statehouse in the Upper South, 1850–1860* (Knoxville, 1975).

1850s were not differences between the North and South.⁶¹ In the South as in the North, men similar in their dissimilarity to their constituencies held office and exercised behind-the-scenes influence. In contrast to the small farmers, indigents, laborers, artisans, clerks, and shopkeepers—the men of little or no property who constituted the great majority of the antebellum population—the men who held office and controlled the affairs of the major parties were everywhere lawyers, merchants, businessmen, and relatively large property owners. In the South they were inordinately men who owned slaves and owned them in unusually large numbers.⁶² It may well be that a society that is stratified economically and socially will confer leadership on those who have what Robert A. Dahl has called substantial material “advantages.”⁶³ It is not clear that this is an iron law. What is clear is that the Old South and the North awarded leadership to precisely such men.

More important than the social and economic backgrounds of political leaders are their public behavior and the ideologies or “world views” underlying this behavior. Not that the thinking or action of powerful men is totally unaffected by their material circumstances. But, in view of the complexity of any individ-

⁶¹ For a recent comparison of Whig and Democratic candidates and party leaders, see my *Jacksonian America: Society, Personality, and Politics* (rev. ed., Homewood, Ill., 1978), 235–41.

⁶² My generalizations on the social and economic standing of antebellum political leaders in the South and North are based on the following studies: William Barney, *The Secessionist Impulse: Alabama and Mississippi in 1860* (Princeton, 1974), 61, 63, 81–89; James M. Russell, “Elites and Municipal Politics and Government in Atlanta, 1847 to 1890,” in Robert C. McMath and Orville V. Burton, eds., *Southern Communities in the Nineteenth Century* (Westport, Conn., 1980); Peter D. Levine, *The Behavior of State Legislative Parties in the Jacksonian Era: New Jersey, 1829–1844* (Rutherford, N.J., 1977), 72; Watson, “‘Bitter Combinations of the Neighborhood,’” 121, 224; Burton W. Folsom II, “The Politics of Elites: Prominence and Party in Davidson County, Tennessee, 1835–1861,” *JSH*, 39 (1973): 359–78; James, *Antebellum Natchez*, 93–94; Wheeler, *To Wear a City's Crown*, 31, 88, 113; Campbell and Lowe, *Wealth and Power in Antebellum Texas*, 117, 123, *passim*; William H. Pease and Jane H. Pease, “‘Money, Class, and Party’: Charleston’s Nullification Politics, 1830–1833,” paper presented to the Ninth Annual Conference in History, held at the State University of New York, Brockport, October 1976, pp. 10, 15, 35–36; Alcorn, “Leadership and Stability in Mid-Nineteenth-Century America,” 697–98; Hurst, “The Elite Class of Newport,” 83, 84, 96, 115; Robert A. Dahl, *Who Governs? Democracy and Power in an American City* (New Haven, 1961), 62–66; Edward Pessen, “Who Governed the Nation’s Cities in the ‘Era of the Common Man?’” *Political Science Quarterly*, 87 (1972): 591–614; Michael H. Frisch, “The Community Elite and the Emergence of Urban Politics: Springfield, Massachusetts, 1840–1880,” in Thernstrom and Sennett, *Nineteenth-Century Cities*, 282, 284–85; Doherty, *Society and Power: Five New England Towns*, 40; Robert M. Ireland, *The County Courts in Antebellum Kentucky* (Lexington, Ky., 1972); Kathleen Kutolowski, “American Political Elites: A Case Study of Local Power, 1803–1860,” paper presented at the Annual Meeting of the Organization of American Historians, held in St. Louis, April 1976; Whitman H. Ridgway, “McCulloch vs. the Jacksonians: Patronage and Politics in Maryland,” *Maryland Historical Magazine*, 70 (1975): 350–62; W. Wayne Smith, “Jacksonian Democracy on the Chesapeake: Class, Kinship, and Politics,” *ibid.*, 63 (1968): 55–67; Joseph Harrison, Jr., “Oligarchs and Democrats—The Richmond Junto,” *Virginia Magazine of History and Biography*, 78 (1970): 189; De Bats, “Political Elites and the Structure of Georgia Politics”; Milton Henry, “Summary of Tennessee Representation in Congress from 1845 to 1861,” *Tennessee Historical Quarterly*, 10 (1951): 140–48; John V. Mering, *The Whig Party in Missouri* (Columbia, Mo., 1967); Edwin A. Miles, *Jacksonian Democracy in Mississippi* (Chapel Hill, 1960); Grady McWhiney, “Were the Whigs a Class Party in Alabama?” *JSH*, 23 (1957): 510–22; Thomas B. Alexander et al., “The Basis of Alabama’s Ante-Bellum Two-Party System,” *Alabama Review*, 49 (1966): 243–76; Brian G. Walton, “The Second American Party System in Arkansas, 1836–1848,” *Arkansas Historical Quarterly*, 28 (1969): 120–55; Gene W. Boyett, “Quantitative Differences between the Arkansas Whig and Democratic Parties, 1836–1850,” *ibid.*, 34 (1975): 63–66; Soltow, *Patterns of Wealthholding in Wisconsin since 1850*; and J. Mills Thornton III, *Politics and Power in a Slave Society: Alabama, 1800–1860* (Baton Rouge, 1978), 66. Many of these studies, when they do present data on wealth, do so only for officials and leaders. When they also contain evidence on the average wealth of the people, the disparity is always striking; the wealth of the leaders usually ranged from four to twenty times the wealth of the led.

⁶³ Dahl, *Who Governs?*, 85.

ual's ideology and of the diverse elements that help shape it, the effect of these circumstances cannot be assumed and is likely to vary from one individual to another. Although the political philosophies of men do not lend themselves to quantitative or precise measurement, the burden of recent scholarship is that most Southern and Northern political activists were similarly ambitious for worldly success, opportunistic, materialistic, and disinclined to disturb their societies' social arrangements.⁶⁴ Men with values such as these were ideally suited to lead the great pragmatic parties that dominated antebellum politics.⁶⁵

Many parties flashed across the American political horizon during the antebellum decades. That the Antimasonic Party, the Liberty Party, and the Free Soil Party almost entirely bypassed the South is an important difference between the sections. The South was not hospitable to organized political dissent, particularly dissent hostile to the expansion of slavery. These parties were small and ephemeral organizations whose leverage stemmed not so much from any great voting support they were able to command as from the nearly equal strength in both sections of the great major parties, the Democrats and the Whigs. Whoever would evaluate the actions of those who held executive or legislative office in antebellum America must, almost invariably, evaluate Whigs or Democrats—at least until the mid-1850s, when a new party emerged during the great controversy over the extension of slavery in the territories.

The Democrats and Whigs were national parties drawing their leaders and followers from both sections. They could usually count on intersectional support for the national tickets they presented quadrennially to the nation at large. Interestingly, the presidency—whether occupied by Southerners Jackson, Tyler, Polk, and Taylor and the Southern-born Harrison or Northerners Van Buren, Fillmore, Pierce, and Buchanan—was in the 1830s, 1840s, and 1850s in the hands of Whigs and Democrats who displayed great sensitivity toward the political and economic interests of the slave-owning South.⁶⁶ In the 1840s Congressmen voted not by region as Northerners or Southerners but primarily as Whigs and Democrats. Party rather than sectional interest prevailed in the roll calls on most issues reaching the national political agenda. In the 1850s, as Thomas B. Alexander has reported, "forces greater than party discipline . . . were evidently at work . . . , forcing party to yield to section on a definable number of issues."

⁶⁴ Pessen, *Jacksonian America*, 172–74. Political leaders are no less idiosyncratic than are other men; they have, of course, been interpreted differently elsewhere. Yet the version I have given of their operative values and the prevalence of these values among leaders in all latitudes follows the evaluations in older studies of antebellum politics and such recent biographies as Irving H. Bartlett, *Daniel Webster* (New York, 1978); Chase C. Mooney, *William H. Crawford* (Lexington, Ky., 1974); John A. Monroe, *Louis McLane: Federalist and Jacksonian* (New Brunswick, N.J., 1973); Herman J. Viola, *Thomas L. McKenny: Architect of America's Early Indian Policy* (Chicago, 1974); James C. Curtis, *Andrew Jackson and the Search for Vindication* (Boston, 1976); and Robert V. Remini, *Andrew Jackson and the Course of American Empire, 1767–1821* (New York, 1977).

⁶⁵ By pragmatic parties I mean, as do most historians and political scientists who have used the term, parties largely but not solely concerned with electoral success, parties not devoid of principles so much as parties of flexible or shifting principles.

⁶⁶ The great political influence of these interests raises questions about the recent interpretation likening the place of the South within the antebellum United States to that of weak and underdeveloped nations in the modern world order.

Yet, even in the 1850s, "both major parties maintained a high level of cohesion and intersectional comity" with regard to the range of issues not bearing on slavery and its right to expansion.⁶⁷

The great national issues of antebellum politics, culminating as they did in Sumter and the ensuing war, were of transcendent importance to Americans. A good case can nonetheless be made that local and state politics touched the lives of people more often and more directly than did national politics, particularly during an era when the men in the nation's capital were inclined to treat *laissez faire* as an article of faith.⁶⁸ State governments in North and South, by contrast, engaged in vigorous regulation of a wide range of economic activities.⁶⁹ Local governments taxed citizens and, if with limited effectiveness, sought to provide for their safety, regulate their markets and many of their business activities, look after the poor, maintain public health, improve local thoroughfares, dispose of waste, pump in water, light up the dark, and furnish some minimal cultural amenities through the exercise of powers that characteristically had been granted by state government. States chartered banks, transportation companies, and other forms of business enterprise, determined the scope of such charters, themselves engaged in business, disposed of land, and regulated local communities. The great question is how did the actual operations of local and state governments in the North and South compare during the antebellum decades.

Antebellum state government was almost invariably controlled by either Whigs or Democrats. The major parties were essentially state parties, bound together in the most loosely organized national confederations. Citizens divided not by geographical section but by party preference within each state. The parties were in all latitudes characteristically controlled by tight groups of insiders that sometimes monopolized power, sometimes shared it with rival factions, in the one case as in the other controlling nominations and conventions, hammering out policy, disseminating and publicizing the party line, organizing the faithful to support it, enforcing strict discipline, and punishing those who dared challenge either the policies or the tactics pursued by the leadership. While party policies could conceivably have been infused with the noble principles proclaimed in party rhetoric, such infusion rarely appears to have been the case. The "Albany Regency," the "Richmond Junto," the "Bourbon Dynasty" of Arkansas, and similar cliques in control elsewhere have been described as realists rather than idealists.

⁶⁷ Alexander, *Sectional Stress and Party Strength: A Study of Roll-Call Voting Behavior in the United States House of Representatives, 1836-1860* (Nashville, 1967), 110. Also see Joel H. Silbey, *The Shrine of Party: Congressional Voting Behavior, 1841-1852* (Pittsburgh, 1967); and David J. Russo, "The Major Political Issues of the Jacksonian Period and the Development of Party Loyalty in Congress, 1830-1840," *Transactions of the American Philosophical Society*, 62 (1972): 1-51.

⁶⁸ For an interesting argument on the relatively slight impact of national government, see Philip S. Paludan, "The American Civil War Considered as a Crisis in Law and Order," *AHR*, 77 (1972): 1013-34. For the argument that state legislatures were "the centers of political power," at least in the Upper South, see Wooster, *Politicians, Planters, and Plainfolk*, 27.

⁶⁹ Oscar Handlin and Mary Handlin, *Commonwealth: A Study of the Role of Government in the American Economy—Massachusetts, 1774-1861* (New York, 1947); Louis Hartz, *Economic Policy and Democratic Thought: Pennsylvania, 1776-1860* (Cambridge, Mass., 1948); Milton S. Heath, *Constructive Liberalism: The Role of the State in Economic Development in Georgia to 1860* (Cambridge, Mass., 1954); James N. Primm, *Economic Policy in the Development of a Western State: Missouri, 1820-1860* (Cambridge, Mass., 1954); and Carter Goodrich, *Government Promotion in American Canals and Railroads, 1800-1900* (New York, 1960).

To call attention to the gulf between the pronouncements and the actions of antebellum state political leaders is not to indulge in cynicism but simply to report the facts as historians have recorded and interpreted them. J. Mills Thornton's recent description of antebellum Alabama's political leaders as demagogues who felt a "secret contempt for the voters" they publicly extolled and whose "primary function was to gain as many offices as possible for the party faithful" is not unlike historians' characterizations of other leaders in other states, both in the North and in the South.⁷⁰ In New York as in Alabama, in Michigan as in Georgia, in Pennsylvania as in Mississippi, in Illinois as in Missouri, the "compelling aim" of the major parties and the groups that ran them appears to have been "to get control of the existing machinery of government" and to dispense to party loyalists the jobs that attended electoral success. While seemingly preoccupied with patronage and gerrymandering or with keeping from the agenda of state governments issues that posed a "threat to property and the social order or which threatened . . . stability,"⁷¹ the major parties did not sidestep altogether economic, social, and cultural issues of some moment. The most germane feature of roll call evidence on such issues is how little there is to choose between legislative voting patterns in the South and the North.⁷²

In towns and cities, unlike the states, party counted for little. Candidates for the mayor's office and the local council or board of aldermen did not fail to remind voters of the moral superiority of their own parties. But, as students of antebellum urban politics have noted, it mattered little whether this major party or that won the election or whether the town was located north or south of the Mason-Dixon line. True, the problems faced by cities in Texas, where "Indian fighting was probably the most important municipal activity," were unknown in the Northeast (and, for that matter, the Southeast). The amazing thing is how similar were both the problems taken up by local government everywhere and the measures enacted for coping with them.

Perhaps in no other milieu was governmental policy so permeated with class bias. Whether it was Natchez or Springfield, Charleston or Brooklyn, New Orleans or Boston, the lawyers, merchants, and large propertyowners who occupied city hall ran things in the interests of the "wealthier inhabitants." Tax rates were everywhere minuscule and property flagrantly underassessed, at the insistence of large taxpayers. Valuable lots were leased to rich men at ridiculously low rates, if not sold to them for a song. Funds provided by the niggardly bud-

⁷⁰ Thornton, *Politics and Power in a Slave Society*, 43, 95, 115, 140, 150, 246.

⁷¹ See Ronald P. Formisano, *The Birth of Mass Political Parties: Michigan, 1827-1861* (Princeton, 1971), 42-43, 55; Watson, "Bitter Combinations of the Neighborhood"; Ridgway, "The Decline of the Post-Revolutionary Establishment," and "McCulloch vs. the Jacksonians"; Shugg, *Origins of Class Struggle in Louisiana*, 127, 155-56; and Rodney O. Davis, "Partisanship in Jacksonian State Politics: Party Division in the Illinois Legislature," in Robert P. Swierenga, ed., *Quantification in American History* (New York, 1970), 149-62. Also see the studies cited in note 62, above.

⁷² Herbert Ershkowitz and William G. Shade have examined roll calls in New Hampshire, Pennsylvania, Ohio, New Jersey, Virginia, and Missouri between 1833 and 1843; see their "Consensus or Conflict? Political Behavior in the State Legislatures during the Jacksonian Era," *JAH*, 58 (1971): 591-621. For an informed criticism of this essay and an effective argument that "partisan voting behavior on selected roll calls" does not indicate the existence of "contrasting belief systems," see Levine, *The Behavior of State Legislative Parties in the Jacksonian Era*, 16, 201, 206, 232.

gets typical of the time were spent most freely to improve or widen streets used by businessmen rather than to clean streets in the neighborhoods of the poor. Improved public facilities for disposing of waste or carrying fresh water into the city were usually introduced first in upper-class residential districts. The "indisputable connection between the policies of the city council and the interests of the wealthier inhabitants" that Richard Wade discerned in Cincinnati early in the era could be found in most other cities.

A contemporary New Yorker attributed to corruption the not atypical favoritism the city showed its propertied elements, observing that "nearly every alderman has in some degree owed his success to the personal efforts and influence of 'backers,' who must be recompensed for their services."⁷³ In the absence of evidence that local officeholders were so motivated, it is more reasonable to assume that they acted out of an honest conviction that the prosperity of the larger community depended in the first instance on the prosperity of its wealthiest inhabitants. That such beliefs were colored by the material advantages of those who possessed them, as by the conservative social values typically absorbed by men of their standing, seems equally reasonable. In any case, the pattern of uncommonly prosperous propertyowners controlling localities in the interests of men and families similarly situated was not confined to one geographical section.⁷⁴

Power is not, of course, confined to control of government.⁷⁵ Control over banks, credit, capital, communications, and voluntary associations, which in an era of laissez faire often exercised more influence than did public authorities over education and culture, crime and punishment, social welfare and poverty, gave to those who had it a power that was barely matched by those who held the reins of government. The burden of recent research is that small social and economic elites exercised a degree of control over the most important institutions in the antebellum North that bears close resemblance to the great power attributed to the great planter-slaveowners by William E. Dodd a half century ago and by Eugene D. Genovese more recently. Influential voluntary associa-

⁷³ [William A. Brewer] *A Few Thoughts for Taxpayers and Voters* (New York, 1853), 74.

⁷⁴ Full documentation of this point would present a catalogue of almost all of the studies of antebellum localities that I have already cited. For these two paragraphs I have primarily drawn up Wheeler, *To Wear a City's Crown*; draft typescript of Pease and Pease's comparative study of antebellum Charleston and Boston (which I have used with the kind permission of the authors); James, *Antebellum Natchez*; Frisch, *Town into City*; David R. Goldfield, "The Business of Health Planning: Disease Prevention in the Old South," *JSH*, 42 (1976): 557-70; Wade, *The Urban Frontier*, 209; and Hurst, "The Elite Class of Newport," 92-94.

⁷⁵ There is a vast theoretical literature on power. Classic discussions include Max Weber, *The Theory of Social and Economic Organization*, ed. Talcott Parsons (London, 1964), 152, and *From Max Weber: Essays in Sociology*, ed. H. H. Gerth and C. W. Mills (London, 1947), 180; Talcott Parsons, "Some Reflections on the Place of Force in Social Processes," in Harry Eckstein, ed., *Internal War* (New York, 1964), 57; and Carl J. Friedrich, *Man and His Government: An Empirical Theory of Politics* (New York, 1963), 199-200. In a stimulating recent paper, Bertram Wyatt-Brown has argued that Southern custom afforded nonslaveowning whites and small slaveowners an informal power to get away with a great deal in the way of personal violent behavior, even to the extent of destroying property, in exchange for accepting the fundamental social order of the antebellum South; Wyatt-Brown, "The Yeomanry and the Issue of Justice: Southern Social Structure and the 'Unanimity of Violence,'" paper presented at the Annual Meeting of the Organization of American Historians, held in New Orleans, April 12, 1979. William Faulkner made a similar point about the postbellum South in several of his writings.

tions and financial institutions appear to have been run by similarly atypical sorts on both sides of the Mason-Dixon line.⁷⁶

Shortly after secession, Governor Joseph E. Brown told the Georgia legislature that in the South the "whole social system is one of perfect homogeneity of interest, where every class is interested in sustaining the interest of every other class." Numerous Southerners agreed with him, and many scholars concur. In their failure to challenge planter supremacy, small farmers—slaveowners and nonslaveowners alike—ostensibly demonstrated the unique identity of interest that was said to bind all whites together in the antebellum South.⁷⁷ The interest of a group is a normative term, known only to God (and perhaps to Rousseau in his capacity as authority on the General Will), in contrast to its perceived interests, as stated in its words and implicit in its actions. There are, therefore, as many interpretations of the "true interests" of Southern—or, for that matter, of Northern—small farmers as there are historians writing on the subject. The South's large enslaved black population doubtless affected the perceptions of all Southern whites, if in complex and unmeasurable ways. Recent research indicates that poorer and nonslave-owning Southern whites were, nevertheless, sensitive enough to their own social and economic deprivation to oppose their social superiors on secession and other important matters.⁷⁸ Whether the acquiescence of the mass of antebellum Northerners in their inferior social and economic condition was in their own interest will be decided differently by conservative, reformist, and radical historians. Our admittedly insubstantial evidence on the issue suggests that the degree of social harmony coexisting with subtle underlying social tensions was, racial matters apart, not much different in the North and the South.

⁷⁶ Dodd, *The Cotton Kingdom: A Chronicle of the Old South* (New Haven, 1921); and Genovese, *The Political Economy of Slavery*, and *The World the Slaveholders Made*. Also see Doherty, *Society and Power: Five New England Towns*; David J. Rothman, *The Discovery of the Asylum: Social Order and Disorder in the New Republic* (Boston 1971); John H. Ellis, "Businessmen and Public Health in the Urban South during the Nineteenth Century: New Orleans, Memphis, and Atlanta," *Bulletin of the History of Medicine*, 44 (1970): 197–212; Gail O'Brien, "Power and Influence in Mecklenburg County, 1850–1880," *North Carolina Historical Review*, 54 (1977): 120–44; John Duffy, "Nineteenth-Century Public Health in New York and New Orleans: A Comparison," *Louisiana History*, 15 (1974): 325–37; M. J. Heale, "From City Fathers to Social Critics: Humanitarianism and Government in New York, 1790–1860," *JAH*, 63 (1976): 21–41; and Walter S. Glazer, "Participation and Power: Voluntary Associations and the Functional Organization of Cincinnati in 1840," *Historical Methods Newsletter*, 5 (1972): 151–68. For a study of the distribution of diverse forms of power in one community, see Edward Pessen, "Who Has Power in the Democratic Capitalistic Community? Reflections on Antebellum New York City," *New York History*, 58 (1977): 129–56.

⁷⁷ Brown, as quoted in Johnson, *Toward a Patriarchal Republic*, 42. Also see Owsley, *Plain Folk of the Old South*, 134; Craven, *The Coming of the Civil War*, 32; Olsen, "Historians and the Extent of Slaveownership in the Southern United States," 101–16; Genovese, "Yeoman Farmers in a Slaveholders' Democracy," 332, 338; and Barney, *The Road to Secession*, xiii–xiv, and *The Secessionist Impulse*, 3, 187. Although he has rejected Genovese's argument of planter hegemony, Degler has accepted Olsen's thesis that small and large Southern farmers had interests that were "at least parallel, not antagonistic"; *Place over Time*, 77, 80.

⁷⁸ Johnson, *Toward a Patriarchal Republic*, xx–xxxi, 33–34, 65–66; Paul D. Escott, "Southern Yeomen and the Confederacy," *South Atlantic Quarterly*, 77 (1978): 146–47, "An Irrepressible Conflict within the South? Class Influences on the Balloting for Secession," paper presented at the Annual Meeting of the Organization of American Historians, held in New Orleans, April 12, 1979, pp. 1, 2, 7, 14–15, and *After Secession: Jefferson Davis and the Failure of Confederate Nationalism* (Baton Rouge, 1978), 21–32; Roark, *Masters without Slaves*, xi, 2, 21–22, 42–48, 55; and Wright, *The Political Economy of the Cotton South*, 42, 70.

LIMITATIONS OF SPACE PERMIT no more than a swift allusion to a number of other matters that are fascinating either because, like religiosity and values, they are intangible or, like crime and violence, they resist precise measurement.⁷⁹ Scholars of a revisionist bent can have a field day with these themes, for the growing literature on them yields tantalizing evidence that appears to overturn the traditional view of a distinctive antebellum South. Legal briefs can thus be written attesting the near similarity of the South and North in their achievements in science, medicine, public health, and other aspects of intellectual and cultural life, in their ideals of womanhood, in their racial attitudes, in their violence and attitudes toward violence, in their materialism as in other values, in aspects of humanitarian reform, and in religion, particularly in the roles played by evangelicalism and the Benevolent Empire in the Protestant denominations that were predominant in both sections.⁸⁰ Much of this literature implicitly pro-

⁷⁹ Michael Stephen Hindus recently pointed out that statistics deal only with reported crime, not with the "dark figure" or "crimes which will never be known to the historian"; Hindus, "The Contours of Crime and Justice in Massachusetts and South Carolina, 1767-1878," *American Journal of Legal History*, 21 (1977): 214. Or, as David J. Bodenhamer has said, "No one can determine the true amount of crime in any society"; Bodenhamer, "Law and Lawlessness in the Deep South: The Local Response to Antebellum Crime," paper presented at the Annual Meeting of the Organization of American Historians, held in New Orleans, April 12, 1979, p. 6.

⁸⁰ The following is a sampling of the burgeoning literature on these themes: Watt L. Black, "Education in the South from 1820 to 1860 with Emphasis on the Growth of Teacher Education," *Louisiana Studies*, 12 (1973): 617-29; Laura D. S. Harrell, "The Development of the Lyceum Movement in Mississippi prior to 1860," *Journal of Mississippi History*, 31 (1969): 187-201; Michael B. Katz, *The Irony of Early School Reform* (Cambridge, Mass., 1968); Stanley K. Schultz, *The Culture Factory: Boston Public Schools, 1789-1860* (New York, 1973); Robert C. Reinders, "New England Influences on the Formation of Public Schools in New Orleans," *JSH*, 30 (1964): 181-95; Doyle, "The Social Order of a Frontier Town," 17; William Stanton, *The Leopard's Spots: Scientific Attitudes toward Race in America, 1815-1859* (Chicago, 1966), 13; John Duffy, "Medical Practice in the Antebellum South," *JSH*, 25 (1959): 53-72, and *A History of Public Health in New York City, 1625-1866* (New York, 1968); James O. Bredeen, "Body Snatchers and Anatomy Professors: Medical Education in Nineteenth-Century Virginia," *Virginia Magazine of History and Biography*, 83 (1975): 321-45, and "Thomsonianism in Virginia," *ibid.*, 82 (1974): 150-80; Gerald Grob, *Mental Institutions in America: Social Policy to 1875* (New York, 1973); Norman Dain, *Disordered Minds: The First Century of Eastern State Hospital in Williamsburg, Virginia, 1766-1866* (Williamsburg, 1971); Goldfield, "The Business of Health Planning: Disease Prevention in the Old South," 557-70; Philip English Mackey, "Edward Livingston and the Origins of the Movement to Abolish Capital Punishment in America," *Louisiana History*, 16 (1975): 145-66; Gordon E. Finnie, "The Antislavery Movement in the Upper South," *JSH*, 35 (1969): 319-42; Lawrence J. Friedman, "Purifying the White Man's Country: The American Colonization Society Reconsidered," *Societas*, 6 (1976): 1-24; Barbara Welter, *Dimity Convictions: The American Woman in the Nineteenth Century* (Athens, Ohio, 1976); John C. Ruoff, "Frivolity to Consumption: or, Southern Womanhood in Antebellum Literature," *Civil War History*, 18 (1972): 213-29; Ronald W. Hogeland, "The Female Appendage: Feminine Life-Styles in America, 1820-1860," *ibid.*, 17 (1971): 101-14; Kathryn L. Seidel, "The Southern Belle as an Antebellum Ideal," *Southern Quarterly*, 15 (1977): 387-401; Litwack, *North of Slavery*; Ira Berlin, *Slaves without Masters: The Free Negro in the Antebellum South* (New York, 1974), and "The Structure of the Free Negro Caste in the Antebellum United States," *Journal of Social History*, 9 (1976): 297-318; John L. Stanley, "Majority Tyranny in Tocqueville's America: The Failure of Negro Suffrage in 1846," *Political Science Quarterly*, 84 (1969): 412-35; William D. Miller, "Myth and New South City Murder Rates," *Mississippi Quarterly*, 26 (1973): 143-53; Guy A. Cardwell, "The Duel in the Old South: Crux of a Concept," *South Atlantic Quarterly*, 66 (1967): 50-69; Richard M. Brown, "Historical Patterns of Violence," in Hugh D. Graham and Ted R. Gurr, eds., *Violence in America: Historical and Comparative Perspectives* (New York, 1969), 50; Leonard L. Richards, "Gentlemen of Property and Standing"; David Grimsted, "Rioting in Its Jacksonian Setting," *AHR*, 77 (1972): 361-97; Robert E. May, "Dixie's Martial Image: A Continuing Historiographical Enigma," *Historian* 40 (1978): 213-34; Bodenhamer, "Law and Lawlessness in the Deep South," 11, 18-19; Parton Yoder, "Private Hospitality in the South," *Mississippi Valley Historical Review*, 47 (1960): 419-33; Bertram Wyatt-Brown, "The Antimission Movement in the Jacksonian South: A Study in Regional Folk Culture," *JSH*, 36 (1970): 501-29; Timothy F. Reilly, "Parson Clapp of New Orleans: Antebellum Social Critic," *Louisiana History*, 16 (1975): 167-91; John Jentz, "A Note on Genovese's Account of the Slaves' Religion," *Civil War History*, 23 (1977): 161-69; Stuart M. Blumin, "Church and Community: A Case Study of Lay Leadership in Nineteenth-Century America," *New York History*, 56 (1975): 393-408; and Michael S. Franch, "The Congregational Com-



Figure 3: A typical antebellum market scene: this engraving by J. R. Brown depicts a fruit market in Pittsburgh. Reproduced courtesy of the Bettman Archive, Inc., New York.

motes the concept of sectional convergence either by upgrading Southern or by downgrading Northern achievements. But, since historians—unlike embattled attorneys—cannot content themselves with evidence that is both insubstantial and contradicted by evidence pointing in an opposite direction, they are best advised to reserve judgment.⁸¹ Wisdom consists in re-examining and re-evaluating the earlier literature on these themes, weighing carefully the merits of the recent contributions, and, above all, in probing for additional evidence.

HAVING EXAMINED economic developments, social structure, and politics and power in the antebellum sections, let me now return to the question of capitalism in the Old South. Several historians have recently argued that Southern planters constituted a “seigneurial” class presiding over a “pseudocapitalistic”

munity in the Changing City, 1840–1870,” *Maryland Historical Magazine*, 71 (1976): 367–80. In addition, Bell Irwin Wiley has adduced substantial evidence on the similarity of the values of young Southerners and Northerners in uniform; see his *The Life of Johnny Reb: The Common Soldier of the Confederacy* (Indianapolis, 1943) and *The Life of Billy Yank: The Common Soldier of the Union* (Indianapolis, 1952).

⁸¹ For unusually interesting if not always convincing recent essays emphasizing sectional disparities with regard to these themes, see Raleigh A. Suarez, “Chronicle of a Failure: Public Education in Antebellum Louisiana,” *Louisiana History*, 12 (1971): 109–22; Duffy, “Nineteenth-Century Public Health in New York and New Orleans”; Donald G. Mathews, “Religion in the Old South: Speculation on Methodology,” *South Atlantic Quarterly*, 73 (1974): 34–52; Sheldon Hackney, “Southern Violence,” *AHR*, 74 (1968–69): 906–25; John Shelton Reed, “To Live—and Die—in Dixie: A Contribution to the Study of Southern Violence,” *Political Science Quarterly*, 86 (1971): 429–43; and Wyatt-Brown, “The Yeomanry and Issues of Justice.”

society, a class whose "world view" ostensibly set them "apart from the mainstream of capitalist civilization." By this analysis, the Old South, though influenced by modern capitalism, belonged (as do early modern India and Saudi Arabia, among others) to the category of "premodern" societies that have been the economic and political dependencies of the dynamic industrial world that exploits them. The antebellum South's banking, commercial, and credit institutions did not in this view manifest the section's own capitalistic development so much as they served to facilitate the South's exploitation by the "capitalistic world market." This argument can be accepted uncritically only by accepting Eugene D. Genovese, Barrington Moore, Jr., and Raimondi Luraghi as the arbiters and interpreters of what represents "every normal feature of capitalism."⁸²

Capitalism is not a rigid system governed by uniform economic practices, let alone inflexible definitions. The economy of the antebellum United States, like capitalistic economies in Victorian England and other nations, was composed of diverse elements, each playing a part in a geographical and functional division of labor within the larger society. As Lewis C. Gray and Thomas P. Govan long ago and other scholars more recently have observed, Southern planters had the attitudes and goals and were guided by the classic practices of capitalistic businessmen.⁸³ The antiurbanism and antimaterialism that Genovese has attributed to the great planters is unconvincing because thinly documented and contradicted by much other evidence.⁸⁴ Some people, including planters themselves, may have likened the planter class to a seigneurial aristocracy. Unlike the lords of the textbook manor, however, Southern planters depended heavily on outside trade, participated enthusiastically in a money economy, and sought

⁸² Genovese, *The Political Economy of Slavery*, 1-3, 19, 23, and *The World the Slaveholders Made*, 33, 130-31; Moore, *Social Origins of Dictatorship and Democracy*, 121; and Luraghi, *The Rise and Fall of the Plantation South*, 8. For an excellent criticism of the enormities in Luraghi's work, see Bennett H. Wall's review essay, "The Myth of the Planter Past," *Plantation Society*, 1 (1979): 273-80. Space requirements dictate that I lump together the views of Genovese, Moore, and Luraghi that do not always coincide and that, in Genovese's case to his credit, are being modified and refined in each new essay he publishes.

⁸³ Gray, *History of Agriculture in the Southern United States*, 1: 301-02; Govan, "Was the Old South Different?" 448; Bateman and Weiss, "Manufacturing in the Antebellum South," 11-13; Tony Freyer, "Law in Antebellum Maryland and Southern Character," paper presented at the Annual Meeting of the Organization of American Historians, held in New Orleans, April 12, 1979, p. 2; and John R. Killick, "The Cotton Operations of Alexander Brown and Sons in the Deep South, 1820-1860," *JSH*, 43 (1977): 169. In a quite recent essay, Shearer Davis Bowman has suggested the compatibility of the conservative social and political stance of planters and Junkers with "entrepreneurial, profit-oriented economic behavior"; Bowman, "Antebellum Planters and *Vornürz* Junkers in Comparative Perspective," *AHR*, 85 (1980): 779-808.

⁸⁴ Genovese's assertion that slaveholders "distrusted the city and saw in it something incongruous with their local power and status arrangements" is documented by a single piece of secondary evidence; *The Political Economy of Slavery*, 24. For numerous testimonies to the alacrity of large planters in establishing town residences for themselves, their evident delight in doing so, and the great influence they enjoyed in cities, see Huffman, "Town and Country in the South," 366-86; and Dorsett and Shaffer, "Was the Antebellum South Anti-urban?" 93-100. Equally unpersuasive are Genovese's assertions that white Southerners were "dreadfully repressed . . . in their sexual mores," ostensibly harboring "unconscious wishes about mother or sister or something equivalent," and that "many travellers" thought Southern values distinctive from Northern; *The Political Economy of Slavery*, 28-30, and *The World the Slaveholders Made*, 96, 146. Genovese based the latter observation on uncited comments by Tocqueville and Achille Murat. In view of the many dozens of visitors who found that most of the important American values commanded national, not sectional, obeisance, it is hard to disagree with Degler's contention that the South's "system of values" was "quite congruent with [that of] the rest of the country"; *Place over Time*, 68. For an effective refutation of the notion that the Creole elite were beyond greed and grasping, indifferent to money, and contemptuous of those who lived to amass it, see Joseph G. Tregle, Jr., "Early New Orleans Society: A Reappraisal," *JSH*, 18 (1952): 20-36.

continuously to expand their operations and their capital. Marx once said that the limits of the serf's exploitation were determined by the walls of the lord's stomach.⁸⁵ The limits of the slave's exploitation were determined by the expanding walls of the world cotton market.

That slavery is not the classic labor system associated with a Marxist definition of capitalism is, of course, true. The problem with Marx as Pundit of capitalism, for all the undeniable brilliance of his interpretation, is that he was, as he conceded, more interested in changing the system than in explaining it. Those of us content with merely understanding so complex a phenomenon as capitalism know that, whether in its labor system or in other respects, it is a flexible and constantly shifting order, susceptible of diverse definitions. The Southern economy did differ in important respects from the Northern, developing special interests of its own. Yet, far from being in any sense members of a colony or dependency of the North, the Southern upper classes enjoyed close ties with the Northern capitalists who were, in a sense, their business partners. The South was an integral component of a wealthy and dynamic national economy, no part of which conformed perfectly to a textbook definition of pure capitalism. In part because of the central place in that economy of its great export crop, cotton, the South from the 1820s to the 1860s exerted a degree of influence over the nation's domestic and foreign policies that was barely equalled by the antebellum North. India within the Empire indeed! The South's political system of republicanism and limited democracy, like its hierarchical social structure, conformed closely to the prevailing arrangements in the North, as they also did to the classic features of a capitalistic order.

The striking similarities of the two antebellum sections of the nation neither erase their equally striking dissimilarities nor detract from the significance of these dissimilarities. Whether in climate, diet, work habits, uses of leisure, speech and diction, health and disease, mood, habits, ideals, self-image, or labor systems, profound differences separated the antebellum North and South. One suspects that antebellum Americans regarded these matters as the vital stuff of life. The point need not be labored that a society, one-third of whose members were slaves (and slaves of a distinctive "race"), is most unlike a society of free men and women. An essay focusing on these rather than on the themes emphasized here would highlight the vital disparities between the antebellum South and North. And yet the striking dissimilarities of the two antebellum sections do not erase their equally striking similarities, nor do they detract from the significance of these similarities.

The antebellum North and South were far more alike than the conventional scholarly wisdom has led us to believe. Beguiled by the charming version of Northern society and politics composed by Tocqueville, the young Marx, and other influential antebellum commentators, historians have until recently believed that the Northern social structure was far more egalitarian and offered far

⁸⁵ My source for this epigram is my class notes for a graduate course in medieval economic history that I took thirty years ago at Columbia University. Fortunately, the authority for the citation is the reliable and admirable Karl Helleiner, my teacher.

greater opportunity for upward social movement than did its Southern counterpart and that white men of humble position had far more power in the Old North than they did in the Old South.⁸⁶ In disclosing that the reality of the antebellum North fell far short of the egalitarian ideal, modern studies of social structure sharply narrow the gulf between the antebellum North and South.⁸⁷ Without being replicas of one another, both sections were relatively rich, powerful, aggressive, and assertive communities, socially stratified and governed by equally—and disconcertingly—oligarchic internal arrangements. That they were drawn into the most terrible of all American wars may have been due, as is often the case when great powers fight, as much to their similarities as to their differences. The war owed more, I believe, to the inevitably opposed but similarly selfish interests—or perceived interests—of North and South than to differences in their cultures and institutions.

It is a commonplace in the history of international politics that nations and societies quite similar to one another in their political, social, and economic arrangements have nevertheless gone to war, while nations profoundly different from one another in their laws of property or their fundamental moral and philosophical beliefs have managed to remain at peace.⁸⁸ The Peloponnesian War, which, like the American Civil War, was a bitter and protracted struggle between two branches of the same people whose societies were in vital respects dissimilar from one another, appears to have owed little to these differences. In Thucydides' great account, Athens and the Athenians were profoundly unlike Sparta and the Lacedaemonians, whether in "national" character, wealth, economic life, ideals and values, system of justice, attitudes toward freedom, or lifestyle. But to Thucydides, as to the leading spokesmen for the two sides, these dissimilarities were one thing, the causes of the war quite another. Athens and Sparta fell out primarily because both were great imperial powers. "The real cause of the war," concluded Thucydides, "was formally . . . kept out of sight. The growth of the power of Athens and the alarm which this inspired in Lacedaemon, made war inevitable."⁸⁹ None of this is to say that sectional differences had no influence whatever on the actions of those influential men that in

⁸⁶ For the influence of Tocqueville's "egalitarian thesis," see Pessen, *Riches, Class, and Power before the Civil War*, chap. 1. In the early 1840s Marx believed that the conquest by Northern workers of the right to vote represented the "political emancipation" of the working classes—a necessary first step to full emancipation. And, influenced by his reading of Thomas Hamilton's *Men and Manners in America* (1833), Marx also believed that class lines were regularly brushed aside in the Northern states. See Karl Marx, *Early Writings*, pt. 1 (New York, 1963), particularly "Bruno Bauer, *Die Judenfrage*," in *The Jewish Question*, 12. Eric Foner has asserted that the Republican Party ideology, emphasizing the great social and economic opportunities available to Northern labor, was given "plausibility" and "a strong cultural authenticity" by the alleged closeness of the facts of Northern life to these claims made by Republican spokesmen; *Free Soil, Free Labor, Free Men*, 33–34. Foner's conclusions appeared just prior to the publication of a number of empirical studies that cast grave doubt on the accuracy of Republican propaganda—at least in this respect.

⁸⁷ For a discussion and cataloguing of these studies, see Edward Pessen, "On a Recent Cliometric Attempt to Resurrect the Myth of Antebellum Egalitarianism," *Social Science History*, 3 (1979): 208–27.

⁸⁸ Hans J. Morgenthau, *Politics among Nations: The Struggle for Power and Peace* (2d ed., New York, 1954). Also see Potter, *The South and Sectional Conflict*, 76–78; and Randall, "The Blundering Generation," 3–28.

⁸⁹ Since I am using the causes of the Peloponnesian War only as a suggestive model for my brief consideration of the causes of the American Civil War, I trust I shall be forgiven for referring only to one study of that earlier war. My generalizations and quotations are drawn from Thucydides, *The Peloponnesian War*, trans. Richard Crawley (New York, 1951), 15, 33, 40, 46–49, 50, 65–67, 80–81, 104–06, 118, 189, 253, 440, 509.

April 1861 culminated in the outbreak of the American Civil War. The point rather is that, insofar as the Peloponnesian War throws any light whatever on the matter, wars between strikingly dissimilar antagonists break out not necessarily because of their differences, important as these are, but because of their equally significant similarities.

Late in the Civil War, William King of Cobb County, Georgia, reported that invading Union officers had told him, "We are one people, [with] the same language, habits, and religion, and ought to be one people."⁹⁰ The officers might have added that on the spiritual plane Southerners shared with Northerners many ideals and aspirations and had contributed heavily to those historical experiences the memory and symbols of which tie a people together as a nation. For all of their distinctiveness, the Old South and North were complementary elements in an American society that was everywhere primarily rural, capitalistic, materialistic, and socially stratified, racially, ethnically, and religiously heterogeneous, and stridently chauvinistic and expansionist—a society whose practice fell far short of, when it was not totally in conflict with, its lofty theory.

⁹⁰ Roark, *Masters without Slaves*, 222.

AHR Forum
Antebellum North and South in Comparative Perspective:
A Discussion

THOMAS B. ALEXANDER and STANLEY L. ENGERMAN
FORREST MCDONALD, GRADY MCWHINEY, and EDWARD PESSEN

EDWARD PESSEN HAS PROVIDED ANOTHER of his very useful surveys of literature relevant to an important theme, this time a comparison of antebellum North and South, to suggest that the two sections "were far more alike than the conventional scholarly wisdom has led us to believe" (page 1147). Forrest McDonald and Grady McWhiney have offered a sectional comparison only by implication—a "lazy" South versus an industrious North. Both "How Different from Each Other Were the Antebellum North and South?" and "The South from Self-Sufficiency to Peonage: An Interpretation," although quite different in approach, attack fundamental and long-held assumptions about the character of Southern life and the nature of Southern society in the years before the Civil War.

PROFESSOR PESSEN SUGGESTS the need for an "overarching synthesis" for the antebellum North and South and offers a "modest hope" that his discussion "will be useful to historians in pursuit of such a synthesis" (page 1119). We shall assuredly find useful this focus on an impressive range of recent work and the relating of pieces of evidence "to one another and to earlier findings and interpretations" (page 1119). The author does not claim to have any one overarching synthesis in his sights and concedes that others would emphasize different aspects of comparison. His way of trying to be useful to historians in this instance is by comparing North and South in terms of the dominant theme of his extensive work as "naysayer" to the egalitarian "myth" in American history.

Professor Pessen has long held that wealth is the best single indicator of social class and of power, that wealth in the antebellum United States was very badly distributed, that highly undesirable class distinctions were embedded in the sys-

Editor's note: All references indicated parenthetically in this article refer to the essays in this issue by Professors McDonald and McWhiney, pages 1095–1118, above, and Professor Pessen, pages 1119–49, above, or to other comments within this discussion.

tem, and that an image of extensive economic and social mobility is unjustified.¹ Not unexpectedly, then, he here examines the economies, the social structures, and politics and power in the two sections. Scrupulously acknowledging the "striking dissimilarities," he concludes that, "without being replicas of one another," both North and South were "relatively rich, powerful, aggressive, and assertive communities, socially stratified and governed by equally—and disconcertingly—oligarchic internal arrangements" (pages 1147–48). Thus, "for all of their distinctiveness, the Old South and North were complementary elements in an American society . . . whose practice fell far short of, when it was not totally in conflict with, its lofty theory" (page 1149).

Professor Pessen is entitled to his judgments about the centrality of these bases of comparison, hedged about as they are with perceptive caveats. My most pervasive concern arises not from his choice of theme but from the quality and coverage of existing studies for illuminating the economic and social life of that large majority of the people who lived outside even small towns. I do not think as highly of these studies as Pessen does. I acknowledge that critical bibliographical commentary throughout the paper would have been impractical and that using studies for one purpose does not entail endorsing any of them for different considerations. The projection of a lucid and coherent image, nevertheless, I respectfully submit, has left a residual impression of greater underpinning for that image than I can accept. Pessen has focused much of his work on the people of towns and cities, and to my mind he has taken his questions and perceptions farther into the countryside than the sources justify. He claims, for example, not only that "in rural as well as urban communities, in large cities and small, and on both sides of the Mason-Dixon line, armies of footloose Americans were on the move, following trails never dreamed of in the Turner thesis," but also that these "armies" consisted "primarily" of "the poorer and propertyless" (page 1135). Granted, fully "one-half of the residents . . . left those communities from one decade to another" (page 1135). But I simply do not believe that we can possibly know that they were mostly the poorer and propertyless rural Americans—at least not at this stage of our work. The problem of rural intergenerational economic mobility, either associated with or apart from geographical mobility, moreover, has hardly been touched and may be beyond reach. And, for the substantial proportion of rural nonfarm population for whom real income is elusive, we just do not yet have adequate evidence that wealth was "the surest sign of social, as well as of economic, position" (page 1130), unless only great wealth is meant.

In this brief comment I can refer to no more than a pair of examples of sources less persuasive than might be inferred. Professor Pessen cites several times Lee Soltow's *Men and Wealth in the United States, 1850–1870* (1975) and Gavin Wright's *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (1978), and I think that these books are of consid-

¹ Some of Edward Pessen's own work is cited in notes to his essay. I have especially found useful one article he did not cite: "Who Rules America? Power and Politics in the Democratic Era, 1825–1975," *Prologue*, 9 (1977): 5–26.

erable importance to the theme of inequality of wealth in Pessen's essay. As Pessen certainly knows but for good reason could not spare the space to discuss, the mixed reviews that these works received suggest caution. One reviewer of Wright's volume offered some high praise but also cited "deformities"—including "the construction of heavy deductive structures on factual quicksands."² Soltow's *Men and Wealth* drew sharp criticism from Pessen himself, although not necessarily on matters pertaining to this essay.³ Soltow's work is based principally on a spin sample from manuscript censuses of 1850, 1860, and 1870 that must average about five families per county per census. However suitable the sample was for making generalizations about the total population, it was not intended to provide illumination of the county-level context within which the distribution of wealth may have had significance apart from the macrocosmic array. Most of Wright's research is based on the Parker-Gallman sample of about five thousand farms from approximately four hundred Southern counties—fewer than one-half of the region's counties. Since Wright limited his selection to the cotton belt, his work is less useful to those who would make region-wide generalizations. I find both of these books valuable. My purpose here is to illustrate my concern that Professor Pessen appears to be more optimistic about the stage of our knowledge than are many of us who are mired in intractable and contradictory sources.

One small positive suggestion about comparing sections is that the population density of those eligible to hold wealth and power—that is, the white population—may provide some insights. In 1860, the Northern county median in population density had about thirty-two persons per square mile; 95 percent of the Southern counties were lower in density of white residents. Apart from the extreme frontier states of 1860, the median county in every Southern state had a white population density below that of the median county of any Northern state (except that Maryland was tied with Illinois). Such relative dispersion of Southern whites may well have resulted in important sectional distinctions in economy, social structure, or concentrations of power. One very modest illustration is provided by a study of 361 marriages in five Alabama counties, in which Nancy C. Roberson sought to identify patterns of wealth and slaveholding in the families of marriage partners.⁴ Her findings justify a cautious assumption that, where suitable partners were far between, propinquity sometimes modified considerations of wealth and slaveholding. Even cultural diffusion from towns or cities involved far greater distances for Southerners because of the location of the region's cities on its periphery.

Professors McDonald and McWhiney have concentrated only on the South "from self-sufficiency to peonage." Their interpretation is intriguing and offered with verve and fervor. They are already aware that most of their novel propositions meet with more incredulity than any brief essay could possibly overcome.

² Robert Higgs, Review of Gavin Wright's *The Political Economy of the Cotton South* in the *Journal of American History*, 66 (1979): 153.

³ Pessen, Review of Lee Soltow's *Men and Wealth in the United States, 1850-1870* in *Reviews in American History*, 4 (1976): 222-29.

⁴ Roberson, "Social Mobility in Ante-Bellum Alabama," *Alabama Review*, 13 (1960): 135-45.

Since they present numerous statements that could be made credible only by massive documentation, I think a very big book will be needed to bring their interpretation to the stage where useful confrontation on defined and delimited questions may be possible.⁵

A few brief remarks are, nevertheless, in order. The authors' conclusions that "Celtic" describes "a single general cultural group, different from the English" (page 1108 note 31) will continue to be challenged. Their extensive claims for the persistence of "cultural preadaptation" will not be readily acceptable to those who have studied the extent of adaptation to environment among differing ethnic elements.⁶ The special "preadaptation" to open-range swine economy will probably be viewed as not very special by students of other contributors to Southern herding patterns.⁷ The *reductio-ad-absurdum* computations on the labor needed to produce Southern crops has been duplicated for Northern crops.⁷ Even the undeniable decline in swine production after the Civil War, I think, will need to be accounted for in more complex terms.

Such a brief comment as this on so wide-ranging an interpretation may be doomed to triviality, and for that I apologize to my friends, the authors. I do wish to raise some questions about antebellum marketing of swine, which I believe to be an important element in their interpretation. My examination of the 1860 census data suggests that swine per capita in Southern counties provided principally for local slaughter and consumption; only a modest proportion of the swine were driven long distances. For the South as a whole, the figure for swine per capita is below two. In each of the ten Southern states a few counties had less than one per person, several counties had more than three, and the median county was at or slightly above the regional level. A median county with one per capita characterized four other Southern states. Intrastate or other nearby sale should usually have been possible. Long drives undoubtedly did occur, but the greatest number of swine in one year for any single trail, according to Professors McDonald and McWhiney, is one hundred and fifty thousand, less than 1 percent of the swine of the region. The impact of fencing on swine marketing as well as the extent of its effect on Southerners is essentially related to the proportion of market activity that required long drives. The actual timing of effective fencing activity is another crucial element that remains to be clarified, together with reasons for the beginning of the decline in the number of swine. Between 1850 and 1860, the number of swine in the South was already leveling off, and the greatest proportional decrease in numbers between 1860 and 1870 did not occur as much in those regions where armies camped or campaigned as in those counties with a high percentage of slaves in the total population in

⁵ For an interesting individual-level analysis that explicitly rejects the concept of preadaptation, see James T. Lemon, *The Best Poor Man's Country: A Geographical Study of Early Southeastern Pennsylvania* (Baltimore, 1972).

⁶ For a commentary on how natural open-range livestock grazing seemed to early South Carolina settlers and on the familiarity of some of the black people with African grazing activity, see Peter H. Wood, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974).

⁷ William N. Parker reported his similar computations for the North in commenting on a version of this essay presented at the Annual Meeting of the Organization of American Historians, held in New Orleans, April 1979.

1860. Quite possibly, the increase in people free to eat high on the hog reduced the attractiveness of untended grazing.⁸

I HAVE NOT GRAPPLED WITH MOST of the many propositions offered by Professors McDonald and McWhiney, for I have found their essay to be a porcupine. I promise the authors that I shall peruse the big book when it emerges, even buy it, but never review it. In his essay, as in many other parts of his work, Professor Pessen undertakes to digest stunningly extensive bodies of work. One of the things we sorely miss in U.S. history is an ongoing effort to bring some synthesis, however preliminary, out of the glut of individual items. Some of the social sciences do better, but then perhaps they focus more narrowly. Those of us who teach antebellum sectionalism courses are, this time, indebted for a stimulating and challenging addition to required reading lists.

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⁸ For comment on the drastic penalty for the "surreptitious feast in the forest with which servants sometimes indulged themselves," see Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 217. I do not share the view sometimes expressed that the 1870 census for the South is too flawed to trust for general trends.



BOTH PAPERS—"How Different from Each Other Were the Antebellum North and South?" and "The South from Self-Sufficiency to Peonage: An Interpretation"—discuss a long-standing historical question, the similarities and differences between the Northern and Southern states before the Civil War.¹ Forrest McDonald and Grady McWhiney also discuss a related question, the impact of the Civil War upon Southern society. A major reason for the antebellum comparison has been the interest in the origins of the Civil War: was it an irreconcilable conflict between two different societies and cultures? As Edward Pessen suggests, the onset of a war, even a civil war, need not imply antagonists with two different ways of life. McDonald and McWhiney indicate, however, that sectional comparisons have had a long history; examples, usually emphasizing sectional differences, are to be found in the seventeenth, eighteenth, and nineteenth centuries. Both articles follow earlier traditions by focusing on the behavior of whites and on white society, culture, and political life.

¹ Since both papers contain rather extensive references to the relevant literature, I shall cite only those works that are the basis for specific points. Space constraints preclude responding to many of the controversial issues raised.

There is a crucial issue posed by Professor Pessen's title, one that confronts much of historical writing, and is particularly critical in the study of social and cultural history. Granted that between two groups (sections)—groups that themselves contain a wide range of variation—there are some similarities and dissimilarities, how can a simple answer to Pessen's type of question be arrived at? Which of the many characteristics are to be regarded as crucial? How can similarities and differences be described (even when not measured)? How can the importance of any set of differences or similarities be determined? Clearly, there will be some similarities and differences, and the interpretation of the nature and importance of such differences and similarities will vary with the specific question studied. All societies are diverse, and in comparing any two it will be possible for comparisons to emphasize either similarities or differences. And, even if we conclude that in only one out of the multitude of characteristics did two societies differ, that one—if important enough—might justify an emphasis upon difference, not similarity. There remains, of course, the further question of the factors that gave rise to the differences between societies (or caused the similarities) and the period of time in which they became important.

PROFESSORS MCDONALD AND MCWHINEY EMPHASIZE what they perceive as an important North-South difference: a "lazy" South versus a hard-working North. They thus present a familiar view of sectional differences. They also follow a long historiographic tradition by attributing these differences to the characteristics of the original settlers. Where they differ is in their emphasis on the role of the Celtic element, not the more frequently discussed Cavaliers. The authors are in some disagreement with those who, while agreeing that there were significant sectional differences prior to the Civil War, argue that such differences were due to the impact of climate and other factors that led to the development of a slave-based society in one of the sections. In this argument, slavery and a slave society produced more important differences between sections, despite the similarities of their settlers—a contention obviously contrary to that of McDonald and McWhiney.²

Their view of one component group in the South—able to obtain an acceptable standard of living with limited production of export staples, relying on livestock raised in a less expensive manner than in the North, and, possibly, working at a more relaxed pace than farmers in the North—seems plausible. But projection of such behavior onto the entire South produces a rather curious result. It becomes difficult to understand the dynamics of the emergence, expansion, and relatively widespread ownership of slave labor producing staple com-

² There remain problems in relating all of the key features of Southern society to the Celtic element. Not only were there different patterns among the Celts, but, since many of the settlers in the South were not Celts, the characteristics of other groups (white and black) remain of importance. Given the early patterns of market production in the Chesapeake and South Carolina, moreover, it may be that the behavior of even the settling Celts varied with geographic and economic conditions. The Southern uplands were particularly dominated by Celts, as McDonald and McWhiney point out (page 1108 note 31), so that their depiction seems most applicable to this region.

modities for sale in national and international markets; the dynamics of the shift from tobacco to cotton in the late eighteenth and early nineteenth centuries; the westward expansion of *commercial* agriculture; the commercial features of Southern society, with financial and transportation developments sponsored by state governments; and the great wealth acquired by Southern slaveowners. Clearly, however accurate their portrayal of small, upland farmers, there is more to the South than what Professors McDonald and McWhiney convey—whether these other features were due to atypical Celts or to misplaced English.

They seem to argue that most everyone in the South was lazy or, at least, did not find it necessary to work long or hard. This characteristic is attributed not only to slaveowners and other whites but even to the slaves. In discussing the nature of the work input, Professors McDonald and McWhiney distinguish between the length of the work day and the number of hours actually at work as well as between the hours worked during peak seasons and other times of the year.³ More needs to be said, however, on the critical distinction between hours of work and intensity of work—a point at issue in recent discussions of the slave economy.⁴ Further, the estimates presented of hours worked in the South and the North-South comparisons of hours worked have major problems. The estimates they use of time spent on corn and cotton production are based upon national averages, for which the details of calculation are not known. The more recent, systematic estimates prepared under the direction of William N. Parker contain detailed regional breakdowns that suggest that McDonald and McWhiney severely understate the time spent on corn production in the South. While Parker's average labor input into cotton production does not differ markedly from the figure used by McDonald and McWhiney, his estimate of the labor time needed to produce a bushel of corn is more than twice that which they use.⁵ (The explanation of the significant sectional differences between Northern and Southern productivity in corn production was itself a frequently discussed question.⁶)

To compare estimates of total labor input derived from measures of crop output per hour with estimates of hours worked derived from time-budget studies

³ In several cases the distinctions had been made by those whom McDonald and McWhiney criticize. Thus, as they note, both Lewis C. Gray and Eugene D. Genovese have distinguished between the length of the work day and the hours actually worked; see note 17 (page 1110), above. The estimates of Robert W. Fogel and Stanley L. Engerman refer only to "peak labor periods," not to the entire year.

⁴ John F. Olson has drawn this distinction between hours of work and intensity of work; see his "Clock-Time vs. Real-Time: A Comparison of the Lengths of the Northern and Southern Agricultural Work-Years" (unpublished essay). Also see note 14 (page 1101), above.

⁵ See Parker and Judith L. V. Klein, "Productivity Growth in Grain Production in the United States, 1840-60 and 1900-10," in Conference on Research in Income and Wealth, *Output, Employment, and Productivity in the United States after 1800* (New York, 1966), 523-80; and Parker, "Labor Productivity in Cotton Farming: The History of a Research," *Agricultural History*, 53 (1979): 228-44. The former provides estimates for labor time in corn, the latter provides estimates for cotton and, in addition, contains citations to the estimates of labor time in land clearing (by Martin Primack) and in dairy farming (by Fred Bateman). Because of the differences in productivity between small farms and plantations, there may be some bias in the application of Parker's estimates to both free and slave workers. (This problem also affects the use of the estimates employed by McDonald and McWhiney.)

⁶ For a recent statement, see William N. Parker, "A Note on Regional Culture in the Corn Harvest," *Agricultural History*, 46 (1972): 181-89. Sectional differences in corn output per acre and per worker were a source of comment in the antebellum years.

can be misleading. The crop-time method that Professors McDonald and McWhiney use for the South leads to a serious underestimation of the actual labor time in agriculture, for reasons not altogether clear, in both sections. Robert E. Gallman's estimates of labor time in antebellum agriculture, also based upon estimates of crop output per labor hour, account for about as few hours as do McDonald and McWhiney.⁷ Why such dramatic differences exist, even when it is thought that appropriate allowances are made for other uses of labor, is worth more attention, but the comparisons presented cannot be used to argue for North-South differences in labor input of the sort McDonald and McWhiney seek to establish.

Despite these criticisms of their empirical measures, as well as of their extending their contentions to the entire South, Professors McDonald and McWhiney do highlight an important aspect of antebellum Southern society and the changes following from the Civil War. There is no space here to discuss their claims as to the culture of the Celts, the question of whether the planter class (whoever they were) actually increased their political power after losing the war, and why, after a thousand years of struggle, these Celts were finally defeated. McDonald and McWhiney do, however, stress the importance of non-slaveholding whites in the antebellum South, who, with relatively limited production of staple crops (though a larger percentage produced cotton than McDonald and McWhiney suggest [page 1106 note 26]) and mast feeding of livestock, obtained adequate consumption with (possibly) more leisure or easier work routines than did Northern farmers. At issue remain two major transitions. There was a sharp increase in cotton output from small, nonslave farms in the 1850s, with the extension of railroads within the South. In the postbellum period there was a marked shift of whites, particularly in older areas of the South, into cotton production, with market involvement (and tenantry) apparently increasing with the decline of the plantation.⁸ These responses focus attention on the determination of the extent to which the antebellum smaller farms had "voluntarily" limited market involvement and the extent to which they had been "forced out" by the greater efficiency of the antebellum cotton plantations that used slave labor. As McDonald and McWhiney point out, the Civil War did have a marked impact on the small white Southern farmer (as it did on planters and slaves). There was a shift from livestock and corn production in the aftermath of the Civil War's destruction of livestock. As measured by farm output, however, the small white farms were less affected than were the antebellum plantations, the decline of which led to a marked reduction in output per black person and an expansion of sharecropping.

⁷ See Gallman, "The Agricultural Sector and the Pace of Economic Growth: U.S. Experience in the Nineteenth Century," in David C. Klingaman and Richard K. Vedder, eds., *Essays in Nineteenth-Century Economic History: The Old Northwest* (Athens, Ohio, 1975), 35-76. The estimates of hours worked in agriculture derived from labor requirements in crops and livestock for the years between 1909 and 1936 similarly fall short of the estimate of annual hours worked derived from direct estimates; see John A. Hopkins, *Changing Technology and Employment in Agriculture* (Washington, 1941), 146-48. Although McDonald and McWhiney attempt some adjustment for omitted output, the basic difficulty for sectional comparison persists.

⁸ For a longer discussion of these issues, see Stanley L. Engerman, "Economic Aspects of the Adjustments to Emancipation in the United States and the British West Indies" (forthcoming).

Professors McDonald and McWhiney make a number of related arguments about the antebellum South. Several of these are in obvious disagreement with those of Professor Pessen and will be touched on below. I have concentrated on one of their points, but it is one that they have made central to their presentation of North-South differences. On that issue, however accurate their analysis of certain groups, it cannot be applied to all of the whites and to the black slaves who lived and worked in the South.

PROFESSOR PESSEN ARGUES for the importance of certain similarities between the antebellum North and South. While he focuses on three major structural features, he notes other characteristics in which sectional similarities were present, as well as a number of characteristics for which significant dissimilarities did exist. His general point, however, is that "the antebellum North and South were far more alike than the conventional scholarly wisdom has led us to believe" (page 1147), and the thrust of his article is to emphasize similarities, not differences.

The basis of Professor Pessen's argument is the considerable body of new data and empirical information—both quantitative and nonquantitative—that have been generated by many recent studies. These studies, of both North and South, have served to provide material for useful correctives to some earlier views.⁹ To the extent that Pessen draws attention to this accumulating information, his essay provides a useful service. It is in his interpretations of some of this work and in his attempts to draw specific conclusions and decide various ongoing debates that problems arise. However useful the new evidence summarized, Pessen's discussion does not deal with a number of key interpretive issues, nor does it satisfactorily confront the argument that even striking similarities in some characteristics cannot adequately account for the patterns of change in the antebellum sections. Because of the style of Pessen's presentation, drawing on the work of others, with attacks on those historians he (for whatever reason) disagrees with, it becomes difficult to discern an overall view of the period. Thus, it remains uncertain whether emphasis on similarities or on differences is more critical for understanding the antebellum period or even for describing the onset of the Civil War.¹⁰

The three similarities stressed by Professor Pessen are the economies, the social structures, and the ways of politics and power. Both sections are seen to have had antebellum economies that were booming, productive, and flexible,

⁹ In part Pessen draws upon works claiming that scholars have misperceived the South, and in part he relies on work (including his own) arguing that scholars have misread the North. Germane to the methodological issue is one of focus; although some of these reinterpretations are based explicitly on the comparative method, in many cases they are based upon separate analysis in only one of the sections. Nevertheless, the major shift in recent work has been away from the claims that the Southern social structure, wealth distribution, and political life were markedly different (if not unique), particularly in comparison with the North (and Western Europe).

¹⁰ Thus, for example, it might be claimed that there was a basic similarity in motivation—economic, political, or otherwise—for westward expansion in both sections. Yet the reasons why there was an inability to achieve any permanent compromise may point to the importance of differences (particularly those due to the differing regional concentrations of the black population and their enslavement). Nevertheless, it could be argued that it was the similarities in motivation and belief that made the conflict inevitable, not any differences in sectional cultures.

with individuals striving to acquire wealth. (Pessen apparently finds, in the South, few of the yeoman farmers discussed by Professors McDonald and McWhiney. Based upon motivation, these, rather than slaveowners, might represent an important difference between North and South. At the least, Pessen denies that these yeomen had the political, social, and economic power that McDonald and McWhiney attribute to them.) The distribution of wealth within white society is seen to be roughly similar in both sections, particularly in urban areas.¹¹ Both regions are seen to have been similarly "hierarchical," with some vertical, and extensive geographic, mobility. And both sections had formal "republicanism" and "limited democracy," with, Pessen argues, the political leadership drawn mainly from the wealthy.

While numerous discussions persist about aspects of these depictions of each section, not to mention their comparisons, an important debate has been in progress about the meaning of these measured similarities. Pessen points to, but does not emphasize, certain differences, possibly crucial in the antebellum period, possibly of potential subsequent importance. Underlying these differences is, of course, the role of slavery in Southern society—the differences between one section with a population over one-third black, mostly enslaved, and most productively used in plantation agriculture, and the other with less than 2 percent of the population black and relatively few enslaved. However similar the motivations of planter capitalists and industrial capitalists, however efficiently each section followed its comparative advantage, and however rapidly both sections were growing economically, one section included a slave-based agriculture and the other had, in addition to a commercial agriculture based on family farms, a developing industrial sector based upon wage labor.¹² One section was more influenced by planter-slaveowners, the other more by merchant and industrial capitalists. These features affected the structure of society and led, for example (as Pessen notes), to certain restrictions on what was politically acceptable. Thus, beneath the structural similarities and some important similarities in motivation, behavior, and belief, there remained key differences in desired policies and in the sources of wealth. These differences, even with some basic similarities in belief and behavior, in conjunction with the importance of attitudes toward race and slavery, had obvious implications for national political and social life. Yet there is still little agreement on how all of these interacted to bring about an intersectional war, nor is there agreement on which of the similarities and differences are central to understanding antebellum life.

AS THESE ARTICLES SHOW, North-South comparisons for the antebellum period remain a lively source of controversy. Professors McDonald and McWhiney

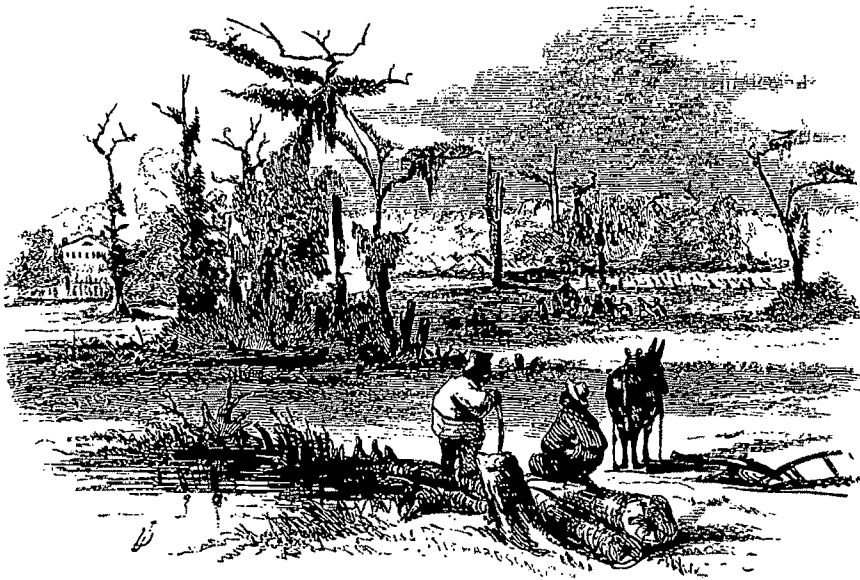
¹¹ Note, however, that if Southern yeomen were not financial-income-maximizers, as McDonald and McWhiney claim here, and, if some farmers and laborers in the North behaved similarly, the interpretation of the measured wealth distributions would be different from what is suggested.

¹² It is unnecessary to debate here whether or not the South was "capitalist," since much semantic confusion besets the issue (some pointing to motivation as the crucial aspect, others to available opportunities) and since, for many questions of historical interest, no determination is necessary. Rather than trying to answer that question, it seems best to restrict the discussion to the various underlying characteristics that are involved in its answer.

present a new twist on older views that emphasize sectional differences, while Professor Pessen draws together fresh evidence noting structural similarities whose importance had not been fully appreciated. Although not included in this discussion, the article by Carville Earle and Ronald Hoffman,¹³ which deals with a similar question, is perhaps the most revisionist of the writings on the subject, inverting many views of antebellum North-South differences in economic performance. While I disagree with parts of their analysis, as I do with some aspects of the two discussed, these three articles are symptomatic of many studies of economic, political, social, intellectual, demographic, and cultural aspects of the antebellum period. Many of the relatively clear-cut comparisons presented in the past have given way to views of considerably greater complexity in describing intra-, as well as inter-, sectional similarities and differences. The new knowledge generated has provided the basis for new interpretations of white (and black) life in the antebellum years and has contributed to a clarification and sharpening of the earlier debates.

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¹³ Earle and Hoffman, "The Foundation of the Modern Economy: Agriculture and the Costs of Labor in the United States and England, 1800-60," *AHR*, pages 1054-94, above.



WE FIND LESS TO DISAGREE WITH in the comments of our critics than in the article by Edward Pessen. But we shall confine our observations concerning his paper to a letter to the editor to be published in the communications section of the February issue, where he will have the opportunity to respond. Thomas B. Alexander and Stanley L. Engerman have offered—both in the remarks published here and in private—useful objections, suggestions, questions, and demurrers; for that wholesome spirit of scholarly give and take we are appreciative.

BOTH COMMENTATORS HAVE OBSERVATIONS AND RESERVATIONS regarding our estimates of how much Southerners worked. Those estimates, as we indicate, are "only" some "rough" calculations (page 1099) that bear out the observations of contemporaries. Had we used William N. Parker's higher estimates for the labor required to grow corn, the overall results would have changed little. We have also conducted other tests, which we did not report. Both of us have grown corn without using modern implements, and we had ample time left for scholarly activities. One of us tested the work involved in the "task system," which was used on many plantations. He physically performed typical tasks as recorded in plantation records; and, though he was fifty-two years old, no task reckoned as a day's work took him more than three hours. As for farm work in addition to that required in raising crops, there is as much or as little to do as one is disposed to do. A compulsive planter like James Mallory tried to keep his hands busy, but that he was atypical is attested by the run-down condition of most Southern farms. There is another consideration: given their climate and ways of doing things, Southerners could neglect chores and still fare well. Northern farmers could not. If they failed to build and maintain fences and barns or to chop a winter's supply of wood, they were likely to be in serious trouble.

Both of our critics believe that the Old South was more heterogeneous, more commercially oriented, and further along on the road to modernization than we do. Professor Engerman comments, for instance, on the "commercial features of Southern society" (page 1156). The footnotes in Morton J. Horwitz's *The Transformation of American Law, 1789-1860* (1977) are instructive here. They reveal that the transformation from a legal system based upon the concept of holding property for enjoyment to one geared to commercial development simply did not take place in the South—for the most part the new law of contracts, negotiable instruments, and so on, was a Northern phenomenon. Engerman's statement about Southern "financial and transportation developments sponsored by state governments" (page 1156) is misleading. Nearly every Southern state had made one or more attempts at banking by 1860, but most had been failures. The Bank of Kentucky, for example, failed in 1814 as a result of mismanagement; the state established forty-six new banks in 1818, and most soon failed. North Carolina's state banks were so poorly managed and so disastrous to the state's fiscal system that the legislature liquidated them in 1828. The Mississippi state bank failed in 1830; a new one was authorized, but it failed in 1839 and liability for its bonds fell on the state. The story was much the same in Louisiana, Alabama, Georgia, Tennessee, and Arkansas. Texas outlawed banks with a constitutional provision that "no corporate body shall hereafter be created, renewed, or extended, with banking or discounting privileges."

Southern ventures into state-backed entrepreneurial activity were quite characteristically Celtic: in keeping with the "indolent unless roused" Scottish syndrome that Thomas Pennant described in his *Tour of Scotland, 1769* (5th ed., 1790), Southerners were given to bursts of enthusiastic activity that petered out for lack of the kind of persistent application that was characteristic of Englishmen and Yankees. Other aspects of state systems of law and public policy like-

wise substantiate our analysis of antebellum Southern social and economic life. The fencing laws are an obvious case in point. So, too, are the tax laws. What was taxed in the South was land, slaves, horses, carriages—the property of the planters. What was not taxed was swine—the principal property of the plain folk. If planters committed to work and to profit-maximization had dominated Southern life, surely the laws would have been different. Revealing in this regard are Peter J. Coleman's findings in his excellent study, *Debtors and Creditors in America: Insolvency, Imprisonment for Debt, and Bankruptcy, 1607–1900* (1974). Development of impersonal bankruptcy laws was necessary to the regularization and modernization of commercial relationships, and such laws evolved in the Northern states during the first half of the nineteenth century. Most Southern states, by contrast, clung to older debtor-relief systems throughout the century—with the significant exception that livestock was generally exempted from seizure for debts.

Commercial activity in the Old South was conducted mainly by outsiders. Most of its successful merchants were Yankees or foreigners. Hinton Rowan Helper noted that Southerners bought everything from shoe strings to fish hooks from Yankees. And Thomas P. Kettell concluded that by 1860 the South had an unfavorable trade balance with the North of more than \$200 million. It was largely Northern capital and initiative that provided such transportation facilities as the South had, and those were relatively primitive. Traveling the 1,460 miles from Baltimore to New Orleans in 1850, for example, meant riding on five different railroads, two stage coaches, and two steamboats. Even that apostle of Southern independence, *DeBow's Review*, was printed in the North. An English visitor in New Orleans, looking for a guide book to the city, was amazed to learn from a bookseller “that we must wait until he received some more copies from New York, for it appears that the printing even of books of local interest is done by presses 2000 miles distant.”

Several of Professor Alexander's comments require brief notice. His skepticism regarding Celtic culture would, we suggest, be dissipated if he read the works of the brilliant sociologist Michael Hechter. Alexander misreads us when he insists that all but a small percentage of the South's hogs were consumed locally. We cannot share his confidence in the census figures on Southern livestock. The number of animals in open country such as that in northern Illinois, where they were fenced and easily counted, may be fairly accurately reported in the census, but enumerators were unlikely to have made a correct count of the Southern animals who roamed the woods and cane breaks. We also find more correlation between the decline in the number of hogs in areas of the South where “armies camped or campaigned” than does Alexander (page 1153). Only in Southern states with relatively small black populations and where there had been little fighting during the Civil War were there significant gains in the number of hogs between 1860 and 1880. As for Alexander's point about pre-adaptation and swine raising, he cites an unfortunate example. Peter H. Wood's study, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (1974), in fact offers excellent confirmation of the principles of pre-adaptation and cultural conservatism: the slaves in early South Carolina had

been herdsmen in Africa and served well as herdsmen in America, but, when their owners tried to make them raise crops, they rose in bloody rebellion.

WE HAVE NOT DEALT WITH ALL of the questions and objections raised. Most of them would be easy enough to handle, space permitting, but there are others for which we do not yet have answers. We are still searching. We are working on the "very big book" Professor Alexander recommends. But in the nature of the subject—the heritage and culture of ordinary white Southerners—it will not always be possible to deal in the "defined and delimited questions" that he would like to see (page 1153). Such questions require "hard" sources, and most of the primary materials shed light mainly on people who were not part of the inarticulate plain folk. We have tried here to utilize those sources; but our main reliance is and must be upon common sense and upon a "soft" source, the one kind that deals directly with the plain folk—the observations of contemporaries. And, we insist, what we see is what they saw.

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STANLEY L. ENGERMAN finds a problem in my "attacks on those historians" that I "for whatever reason" disagree with (page 1158). Like the children in Bemelmans's charming story *Madeleine*, I smile at the good and frown at the bad. I am alert to error and particularly sensitive to it when it is committed by the great and the influential. Even the best of us are imperfect—Tocqueville in his disdain for mere facts, Marx in his sometime readiness to subordinate the facts to a noble social purpose, lesser mortals among us (from Rochester and elsewhere) in any number of ways that I believe it worthwhile to report. Like others who delight in dishing out needful criticism, I take no great pleasure in myself receiving it. I can only count my blessings in having as formal critics scholars as judicious and fairminded as Engerman and Thomas B. Alexander (as recently I have been similarly fortunate in my public exchange of views with the gracious Richard P. McCormick and Robert E. Gallman). Professor Engerman is the kind of gentleman, who, discovering in my essay a glaring factual error (I had inaccurately transcribed from an illegible note), privately called it to my attention so that I could correct it. And an author can only pray for a critic as sensitive to the nuances in his every argument as is Professor Alexander, who was kind enough to call to the attention of readers one of the few of my essays that some-

how I had overlooked citing in the footnotes. Having said this, let me now leap into the fray.

FIRST, SOME SMALL THINGS. In writing that I apparently find in the antebellum South "few . . . yeoman farmers" (page 1159), Professor Engerman has failed to note my agreement with Frank L. Owsley that "the most typical white Southerners by far were small farmers" (page 1128). That Owsley may have been wrong on some things does not mean that he was wrong on all things. According to Engerman I suggest that a "civil war need not imply antagonists with two different ways of life" (page 1154). My actual point is that the outbreak of civil war between antagonists with different ways of life may owe relatively little to these differences. And I must demur slightly from Professor Alexander's explanations of my convictions concerning the state of antebellum American society and why I chose the particular themes I did for discussion. Perhaps the citizen in me does find antebellum wealth *badly* distributed and its class distinctions *undesirable*, if inevitable. The scholar in me simply finds that wealth and class in the antebellum United States were not what many contemporaries and later scholars believed them to have been. I chose my themes, as I try to indicate, not because they are more important than other features of antebellum life but because in my judgment, based on my reading of the literature, they have to date been the most definitively researched. While I may feel at home with economic, social, and political evidence, the larger study, of which this essay is a part, is equally concerned with dozens of other matters. And working hours, the pursuit of other subjects, and the state of my golf game permitting, I hope in the future to return to that larger study.

Professor Alexander's chief concern is with the quality and quantity of the evidence underlying my portrait of antebellum rural economic and social life. Having noted in this essay and elsewhere that germane evidence on social mobility and social structure is in "pitifully short supply," lighting up only a small corner of the antebellum social landscape, I share his feeling that we do not know nearly as much as we need to know. The data for this city slicker's rural generalizations are nevertheless more substantial than his comments may suggest. Interestingly, while he wonders about the universality of some of Gavin Wright's generalizations, resting as they do largely on evidence from four hundred Southern counties, Alexander is himself ready to draw "a cautious assumption" about Southern rural marital patterns from a study of five Alabama counties.¹ That Wright's "very valuable" book may have received "mixed reviews" does not detract from the authority of its findings about the distribution of wealth in cotton counties. The rural South was, of course, more than cotton counties. As my essay points out, the work of many other scholars, most notably Albert Niemi, Jr., Randolph B. Campbell, and Richard G. Lowe, in addition to

¹ The author of that study is quite aware of the need for "further and more extensive samplings in other counties" before we can speak of the influence of "economic status" on rural marriage patterns; see Nancy C. Roberson, "Social Mobility in Ante-Bellum Alabama," *Alabama Review*, 13 (1960): 144-45. I am indebted to Professor Alexander for calling to my attention this interesting article by his then seminar student.

Lee Soltow, indicates a high degree of similarity between cotton and other types of rural Southern counties in their patterns of wealth distribution. I have indeed been sharply critical of Soltow's *Men and Wealth in the United States, 1850-1870* (1975) but almost entirely for its dismal manner of telling. Although I am not unaware of its methodological lapses, I believe now, as I did four years ago when I reviewed it, that "for all [of its] shortcomings, its chief findings emerge unsailable in their essential dimensions, if not in all their particulars."² It is the work of a painstaking, scrupulously fair, and refreshingly ideology-free scholar who, in accord with Heywood Broun's old dictum, does not class-angle his statistics.

Professor Alexander does not think that our evidence on rural society sustains my observation that America's large transient population was primarily "the poorer and propertyless" elements in their communities (page 1135). I wish, too, that we had more and better data. Yet what we do have is hardly valueless. As Fabian Linden pointed out a generation ago, Herbert Weaver's evidence on antebellum rural Mississippi reveals, when broken down, that nonslaveholders fled at a greater rate than did slaveholders and that the fewer the slaves a family owned the more likely it was to depart from its community.³ It would be surprising indeed if the significant correlations that scholars of both antebellum sections have found in urban communities large and small between "persisters" and relative economic well being does not obtain too for other rural counties in addition to those of antebellum Mississippi. Professor Alexander may well be right in his fear that definitive evidence on rural economic mobility "may be beyond reach" of scholars. If I am optimistic, it is not so much about the quality of what I agree is to a large extent "intractable and contradictory" data (page 1152) but rather about future research that "will yet disclose that . . . , during what was a period of economic expansion . . . , significant numbers of Americans improved their lot, even if modestly" (page 1135), in the countryside as well as in towns and cities. The extant evidence reveals no important sectional disparities in mobility, whether vertical or geographical. Given the subjectivity of all definitions of social class or position, Professor Alexander is, of course, free to omit from his definition wealth—which, he appears, in his comment, to equate with income. I continue to agree with Lawrence Stone and that army of modern scholars who believe wealth to be the best single clue to class position in modern Western society and with old Norris Hundley and young Gavin Wright that wealth is a particularly apposite social indicator for all antebellum milieus. I agree with Alexander that the "relative dispersion of Southern whites may well have resulted in important sectional distinctions" (page 1152). I would go even further. It is *certain* that this dispersion had some effect. The question is, Did it have the particular effect he is prepared to attribute to it? The author of the source he cites concluded not that the Southern social structure was unique or devoid of "class lines" but only that these lines may not have been "rigidly ob-

² Edward Pessen, "The Distribution of Wealth in the Era of the Civil War," *Reviews in American History*, 4 (1976): 227.

³ Linden, "Economic Democracy in the Slave South: An Appraisal of Some Recent Views," *Journal of Negro History*, 31 (1946): 178. Also see William L. Barney, *The Road to Secession: A New Perspective on the Old South* (New York, 1972), 7; and Herbert Weaver, *Mississippi Farmers, 1850-1860* (Nashville, 1945).

served" by some marriage partners.⁴ The burden of the evidence is that, for all the greater dispersion of its smaller population, the antebellum South's distribution of wealth, status, and power was strikingly similar to the distribution of these resources in the North.

Professor Engerman finds that my "discussion does not deal with a number of key interpretive issues" (page 1158). Whatever these may be, I am sure he is right. I do leave out any number of issues—key, interpretive, and every other sort. He is no doubt right, too, in feeling that "striking similarities in some characteristics cannot adequately account for the pattern of change in the antebellum sections" (page 1158). Change, let alone pattern of change, is a tricky and complex matter that historians devote volumes to explaining, never satisfactorily. And he is right again in observing that I do not emphasize sectional differences and that "it becomes difficult to discern an overall view of the period" in my essay (page 1158). Since I have attempted to present not a comprehensive overview or synthesis but rather an argument for the importance of several, often overlooked, sectional similarities, I cheerfully plead guilty to the charge that I have neither said nor tried to say the last word. My purpose is to help keep the discussion going by suggesting a new perception of some of the important issues.

BELL IRWIN WILEY had an unparalleled mastery of the minds and the values of the young men who fought for the Blue and the Gray. In an informal conversation I had with him (at one of Ben Wall's great SHA parties) shortly before Wiley died, he told me that, if the thousands of letters written home by the youngsters on both sides were thrown in the air and then fell to earth with all identifying characteristics removed, it would be impossible to know which were written by Rebs, which by Yanks. Neither Wiley nor any other sensible person would thereby have concluded that the antebellum South and North were altogether similar to or more like than unlike one another. Slavery, to cite the most dramatic and, no doubt, the most important difference between the sections, touched every aspect of Southern life, thought, and feeling, shaping a unique civilization below the Potomac. I have not sought to deny either the significance or the distinctiveness of the "peculiar institution" or any other feature of antebellum life that may have been confined to one latitude. I have sought rather to show that, important differences notwithstanding, the Old South and the Old North were in significant respects strikingly similar and complementary elements in a powerful and dynamic nation that was the creation of, and whose spirit and character were steadily nourished by, both of the great antebellum sections—the North and the South.

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⁴ Roberson, "Social Mobility in Ante-Bellum Alabama," 144–45. Roberson conceded that her evidence on marriages between nonslaveholding and "large slaveholding families" is "inconclusive." She also reported that there may have been a "greater tendency to stratification in . . . [an] older and richer [rural] community"; *ibid.*, 142.

Reviews of Books

GENERAL

HUGH THOMAS. *A History of the World*. New York: Harper and Row. 1979. Pp. xix, 700. \$17.95.

This book has many fine qualities. It is a history of the world: in tracing his themes through successive historical epochs, Hugh Thomas is careful to note what was happening during each epoch on each continent in every major culture. The author, moreover, has read widely and well in the best general surveys (for example, Bernard Lewis's *The Arabs in History* and Joseph Needham's *Science and Civilisation in China*) and in a number of outstanding monographs. And he has read critically, subjecting each statement in his authorities to the power of an intelligent, critical mind and recognizing the right of the reader to be informed by running footnotes of the source of each major assertion. To be sure, specialists will raise their eyebrows at the dubiousness or, better, the rapidity of some particular interpretation (for example, of feudalism), but they will also recognize that the author has done his reading, knows the controversial historiographical issues, and in nearly every instance has reached a plausible, defensible decision.

In presenting his history, Thomas has invented a novel organization of his material that yields fresh insights. The book divides easily into four parts: man as hunter, until approximately 10,000 B.C. (8 percent of the pages); the age of agriculture, until 1450 A.D. (20 percent); a transition period of economic and intellectual transformation in the West, to 1750 (11 percent); and then the age of the machines, considered in two segments entitled "Industrial Triumphs" (33 percent) and "Political Failures" (28 percent). Three-fifths of the work is on the years since 1750. Under each of the four parts the chapters are organized by themes or subjects: for example, under the age of agriculture, there are chapters on "Population: Famine and Food," "Agricultural Techniques," "Landlords and Laborers," "Agricultural Cooperation," "Cloths and Clothes,"

"An Early History of the Soul," "Universal Religion," and "Law, Government, and Liberty." Each chapter traces the experience of each major culture (Chinese, Hindu, and so forth) with the theme of the chapter during the centuries of the given age.

The fourth virtue of the book is that it is fun to read. Thomas writes with sustained gusto, and there is not a single dull sentence in the 618 pages. He conveys a sense of the abundance of life.

The book is nevertheless subject to two reservations. First, Thomas writes as a free-born Briton who unashamedly, unabashedly, and proudly believes in liberty, government by law, and a free-wheeling capitalism. His bias is so delightfully open as to be inoffensive, but again and again it affects his explanation of the rise and fall of economies, societies, and political regimes. Second, his mode of organization yields fresh insights, but it prevents the author from placing together in the context of a single culture the various elements of that culture. If, for example, in the age of agriculture we have to read about Chinese food, agricultural techniques, landlords and laborers, agricultural cooperation, and other topics in nine separate chapters, we can never see Chinese culture moving synthetically and contextually over time. Hence we can never explain and understand that culture and its movement. And this reservation applies to the history of all the cultures that the book discusses.

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ROBERT H. CANARY and HENRY KOZICKI, editors. *The Writing of History: Literary Form and Historical Understanding*. Madison: University of Wisconsin Press. 1978. Pp. xv, 165. \$17.50.

This provocative, perceptive collection of five essays, assembled by the editors of *Clio*, is part of an ambitious program to locate the very idea of history somewhere within the matrix of its narrative presentation. The common thread that ties these essays

together is the belief that a historical masterpiece is necessarily a literary artifact; that it is the iconology underlying the narrative form of a historian's presentation, and not the authenticity of source material, that plays the crucial role in creating the appearance of mirroring historical reality; that the object of the narrative artifact (as of any work of art) is not to represent the past but somehow to "signify" it; and finally that the true mission of the historian is not consummated in drawing correct inferences from historical evidence but in producing literary works of art. Thus conceived, Clio's handmaiden is not the social scientist but the literary critic.

The opening essay by Lionel Gossman, a literary critic, is an erudite exposition of the historical connections that have always existed between history and literature, with special attention paid to developments in France. Kieran Egan, another literary critic, explores F. M. Cornford's brilliant thesis in *Thucydides Mythistoricus* that Thucydides' *History* borrowed heavily from Aeschylean tragedy in achieving its narrative intelligibility. The main argumentation in support of the proposed alliance between the fictive and the historical imagination is to be found in the essays by historian Hayden White and philosopher Louis Mink. White's essay, "Historical Text as Literary Artifact," is a retrospective presentation of some of the themes advanced in his *Metahistory*: that explanation in narrative history is achieved by structuring historical episodes in such a way as to conform to one of four possible plots identified by critic Northrop Frye as comedy, tragedy, romance, and irony; that there are four plots because of the "tropological" deep structure of figurative language. White's views are implausible to this reviewer; his general argument here, however, is much more accessible than it was in *Metahistory*. Mink's essay, "Narrative Form as a Cognitive Instrument," delineates some of the epistemological issues that arise once the kinship between history and fiction is legitimized: How can we insist that historical narratives be "aggregative" when (like fictional narratives) they cannot aggregate "in so far as they are [distinct] narratives" (p. 143)? If "narrative form in history, as in fiction, is an artifice, the product of individual imagination," how can narrative histories claim "to be representing a real ensemble of interrelationships in past actuality" (p. 145)? Why do we continue to regard "the actuality of the past as an untold story" when "there can in fact be no untold story at all, just as there can be no unknown knowledge" (p. 147)?

One weakness in the essays by both Mink and White is the failure to consider the fact that narrative intelligibility is required in *any* history, whether the subject matter is human actions or natural events. Why is James Hutton's great narrative his-

tory, *The Theory of the Earth*, as "literary artifact," not exposed to the sorts of philosophical difficulties alleged to accompany the writing of human history?

The editors, Robert H. Canary and Henry Kozicki, have provided an introduction and a useful bibliography.

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PETER MATHIAS and M. M. POSTAN, editors. *The Cambridge Economic History of Europe*. Volume 7, *The Industrial Economies: Capital, Labour, and Enterprise*. Part 2, *The United States, Japan, and Russia*. New York: Cambridge University Press. 1978. Pp. xi, 639. \$39.50.

This is the ninth physical volume to appear in the series begun over a quarter of a century ago. Following the format of the previous volume (Vol. 7, Pt. 1), which discussed several European countries, it brings together under one cover eight essays by distinguished scholars from Great Britain, Japan, and the United States on the themes of capital formation, labor, and enterprise in Russia, Japan, and the U.S. In criticism of the volume as a whole and its editors, Peter Mathias and M. M. Postan, it must be explained to prospective readers that it does not consist of comparative history in any meaningful sense. Neither is it a comprehensive history of industrialization—or even of capital, labor, and enterprise—in the three largest non-European industrialized economies of the world. The editors presumably have restricted their function to the assignment and evaluation of essays on the three themes indicated. Moreover, there are wide gaps within the themes. Soviet industrialization is touched upon only tangentially in a ten-page essay restricted to the NEP period. Indeed the twentieth century is not treated in most of the book, and there is no chapter at all on American labor.

Turning to the essays individually, a reviewer must try to do justice to what are the illuminating and important works of distinguished historians and economists. Space does not permit separate reviews of each essay when some amount to full-scale monographs—the "chapter" by Olga Crisp, for example, comes to over one hundred printed pages. Nor is it possible for a reviewer to attempt the comparative analysis not provided by the editors and authors presupposing mastery of scholarship in two disciplines, history and economics, and two exotic languages, Japanese and Russian. I can pass on to the reader some matters of interest and importance observed along the way and some general comparative statements.

Turning first to the essays on enterprise: little comparative analysis can be made of these essays, as

they deal with quite different aspects of enterprise, although with roughly the same periods of the nineteenth and early twentieth centuries. Kozo Yamamura and M. C. Kaser deal with private enterprise as traditionally defined: organized activity for economic ends within a market framework. Kaser, however, focuses on the many types as well as social and ethnic groups of entrepreneurs that have functioned in Russia during the past millennium and on their relationship with the state, which acted as a promoter of or impediment to enterprise or as a substitute for it. He sees NEP as "the last period in which entrepreneurship could be exercised" and views the Stalin and post-Stalin periods as characterized by the substitution of state administration for entrepreneurship, except for "the fringe of managerial action and the penumbra of the private section beyond" (p. 492).

Yamamura focuses elsewhere: on the problem of Westernization in the evolution of Japanese enterprise. Reviewing the orthodox and revisionist historiography of the Japanese entrepreneur, he emphasizes more the roles of peasant and merchant capitalists and less those of samurai and banks. In examining the various types of enterprise and management in nineteenth- and twentieth-century Japan, he criticizes the venerable theory of Westernization in favor of a look at the adaptation of Japanese institutions to modernization. "Japan," he concludes, "modernized in a Japanese way" (p. 264).

The essay by Alfred O. Chandler, Jr., deals with a new field of American business history, that of industrial administration. This is touched upon briefly in the Yamamura essay in a discussion of paternalism and the *ringi* tradition in Japan, but practically nothing is said about the substantial subject of Soviet management in the Kaser essay. This is unfortunate, because a comparative administrative history of industrialization in Japan, Russia, and the United States would be most enlightening. Chandler takes as his theme the proposition that "organizational innovation, like technological change, has been central to the process of modernization." His essay, the most important and well-written contribution to the collection, traces the history of industrial administration in the private sector in the United States during the nineteenth and twentieth centuries from the "all purpose colonial merchant . . . not very different from those that had handled production, distribution, finance and transportation in Europe since the Renaissance," to the complex corporate administrations spawned first by the railroads and the demands of their technology in the late nineteenth century, to the multinational conglomerates and media networks of today. He sees the market and technology as determinants of this process. Modernization breeds

bureaucracy, one might add, in the economic as well as the political sphere.

More significant comparison can be made of the two fascinating and vivid essays on industrialization and labor in Russia and Japan in the late nineteenth and early twentieth centuries. The "red thread" that runs through Olga Crisp's study is the theme of ruralization of Russian industry and the industrial labor force. Russia's factory workers in the late tsarist period never ceased to be peasants and were never fully transformed into a permanent, hereditary urban proletariat. This was of tremendous consequence for the economic modernization of tsarist Russia, because it determined the continuing rural location of industry, low wages, low productivity, low profits, and high concentration of industrial organization. The result—with Russian labor as with capital, technology, and the state in late tsarist times—was a very partial kind of modernization.

The transition from tradition to modernization in Japan was far more comprehensive and convulsive during the same period. The building of a Japanese factory labor force at the turn of the twentieth century involved a nightmare of exploitation, as compared to Russia where, as Crisp notes, factory conditions were comparatively quite "rosy." Koji Taira vividly relates the horror story of the kidnapping and enslavement of female labor in what can only be called factory prisons in Japan in the period 1890–1910. Japanese women left the farms in those days to make up most of the industrial labor force, unlike the much smaller proportion of female factory workers in tsarist (not Soviet) Russia. Russian women remained in the villages.

The role of the state in factory legislation can be contrasted in tsarist Russia and Japan. In both cases it was positive, but for different reasons. The tsarist government was more antimodern in its attitude toward industry and capitalism. The Japanese government supported a military modernization program that aimed for a "strong army and a rich country" at a time when Japanese capitalism was producing unhealthy and demoralized recruits and mothers of recruits. The government's response to this was factory legislation and education; private industry's response was a more progressive and rational management.

The three essays on capital formation in the United States (Lance E. Davis and Robert E. Gallman), Japan (Kazushi Ohkawa and Henry Rosovsky), and Russia (Arcadius Kahan) are concerned with measurement of capital input. Space does not permit, nor does the reviewer feel qualified to provide, the technical comment that these essays deserve.

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ROBERT L. HEILBRONER. *Marxism: For and Against*. New York: W. W. Norton. 1980. Pp. 186. \$9.95.

Robert L. Heilbroner is a distinguished professor of economics, well known for his stimulating, lucid, and iconoclastic writing. *Marxism: For and Against* is his latest work, a monograph of slightly over 150 pages of large print. In addition to an introduction, there are four chapters, corresponding to what Heilbroner identifies as the four main elements of Marxism, and it is the author's intention to provide the reader with a sympathetic yet critical introduction to the thought of Karl Marx and his followers.

No short overview of Marx will make everyone happy, but Heilbroner provides an adequate, if mainstream, general presentation of the Marxist perspective. The first three chapters discuss the dialectical approach to philosophy, the materialist interpretation of history, and the Marxist "socioanalysis" of capitalism. Integrated with the exposition of each chapter is a fairly straightforward account of the strong and weak points, as Heilbroner sees them, of that aspect of Marxist thinking. For example, he explains the attractions of a dialectical approach while at the same time indicating why it remains so intellectually elusive. The chapter on capitalism, as those familiar with Heilbroner might suspect, is the best, and he succeeds in making clear to a general reader how Marx saw the everyday commodity as holding the secret to understanding the socioeconomic essence of the bourgeois world.

The final chapter on the Marxist commitment to socialism is the least trenchant. Heilbroner ranges over the connection between Marxism and the totalitarian regimes that pledge allegiance to it, the nature of freedom, the Marxist conception of the individual, and the ways in which socialism is supposed by Marxists to constitute a distinctive form of society. These are difficult issues requiring an extended treatment that Heilbroner does not supply. Although the reflections he offers are not unintelligent, neither are they especially insightful or profound.

This characterizes the book as a whole. It is eminently readable, but slight. Heilbroner prides himself on adopting an attitude of *for and against*, but a number of other writers furnish even-handed treatments of Marx. Advanced readers will learn little, if anything, from this elementary essay, and, although those unacquainted with Marx would presumably find Heilbroner's book beneficial, undergraduates would be better steered toward one of the many more substantive introductory texts available on the subject. Fans of Heilbroner may wish to read the book anyway (after all, it is short), but otherwise *Marxism: For and Against* cannot be strongly recommended for anyone's reading list.

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LOUISE A. TILLY and JOAN W. SCOTT. *Women, Work, and Family*. New York: Holt, Rinehart and Winston. 1978. Pp. xiv, 274. \$5.95.

The subject of this volume, as everyone with a knowledge of the two authors, Louise A. Tilly and Joan W. Scott, would know, is primarily Frenchwomen—Frenchwomen at work and Frenchwomen in the family during the time of industrialization. But at all relevant points the English story is set alongside the French, and this book therefore is a genuine specimen of an exercise often commended but rarely properly carried out, a cross-national historical comparison.

It is a pity that the title does not proclaim the character of the work. This is one more example, no doubt, of the values of the publisher being imposed on those of the writer, for it is a publishing house dogma that books about individual "foreign" countries lack a market. Not only does this mislead the reader in the present case, but also it serves to obscure the difficulty of the task that Tilly and Scott have set themselves and the extent of their achievement. Comparative historical sociology makes formidable demands not only on the researcher but also on the writer. A shapely, useful book can be expected only rarely.

There are three conclusions that come out of this somewhat laborious exercise, all of importance to the subject in hand and to the pursuit of historical social structure. First, there is no one story to be told of women and the family before, during, and after industrialization but rather many stories. The processes and the relationships varied above all by class and also by occupation, by country and cultural area, and by region or subregion within those areas. Second, economic and technological events and vicissitudes were of paramount importance. When and where textile industries flourished and mechanized, for example, women got jobs outside the home, at least in the early stages of development, but where mining or metal trades developed, they did not. Third, a two-nation comparison tells us a great deal more than a one-nation description. It is nevertheless puzzling to some extent in its outcome and often unfortunately confusing to the reader. From all this it follows, however, that no straightforward generalization about woman's liberation can be made out of the English and French files, the two earliest industrialization cases.

There has been no linear development out of patriarchy into sex role consciousness and equality. Indeed, the Whig account of the process, where everything is looked upon as culminating in our present, is demonstrated as inadequate by detailed and illuminating discussion and description in this book. Work now in hand or published since Tilly and Scott finished theirs certainly confirms this conclusion.

Keith Snell, for example, has now demonstrated

that the apprenticeship of women to a whole range of handicrafts, skilled and unskilled, including that of the blacksmith, was accepted practice in England up to the end of the eighteenth century. It was the exigencies of nineteenth-century working-class life that drove them out of the trades. In a brilliant little study of the French peasant family (*Mari et femme dans la société paysanne* [1980]), Martine Segalen insists on French peasant marriage as a collaboration in production between man and woman. It was a symmetrical relationship, if not of equality, then certainly of interchange between individuals known by both to be indispensable to each other. Behind all this it might be suggested there lies a conception of marriage as a common project shared between man and woman, shared while they are still at home by their children and even by the living-in servant.

To the common project (the family, the household, the craftsman's enterprise, or the farm) corresponded the family fund, and this was administered to a surprising extent by the women, a practice that may go far back in time in both countries. Daughters may have felt more committed to that family fund than sons. That this is not the whole description of family, women, and work in European pre-industrial society is made apparent if we glance from the French and the English toward the center of the Continent, where the role of women in *dem ganzen Haus* was somewhat different. But it would be less than just to the book under review to pile country upon country and obscure the value of the comparisons that have been drawn by Tilly and Scott.

The distinctions they make between the wage economy and the consumer economy seem to me fundamental to the subject, but I found other parts of the dialectic rather less persuasive. It was a good, brave notion to add a coda on women since World War II, and there is much new and interesting material about internal familial relationships, especially of mother and daughter. The descriptions of economic development seem to me to be sounder and much more useful than their demographic descriptions, and there is a tendency to accept fashionable misjudgments on such things as want of affection between peasant spouses and for their children. The account of illegitimacy I also found unconvincing.

The bibliography is good and useful but unfortunately incomplete. It still does not prevent the reader from having to shuffle through the footnotes for an author and a title. Most of the graphic presentations are helpful, although one or two of the figures could not be so described (as, for example, those on page 158). The general style of the prose I found a little disappointing. But this is a useful text, and quite an important one.

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PETER N. STEARNS. *Be a Man! Males in Modern Society*. New York: Holmes and Meier. 1979. Pp. ix, 230. Cloth \$19.75, paper \$9.75.

Peter N. Stearns, well known to the profession for his many books on the social history of modern Europe, has undertaken to survey a topic not previously studied as such. Though feminists have rightly complained that until recently most written history was devoted to men, manhood itself escaped scholarly attention. Thus Stearns is in the position of reviewing something about which there are no historical monographs. This reverses the normal order of things whereby general studies build on specialized investigations. There is a literature bearing on the problem, and Stearns has read a remarkable number of social histories and works of contemporary social science, but few of them confront manhood directly. Lack of specific data has obliged Stearns to make inferences and to speculate more than might be wished. Yet he has succeeded in defining maleness as a historical subject in ways that later investigators will probably find useful. He is sensitive, as might be expected, to class as well as gender differences. He has tried to develop generalizations applicable to the West as a whole rather than to specific cultures. This is a brave book, also, that does not hesitate to make, or take on, controversial opinions.

The weaknesses of this study are, for the most part, related to its strengths. Manhood is such a large topic that many aspects have been omitted of necessity. More troubling is the absence of a unifying thesis that would have tied the often disparate bits of information together. On the other hand, to Stearns's credit he has not forced his material into a predetermined mold. He is to be admired also for not moralizing about social change. There are no villains here nor any search for scapegoats—a welcome change from too many recent works by historians in which indignation poses as social analysis.

Despite occasional value judgments that will be annoying to those who do not share his assumptions in all respects, this is a serious book by a responsible scholar. In seeking to establish a historical backdrop for a subject often discussed in a vacuum, Stearns has rendered a valuable service. And he has posed a challenge to other historians to make better sense, if they can, of a subject that, while seemingly self-evident, turns out to be more bafflingly elusive the further it is pursued.

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VERN L. BULLOUGH. *Homosexuality: A History*. New York: New American Library. 1979. Pp. ix, 196. \$4.95.

Over the past few years Vern L. Bullough has published several volumes dealing with the history of

sexuality and has thereby drawn attention to a much-neglected branch of history. Despite its frequent subservience to psychology and sociology, the history of sexuality deserves to take its place alongside its elder brethren among Clio's offspring, including social history and the history of the family and of women. At this early stage, Bullough's work provides a useful survey of general trends and issues and is likely to inspire more detailed monographs dealing with particular periods or themes. Given the readily available sources, the concern is still with the history of attitudes toward sexuality, rather than the history of sexuality *per se*. The current work deals with homosexuality and provides the only available broad sketch of the myths and theories connected with this most controversial subject. Bullough is especially valuable when dealing with the sexual liberation movements of the nineteenth and twentieth centuries in the English-speaking world, a subject that is rarely treated in the context of the social reform movements that animated the period. He provides a good description of the contributions of such figures as Magnus Hirschfeld, Henry Hay, and Havelock Ellis, along with a host of sociologists and psychologists who were instrumental in changing attitudes toward homosexuality. The earlier periods, from the ancient world to the nineteenth century—because one must rely on inexplicit, sparse literary sources—are somewhat less satisfactory, although even here Bullough performs an important service by bringing together under one cover material that is otherwise dispersed in other volumes, including some by the author himself.

While various modern theories concerning the genesis of the repressive approach to homosexuality in the Western world are presented, this extreme animus remains a riddle. It might also be helpful to examine the relative treatment of homosexuals in various periods, as opposed to adulterers, rapists, prostitutes, and other sexual offenders, in order to determine whether (within the context of general persecution) there have been periods of greater liberalism, and why. The peculiar role of religion as an enforcer of sexual conformity deserves further attention. Bullough's work, rather than being a detailed history of homosexuality, is an extended essay supported by historical examples and serves as a good introduction to the treatment of homosexuality from ancient times to the present.

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MICHEL DESPLAND. *La religion en occident: Évolution des idées et du vécu*. Preface by CLAUDE GEFFRÉ. (Héritage et

Projet, number 23.) Montreal: Fides. 1979. Pp. xiii, 579.

The preface by Claude Geffré that graces this volume opens by reminding us that "the debate concerning the origin of the idea of religion is always open." Geffré goes on to say that the present study in this context makes a unique contribution—he knows of no equal in the French language. What Denis de Rougemont has done for the history of love in the West, so has Michel Despland done for the history of the idea of religion in the West, he affirms. He also predicts that the book will become a classic that must not be overlooked by any historian or theologian (not to mention philosopher). In a way, the author writes a history of the West by following the destiny of the word and the concept. Though the book may never become a classic, it certainly should not be overlooked, for it does make a major and unique contribution to the subject at hand.

The inquiry is historically based from start to finish. In one respect, then, it is the finest survey of the history of the contextual definitions of religion in the West. One of the most helpful parts of the volume is an appendix (among other appendixes) of forty definitions of religion from Cicero to Schelling with corresponding page references to the text. He begins with Greek and Latin pre-Christian definitions of religion. Cicero is presented as the first to explain systematically the idea of religion and to show the ambiguity involved in the idea of religion as public order and religion as interior experience. The origin of the idea of religion in the West is not uniquely Christian, but it is true that the church fathers molded the idea that was then passed on to the medieval and later periods—the Christian religion exclusively transforms men first and then gives order to society (*à la* Augustine). An excellent survey of medieval writers is followed by careful attention to radical changes in sixteenth- and seventeenth-century Europe. Of France Despland writes: "The country that invented religion as civilization separated religion from civilization." Voilà! The stage is set for the eighteenth-century crisis and the modern postreligious mentality. Despland at this point analyzes correctly the developments in the field of philosophy of religion and the scientific study of religions. One lesson that the theologians must learn as a result of this study is to say the words "faith" and "religion" in the plural and not in any exclusive way, as some do. Despland's book opens many ecumenical doors.

Despland's knowledge of the sources is encyclopedic. He neglects no major writer as he traces the history of the idea of religion. We should hope that Despland will someday give us a companion volume—a modern philosophy of religion. He proves

himself to be capable of this in the present volume, one that will not be soon replaced. Excellent notes, bibliography, and useful appendixes further enhance this book.

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E. P. MEIJERING. *Calvin wider die Neugierde: Ein Beitrag zum Vergleich zwischen reformatorischem und patristischem Denken.* (Bibliotheca Humanistica and Reformatorica, number 29.) Nieukoop: B. De Graaf. 1979. Pp. 122. f 55.

E. P. Meijering has written a compact, comparative study of attitudes toward speculative theology in patristic times and the Reformation. A patristics scholar, Meijering presents a new perspective both on Calvin's use of the church fathers and on the Reformation itself. Calvin felt that the Bible contained the entirety of religious truth; to allow curiosity (*Neugierde*) to carry one beyond the truth of Scripture was to engage in useless and dangerous philosophical speculation. He was sure that the theology of the church fathers had been more biblically pure than that of the medieval Scholastics, that the fathers had avoided speculative *Neugierde* while the Scholastic theologians had perverted early Christian theology with their dangerous incursions into philosophy. The Reformation had restored the biblical purity of the earlier period.

Meijering compares Calvin on this issue with three church fathers. In the course of his treatment he deals with several major theological questions, such as the nature and will of God, the creation and the fall, predestination, whether God is the author of sin, and the timing of the incarnation of Christ. His judgment is that Calvin was closer to Irenaeus on the issue of curiosity and farthest from Augustine, with Tertullian being halfway in between. Thus, in spite of Calvin's heavy use of Augustine and his general agreement with Augustine on the doctrines of human sin and divine grace, he rejected Augustine's "platonic" speculations. Calvin, however, seldom expressed his suspicions of Augustine. Generally, the author feels that Calvin was wrong about the fathers, that his position on *Neugierde* was not theirs, except perhaps for Irenaeus. Augustine and Tertullian were much more willing to engage in philosophical speculation than Calvin was willing to admit.

Meijering concludes that Calvin was both a bibliclist and a reductionist: a bibliclist because he thought that everything useful and necessary to know was contained in the Bible, a reductionist because he thought that the entire Bible could be treated from the focal point of the doctrines of sin and grace. Ironically, the doctrine for which Calvin

is most famous, the free elective will of God, resulted, in the author's opinion, from the forbidden *Neugierde*, and it was on this doctrine that Calvin reached his highest level as a theologian. And that is Meijering's real point—that the modern theologian, in order to reach a higher plateau, must eschew biblicism (whether of Calvin's type or the more modern variety) and admit that he and the philosopher are engaged in the same quest for truth.

If Calvin was a bibliclist, so were most all of the Protestant reformers. The fact that Calvin rejected speculation by both the medieval Scholastics and the church fathers is hardly surprising given the Reformation principle of *sola scriptura*. But this fascinating little book does place this Reformation emphasis within its larger historical perspective, and it suggests the extent to which the Reformation destroyed old patterns of authority and substituted for them what was, in essence, the new authority—the Bible.

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O. H. K. SPATE. *The Spanish Lake.* Minneapolis: University of Minnesota Press. 1979. Pp. xxiv, 372. \$39.50.

This volume is a comprehensive and imperial treatment of Pacific history from the day the ocean was first sighted by Balboa until the English interfered with the Iberian monopoly. O. H. K. Spate is not just concerned with the sixteenth century, however; his first chapter is entitled "The World without the Pacific." Exploration, starting with Magellan's voyage, is well covered. Spate's training as a geographer has enabled him to draw simplified maps, one of which shows the antimeridian of the Tordesillas Line.

The history of Spanish expansion on the eastern shores of the ocean is more detailed than one would expect in a book on the Pacific; Spate puts great stress on the impact of silver. Also, Spate naturally deals with the history of the Asian countries facing the Pacific and their relations with Europeans. The Manila galleon trade was the link between them and Spain by way of Mexico. The Japanese might have conquered the Philippines had they succeeded in Korea. Spate points out similarities between the 1950-53 Korean War and Japan's campaign in the peninsula. The book ends with Francis Drake's irruption into the Pacific and the Spanish attempts to prevent a recurrence of foreign incursions.

The Spanish Lake does not have a bibliography with authors listed in alphabetical order. Spate explains that "the machinery and the toil of making this book is adequately illustrated in its notes" (p. xx). There are sixty-one pages of notes with abbreviations for works referred to several times. A con-

siderable number of works in English, French, Spanish, and Portuguese has been consulted. Spate has used monographs, collections of documents, and some unpublished Ph.D. dissertations. He also recognizes his debt to Pierre Chaunu who has published on Spain and the Pacific.

Although Spate has perused Celsus Kelly's *La Australia del Espíritu Santo*, no reference is made to the Kelly-Bushell collection of documents on the Spanish voyages in the South Pacific called *Australia Franciscana*, which now has six volumes and is more up-to-date than translations made long ago by Markham and Amherst for the Hakluyt Society. The author frequently refers, however, to Colin Jack-Hinton's *The Search for the Islands of Solomon, 1567-1838*. Jack-Hinton has made extensive use of such documents. In a work of Spate's scope, it is naturally impossible to go to primary sources in each instance.

The Spanish Lake is meant to be the first of a multivolume series entitled *The Pacific since Magellan*. It is a remarkable and needed contribution in English to Pacific history. Spate gives recent opinions on controversial questions, sometimes taking to task well-known writers like the late S. E. Morison. In some cases one cannot help wishing that part of his argument had been relegated to the notes to make for smoother reading. On the whole, however, the book is well written and interesting, and it abounds in touches of humor, often in a foreign language, revealing an extensive erudition on the part of the author. The next volumes in the series are to be looked forward to.

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ANCIENT

ELIZABETH M. CRAIK. *The Dorian Aegean*. (States and Cities of Ancient Greece.) Boston: Routledge and Kegan Paul. 1980. Pp. x, 263. \$22.50.

Elizabeth M. Craik's book studies the Dorian islands of "Melos, Kimolos, Pholegandros, Sikinos, Thera with Therasia, Anaphe, Astypalaia, Kalymnos, Kos, Pserimos, Nisyros, Telos, Karpathos with Kasos and Saros, Chalke, Syme with Teutlunssa, and Rhodes." Successive chapters deal with "Setting" (geography, maritime communications, progress of excavation), "Resources" (foodstuffs, wine, trade, timber, marble, silk), "History" (from prehistoric times until the Early Roman Empire), "Language and Script," "Literature," "Medicine and Science," "Myths," "Cults," and "Administration" (mainly of sanctuaries, but the author notes

that sacred and secular administration overlap). A substantial appendix lists epigraphically attested titles of deities worshipped in these Dorian islands. There are indexes of names and subjects and also an outline map.

The book is intended for non-specialists as well as classicists. Citations of ancient works are given in English translation. Without impairing her focus on the Dorian islands of the Aegean, the author gives under each heading enough introductory information to clarify the place of the islanders in general Greek development. The work is essentially a collection of data; theory is kept to a minimum, although the author argues persuasively for the thesis that Dorian settlers reached the islands before the end of the bronze age (pp. 27-30).

The chapters on religious activities are the richest; the book appears to have started from research into the administration of cults. The treatment of literature, science, and medicine is illuminating; Kos and Rhodes play a larger part in these chapters than the other islands. The treatment of political history is restricted by the scanty character of the sources, yet perhaps a little more could have been attempted; *Hellenica Oxyrhynchia* 15 (10) might have been exploited both for the revolution it narrates and for the political group, the *Diagoreioi*, that it attests. There are a few slips on points of general history, for example, on the dates of Philip II's siege of Byzantium, of the battle of Pharsalos, and of Pompey's operations in Asia Minor (pp. 37, 43, 98). Even so, the author is to be congratulated on having done a workmanlike job of presenting information without distortion. For example, she does not try to impose developmental unity on her islands; it is apparent to the reader that as early at least as the period of the Persian Wars the different islands followed different historical paths.

Presumably the publisher is responsible for the decision not to give foot- or endnotes. References are given in parentheses in the text; some of them are long enough to disturb the continuity of reading.

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FRIEDRICH PRINZ. *Gründungsmythen und Sagenchronologie*. (Zetemata, number 72.) Munich: C. H. Beck. 1979. Pp. xi, 483. DM 129.

This book offers detailed analysis of a number of Greek foundation myths, embracing those for places founded by Greek states in the great era of colonization, for Greek states and for originally non-Greek states whose origins precede the era of colonization.

Friedrich Prinz, diligently displaying ancient tes-

timony from well-known as well as inaccessible authors, gives an accurate summary of the story and its variants for each myth. A signal enhancement is the quotation, often at full length, of most of this matter in the text and in 217 items of "Testimonia" (pp. 377-450). In addition, the author scrupulously tracks developments in modern scholarship. The book's clarity of presentation and attention to detail put it on a par with Prinz's exemplary "Herakles" article in Pauly-Wissowa-Kroll, *Real-Encyclopädie der classischen Altertumswissenschaft* (Suppl. 14 [1974]: 137-96).

But Prinz's aims to do more than merely produce "eine zweckdienliche Ergänzung eines mythologischen Lexikons." He seeks to counteract the tendency of modern interpreters to place too much faith in the existence of historically reliable "kernels" within the foundation myths. He argues that careful, strictly intrinsic scrutiny of the stories reveals a transparent "Logik" behind most of them: they were formulated first to carry the origins of the given city back as far as possible and, secondly, to associate the foundation with some prominent hero, for choice a Homeric figure, Herakles, or an argonaut. To this end, willful distortions and fabrications were introduced on the strength of tenuous associations with fitting heroid myths. For example, according to Pausanias and pseudo-Apollodorus, the city of Miletus in Asia Minor was founded by the eponymous hero Miletus, who emigrated from Crete with the hero Sarpedon (pp. 97-111). But this creates a problem. Sarpedon, according to the *Iliad*, was king of the Lycians during the Trojan War and son of Zeus and Laodameia. Our present sources prefer, however, the Hesiodic genealogy that makes him son of Zeus and Europa and brother of Minos. This puts him some two generations *before* the war at Troy! The truth is that the official version has no authority earlier than Ephorus (fourth century), who followed Hesiod (and after him Herodotus) in order to maintain the chronologically troublesome Cretan connections—this because of the existence of a place called Miletus (near Mallia) on Crete. This coincidence of place-names was the original impetus for a story that conveniently linked the Anatolian city with a suitable heroic character; this concern overrode whatever uneasiness there may have been about maladjustment either with Homer or with the actual circumstances of the city's founding.

This sort of discussion is typical and makes the book valuable not only as a compendium but also as a judicious devaluation of the historicity of Greek foundation myths.

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DAVID M. SCHAPS. *Economic Rights of Women in Ancient Greece*. Edinburgh: Edinburgh University Press; dis-

tributed by Columbia University Press, New York. Pp. vi, 165. \$16.00.

In a Harvard dissertation completed in 1972, David M. Schaps reviewed the evidence for women and property control in Classical and Hellenistic Greece. The results of this research have now been published, virtually unrevised. With the exception of two articles by Schaps, the most recent work cited appeared in 1974.

The focus is on the "ordinary free Greek woman," although the financial transactions of freedwomen and prostitutes are examined by way of contrast. The topics covered include types of property, acquisition, the *epikleros* (heiress), the economic function of the *kyrios* (guardian), exchange, disposition, inheritance, and dowry. Forensic orations and inscriptions from mainland Greece and the Aegean to 146 B.C. provided the raw data. Thus papyri are not examined, perhaps owing to the commonly held view (traceable to Herodotus) that whatever happens in Egypt is not typical of the rest of the Mediterranean. The deliberate exclusion of Hellenistic queens is unfortunate, since it was by following their precedent that nonroyal women increased their economic participation in the public sphere.

Schaps's work is a model of patient scholarship limited to the narrow description of circumscribed topics. He offers innovations only in his discussion of the *epikleros*, and, except in one chapter titled "Patterns in Women's Economics," he avoids analysis. He presents evidence from the end of the fifth and from the fourth century that citizen women worked as midwives and vendors—jobs that few respectable women held in the prosperous period of the Athenian Empire—yet he nowhere theorizes that the Peloponnesian War precipitated the change. He shows that some women owned some slaves and movables acquired or produced during marriage and might take them in case of divorce but fails to note that no woman took her children. His ignorance of some secondary literature results in error. He gives examples of wealthy fathers who contributed small dowries, warns us against presuming any relationship between a father's wealth and his daughter's dowry, and states "the purpose of the dowry was to attract a husband" (p. 78). Yet, in 1967 Wesley Thompson discovered an endogamous pattern of marriage between first cousins among the wealthy that reduced the need to attract outsiders. Schaps agrees with earlier scholars that women were not permitted to own land in Attica but fails to ask the obvious questions that this prohibition raises. Were women on a par with the resident aliens who, although they served in the military, were excluded from owning the land they defended? What does "citizenship" connote for

women? The Athenians believed that they had not conquered Attica but had always possessed it. Can the denial to women of the right to own land be explained by this myth of autochthony, denying a birth from woman and positing instead that the men of Athens had sprung directly from the land?

Other historians will surely ask more provocative questions of the same material and draw more complex conclusions, but their work will be facilitated by this sane little book.

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CHRISTIAN HABICHT. *Untersuchungen zur politischen Geschichte Athens im 3. Jahrhundert v. Chr.* (Vestigia, Beiträge zur Alten Geschichte, number 30.) Munich: C. H. Beck. 1979. Pp. x, 163. DM 48.

The political history of the third century B.C. has not yet been written and for good reason. From the end of Book 20 of Diodorus Siculus to the beginning of Polybius's *Histories* (301 B.C. to 221 B.C.) no comprehensive narrative of political and military affairs based on a chronological scheme has survived from antiquity. Some important events of the period cannot be precisely dated, and relations between the powers, the Hellenistic kings as well as the Greek cities and leagues, remain obscure.

One major source of information has been inscriptions. The thousands of Greek texts found in the excavations of the Athenian Agora by the American School are especially important. In 1971 the virtually complete text in 109 lines of a decree passed by the Athenians honoring Kallias, an Athenian citizen who took service with the Ptolemaic court in Egypt, rose to high rank, and used his position to aid Athens, was found. This inscription, which forms the central point of Christian Habicht's *Untersuchungen*, was later published with full commentary by T. Leslie Shear as *Hesperia*, Supplement 17 (1978).

Habicht deals with Kallias's decree in section 4 of *Untersuchungen*. He discusses two points where he disagrees with the editor's interpretation: the date of the revolt of Demetrios Poliorketes at Athens (spring 287 against 286), and the nature of the peace of 287 (between Ptolemy and Demetrios only, not universal). The argument cannot be followed without the *editio princeps* and its commentary, but Habicht convinces me that he has established 294 as the date of Demetrios's capture of Athens, 287 as the date of the successful revolt against him, 294 to 229 B.C. as the years when the Macedonians continually occupied Munychia at the Peiraeus, and the nature of the peace of 287 B.C.

Habicht's primary concern is chronology, but he

ranges far and wide over the history of third-century Athens. The volume contains rich observations on internal and external Athenian politics and, in section 9, a lucid account of the problems in establishing an Athenian archon list (only six are fixed from 290 to 200 B.C.) on which our chronologies depend. This volume is not a history but rather is intended "als Vorstudien zu einer allgemeinen Darstellung Athens in hellenistischer Zeit." As such it is important and indispensable; these studies of seemingly discrete problems bring us far closer to the possibility of a history of the third century B.C.

Habicht's work is for the specialist—the volume cannot be used without the corpora of Greek inscriptions at hand and a thorough knowledge of the scholarship of the period—but it is elegantly written and clearly argued. It has moved us far ahead in establishing the framework on which Hellenistic history can be written.

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KARL-WILHELM WEEBER. *Geschichte der Etrusker*. Stuttgart: Kohlhammer. 1979. Pp. 215. DM 36.

The Etruscans have always had a great fascination as much for ancient as for modern students. Their art, their religion, their metal-working, their funeral customs, their language—perhaps their very inaccessibility—have challenged scholars since George Dennis (*The Cities and Cemeteries of Etruria* [1848]) to explore the nature and history of their civilization. Karl-Wilhelm Weeber has written a very useful book that covers the whole period from the first cultures in central Italy to the final disappearance of a distinctively Etruscan identity in the first century B.C. As a concise and sober account of the present state of our knowledge, it can be warmly recommended. It is written in a clear and simple style, and, although the type is too small for comfort, the bibliography inconveniently presented, and the illustrations too few for such an important subject, Weeber has made a judicious synthesis. His approach is primarily historical rather than archaeological, and it is a great pity that the production of his book coincided with the collection of seminal articles edited by D. and F. Ridgway, *Italy before the Romans: The Iron Age, Orientalising and Etruscan Periods*, which throws a great deal of new light on the archaeological background of the Etruscan period of Italy.

Two major issues stand out. The first is the question "Who were the Etruscans?" Their highly individual culture emerges in the eighth century B.C., chiefly in the area between the Rivers Arno and Tiber but with offshoots having their own local characteristics in Campania and in the Po Valley. Weeber reviews the gradual transition from the

indigenous Villanovan to the fully fledged Etruscan civilization and has a good account of the various upheavals and population movements that occurred around the Mediterranean between 1200 and 700 B.C. Whereas Ridgway and his contributors tend to explain the Etruscan phenomenon as the result of the steady influence on the native societies by Mycenaeans, Phoenicians, and Greeks (especially those settled on Pithekoussai with their skills in pottery and metallurgy), Weeber holds, I think rightly, to a more traditional view that some external infiltration must have taken place that gave a radically new turn to the way of life in Etruria. Only in this way can the linguistic and religious factors be accounted for.

Secondly, there is much controversy as to how far Rome was Etruscanized and became a *Tyrrhenis polis*. Recent excavations in other Latin settlements have still not been published or digested, and, while some archeologists tend to discount the importance of Etruscan influence at Rome, it is difficult not to believe that Weeber is fundamentally right in arguing that the evidence of family names, social institutions, and religious practices indicates that the Etruscan presence in Rome was not just a passing domination but a pervasive penetration.

It is greatly to be hoped that this book will soon be revised and issued in an English translation.

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J. M. BLAZQUEZ. *Economía de la Hispania Romana*. Bilbao: Ediciones "Najera." 1978. Pp. 725.

Readers led by the title to expect a systematic analysis of the Spanish economy during the Roman period may be disappointed to discover that J. M. Blazquez's work is instead a collection of essays and monographs on only certain aspects and selected periods of the economic history of the Iberian peninsula. A number of additional essays deal with interesting, but unrelated, topics such as Roman propaganda during the Julio-Claudian period, Iberian relations with Africa and the Semitic-speaking world, and the Moorish invasions of the second century.

The bulk of the work is devoted to the late Republic and the third and fourth centuries A.D. Considerable overlapping occurs in the former section where the two principal articles, "Economía de la Hispania Republicana" and "Economía de Hispania durante la República Romana," recapitulate most of the other six essays. The portion of the book devoted to the Empire is dominated by a monograph (134 pages) on the peninsula during the third and fourth centuries.

The book's main strength is its exhaustive exposi-

tion of the available literary, epigraphic, numismatic, and archeological evidence for the Iberian peninsula in the periods discussed. Unfortunately, it is a difficult work to use. There is no index, and readers must provide themselves with detailed maps of ancient and modern Spain since Blazquez's discussion is heavily topographical.

As is true of other studies of individual provinces of the Empire, *Economía de la Hispania Romana* provides a useful corrective to the tendency to see the expansion and maintenance of the Roman Empire from the narrow perspective of the city of Rome. The author's detailing of the large quantities of available evidence gives a sense of the complexity of the province's internal development. This approach counteracts the prevailing view that Rome's expansion occurred in a world neatly divided between a developed Greek east and a backward barbarian west.

Understandably, the relevance of Spain to the broader subjects of Roman imperial economic, administrative, and fiscal policy will interest most readers. Blazquez's intent, however, is more expositional than analytical, and not much space is devoted to empire-wide issues. Having demonstrated the exceptional agricultural and mineral wealth of Spain, especially the southern portions, Blazquez suggests that Roman strategic goals in the second Punic War aimed at cutting off the Carthaginians from their main sources of supplies and that Scipio's strike at Carthage was motivated by this strategy. Once the Romans accomplished this goal, Hannibal, according to Blazquez, was forced to go on the defensive in Italy. Similarly, the economic riches of Spain dictated Rome's decision to remain after the Hannibalic War, and the pursuit of precious metals occasioned, in one way or another, most of Rome's wars in the peninsula down to the time of Augustus. Baetica was colonized earliest and most profoundly because it was, economically, the most attractive part of Spain. Unlike Italy and Africa, however, latifundia were not widespread. Skipping the Principate, Blazquez surveys the third and fourth centuries in Spain in his longest essay. He concludes that the alignment of Spain on the side of Albinus against Septimius Severus was ultimately disastrous for Baetica, though not as immediately destructive as the German incursions of the third century. After this epoch many of the characteristic features of the Middle Ages began to appear in the Iberian peninsula.

These reflections of Blazquez are not, it should be emphasized, the main object of his work, though they do beg for further analysis in the wider contexts of Roman history. Van Nostrand's aphorism that "the chief item imported from Spain to Rome between 200 and 133 was experience" is still a view held by many scholars. Blazquez's discussion of the

wealth of Spain could be usefully inserted in the recently revived debate over the degree of economic motivation behind Roman imperialism. Similarly, his discussion of overpopulation and unrest in Celtiberia during the periods of Carthaginian and Roman occupation needs to be tied to the overall military and frontier policies and actions of the two powers in Spain. The absence of latifundia, the reconciliation of large export markets with the survival of small holdings (as contrasted with Africa), and the magnitude of Italian emigration, both formal and informal, should be taken into account by those interested in agrarian and demographic matters in Italy. Blazquez's interesting and wide ranging discussions also touch on Augustus's policy in Spain and the character and local impact of senatorial control of Baetica.

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ROBERT C. KNAPP. *Aspects of the Roman Experience in Iberia, 206-100 B.C.* (Anejos de Hispania Antiqua, number 9.) Valladolid: Universidad de Valladolid. 1977. Pp. 238. 875 ptas.

The Roman conquest of Spain was long, bloody, costly, but in the end extraordinarily successful. Unfortunately our ignorance of even basic facts after 167 B.C. (when Livy's record fails) is such that its history cannot be written. Robert C. Knapp's book (finished in 1975, published in Spain in 1977, and received here late in 1979), is the first attempt in English since 1939 to make large-scale scholarly contributions to its study. As regards Spanish topography, archeology, and scholarship, it is probably the best-informed work in this field ever written outside Spain.

It really starts from the beginning of Roman involvement and pursues several topics well past 100 B.C. Part 1 (pp. 13-57) collects and analyzes what can be known or conjectured of Rome's military and diplomatic methods. Part 2 (pp. 59-139) similarly treats the evidence on organization and administration. Part 3 (pp. 141-77) discusses Italian settlement, cultural penetration, and the spread of Roman citizenship. There are seven appendixes: three lists of Romans involved in Spain (useful, though badly organized and not wholly reliable) and four short special studies (only the last, "Pliny and the Towns of Ulterior," important); five excellent indexed maps; a bibliography; and a patchy, short index. The printing is appalling. An insert by the author blames the publisher for the "regrettable" errors, a few of which (mostly trivial) he corrects, while introducing more.

The topographical and (in part) the prosopographical sections are the most valuable. Discussion

of diplomacy is impaired by inadequate acquaintance with general scholarship on this (thus Knapp attempts [p. 44] to distinguish *deditio in fidem* from *deditio in dicionem*), but he commendably discards the orthodox scholarly model of a *lex provinciae* for each province and rejects recent attempts to deny the ancient record of an immediate division of Spain into two provinces. His grasp of the development and terminology of Roman administration is shaky in detail (for example, he implies the existence of judicial *conuentus* right from 200 B.C. and shows some uncertainty over "proconsular" and "propraetorian" *imperium*), but there are many acute individual observations. The attempt to work out the total cost and profit of Roman administration during this period (pp. 165-77) is, in the state of the evidence, totally useless, and not improved by ignorance of Roman numismatics.

After setting out the massive evidence for large-scale Italian settlement (no doubt chiefly by long-term veterans staying with their families), he unfortunately follows P. A. Brunt, whose thesis on Italian demography forced him to deny such settlement both in Spain and in Africa. Knapp, having no such thesis, should, instead of struggling to explain the evidence away, have resolutely followed it to the conclusion reached by Gabba and by most scholars. Perhaps he will reconsider.

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DAVID STOCKTON. *The Gracchi*. Oxford: Clarendon Press of Oxford University Press. 1979. Pp. xiii, 251. Cloth \$27.50, paper 12.95.

This is not an original book in the sense that it advocates some new thesis to interpret the general historical meaning of either or both of the Gracchi brothers. Instead, it is largely a reasoned analysis of the various reconstructions of the evidence that have been advanced to explain both individual problems as well as the overall meaning of the tribunes of Tiberius and Gaius Gracchus in Roman history (p. xii). It is not clear, however, who the readers are to whom the book is directed. It begins with an elementary explanation of the Roman constitution as it existed in the second century B.C., followed by a similar exposition of the (wretched) evidence for the Gracchi at our disposal. Yet appendix 2 at the end quotes Greek and Latin extensively in a discussion of the speeches of Gaius. Obviously, beginners will make little of the latter, and advanced students will not require either the former or the latter. On balance, and in American terms, the book seems best designed to fit the needs of beginning graduate students, although undergraduates or general readers, asking for a good detailed account

of the Gracchi, can be told to skip the Greek and Latin and read the rest of the book to their profit.

One feature might be expected to give trouble, but does not. In the cases of both brothers, the problems of their legislation *per se* are analyzed first, then a narrative account of events follows. The anticipated repetitiousness of this organization does not occur in any obtrusive way; in fact, the presentation is both clear and economical. In this reader's opinion, the book is an excellent introduction for the "advanced beginner."

On particular points, one approves the discussion of the limits of prosopography as a key to Roman history. The analyses of the complicated motives of both brothers, scions of the highest circles of the oligarchy, in turning against it are excellent. Errors or inadvertencies seem rare; yet David Stockton seems naive in attributing the "long sight" of historians to their looking backward, "passing judgement on matters which do not impinge directly on their own personal interests" (pp. 77-78). But in ancient historical studies, where scholarly writing is polemical more than anything else, historians have a professional stake in contesting rival interpretations. Historical writers should read widely in the epistemology of history, as well as in monographic literature. Yet, to end on a positive note: Gaius "was infected with a restless, but not necessarily reprehensible, urge to arrange and organize and improve" (p. 179). Such a verdict cannot be proved or disproved, yet it seems to explain much of what actually happened.

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J. P. V. D. BALSDON. *Romans and Aliens*. Chapel Hill: University of North Carolina Press. 1979. Pp. x, 310. \$24.00.

Books published posthumously often disappoint, for one reason or another. That is not the case here; indeed, it may be true, as is said on the blurb for a book club that has adopted this volume, that this is J. P. V. D. Balsdon's best work.

In a superficial sense, the book, filled with miscellany, is almost the kind of work usually entitled "A Companion to . . ." It discusses, with fine style and happy illustration, all the interesting and useful things that students ought to know that are not to be found in the usual textbook: the city, the classes (including slaves), freedmen, and foreigners—scores of topics. But the volume is much more than this. Balsdon has made some original contributions, and the collection, though skimpy in places, usually reflects long research. Moreover, the impression of the material somehow adds up to a cumulative whole. An underlying theme is that the decline invariably

stressed in ancient and modern studies of Rome was less comprehensive than is usually thought. It may be worse in our own time: "Our present age, unconcerned by its spiritual and moral decline, finds excitement in the technological advances of science and industry" (p. 8). Some Romans worshiped materialism too, but they were mostly members of the relatively small upper class.

The chapter titles convey the flavor of the text. The first two, "God's Own People" and "Snobbery Begins at Rome," are indicative enough. Balsdon wrote as he lectured, with careful and meticulous scholarship but also with a light touch and subtle wit. Much of the material is familiar, but Balsdon had the knack of finding something meaningful in the sources that others overlooked. Each chapter has its separate bibliography. The tremendous stream of data is supported by page after page of footnotes.

Detail in such great bulk must inevitably involve occasional errors. In chapter 9 on "Communications, Mainly Latin and Greek," Balsdon wrongly says, "Legends on the coins of the Italian insurgents in the Social War were in Latin" (p. 116). Yet the general point, the penetration of the Latin language throughout Italy, remains valid and, in fact, is shown by even earlier inscriptions. Do the scholars who argue that Jesus spoke Greek really deserve serious consideration (p. 138)? Surely it was the younger Scipio Africanus, not the elder, whom Plutarch matched with Epaminondas in the lost lives (p. 206). Infelicitous language is rare, but on the same page a confusion of pronouns makes it unclear whether a paragraph deals with Dio Chrysostom or his grandson. And it is shocking to find "sculpt" as a verb (p. 253). Perhaps an illiterate editor?

Balsdon's tilt in favor of the Romans is shown in chapters 12 and 13, entitled, respectively, "A Bad Press for Rome" (one subtitle is "Bricks to throw at the Romans") and "A Generally Good Press for Rome." The most trenchant critics of Roman society and government were the Romans themselves, the historian Tacitus, for example. The more favorable treatment came mostly from Greeks, Polybius and a long list of others. Of Diodorus Siculus, Balsdon says, "He was an alien, and evidently just as free, in a book published in Rome, to criticise aspects of Roman life as were the Romans themselves" (p. 200). A little later he adds that the Greeks who wrote these favorable things about Rome "knew from personal experience what the business of imperial administration was all about. In this respect they have an advantage over their modern critics" (p. 213).

Thus, Balsdon himself has here given the Romans "a generally good press," as in his earlier books and essays. He always took a tolerant view of

them as human beings not so very different from the rest of us.

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ANTHONY BIRLEY. *The People of Roman Britain*. Berkeley and Los Angeles: University of California Press. 1979. Pp. 224. \$20.00.

This admirable work represents the first attempt at a comprehensive prosopography of Roman Britain, and no one is better qualified to undertake it than Anthony Birley. Every individual whose name is recorded is considered and his part in the life of the province so far as possible established. Britain is ill-documented both by literature and by epigraphy, and the fact that such inscriptions as we have are concentrated in the military area further restricts the prosopographer. As a result, although the author has sifted all the evidence, down through potters' stamps to the meanest graffito, the greater part of the book concerns Roman officials and soldiers. This is splendidly done and constitutes a major contribution to our understanding not only of Roman Britain but also of the Roman Empire as a whole. For the native population there are additional difficulties, not merely the confusion produced by the fabrication of *gentilicia* but also by the fact that most of the auxiliary units stationed in Britain (and their veterans) were originally recruited in other Celtic provinces, which makes it hard to distinguish Britons from Gauls and others. Here some suggestions may cause surprise—for example, that Tetricus (p. 93) and Senecianus (p. 99) are recognizably Celtic names, and still more that a Frumentius (p. 95) should be a German. (Axumite Christians would be astonished!) Similarly the acceptance of *Provincialis* as a cognomen for M. Didius (p. 88) must make one hesitate to accept the suggestion that Tertullus *Provincialis* (and perhaps even Anencletus, entrenched though that belief is) should be a *servus provinciae* (p. 145). And poor C. Piscus Fagus, who lost his pot if not also his life on the Porcupine Bank southwest of Ireland, is surely more likely to have adopted the name as a fisherman than as a "merchant seaman."

These, however, are very minor criticisms, made more to illustrate the scope of the work than to decry it. The only factual error that this reviewer has detected is also unimportant (some of the *negotiatores* inscriptions (pp. 126–27) are from Colijnsplaat, not Domburg). There are very few misprints (all of them probably due to the abominable modern practice of going straight into page proof) and none of great significance, and one's only real criticism is of the incompleteness of the index of personal names. This is explained in a note (p. 217),

and the industrious reader can find them all by going to the fully documented chapter notes, but the lack of a comprehensive list makes it difficult not only to trace particular individuals but also to spot homonyms (for example, *Provincialis*) and detracts somewhat from the convenience of what is an invaluable work of reference.

A. L. F. RIVET
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MEDIEVAL

FRANK BARLOW. *The English Church, 1066–1154*. New York: Longman. 1979. Pp. xii, 340. \$36.00.

Frank Barlow of the University of Exeter has long been one of the major contributors to our understanding of the ecclesiastical history of England during the eleventh and twelfth centuries, more particularly in its constitutional and administrative aspects. The book under review is a sequel to his *English Church, 1000–1066*, published in 1963. Whereas that was basically a constitutional treatment, the present work embodies a magisterial survey of the period between William of Normandy's conquest of England in 1066 and the death of Henry II in 1154. A brief introduction precedes seven chapters, dealing respectively with the church's structure, personalities and principal events, government, justice, monasticism, education, and the problem of "church and state." Then comes a summary conclusion and a useful selection of maps and diagrams.

In a sense this is a textbook, but unlike many books so labeled it is written by an expert in the field, one thoroughly prepared to take issue (in the nicest way) with the views of other scholars. Thus, of H. S. Offler's comments (*English Historical Review* 66 [1951]: 32–41) on the "libellus" *De iniusta vexatione*—an *ex parte* account of the "trial" of William of St. Calais, bishop of Durham—he remarks (p. 281 n. 46): "Most of Offler's difficulties can be explained by the faulty transmission of the manuscript and the erroneous extension of names abbreviated to initials." He is equally wary (p. 252 n. 201) about accepting some of R. W. Southern's observations on the "School of Chartres," which in their original form fell like a bomb among an unsuspecting audience of the Ecclesiastical History Society, which included the late E. F. Jacob and David Knowles. These have now been published ("Humanism and the School of Chartres" in *Medieval Humanism and Other Studies*). On the vexed topic of grammar schools attached to secular cathedrals, the author argues that only three—York, Salisbury, and London—can be *proved* to have existed prior to 1154 (p. 234). This particular chapter ("Schools, Education and Scholars") ranges far beyond the

confines of England. The random nature of the evidence demands judicious handling, and it is a tribute to Barlow that there emerges a remarkably evocative picture of what education meant in the twelfth century, from local children learning the alphabet, reading with the aid of such basic texts as the Lord's Prayer and the Creed, or imbibing their Donatus "on the low form" (pp. 238, 242), to migrant scholars like Robert of Béthune, an erstwhile schoolmaster who studied at Paris and Laon and later became bishop of Hereford (pp. 249-50).

In his conclusion Barlow dwells mainly on the Normans' imperialistic attitude toward the English church. The reviewer's more lasting impression is of the church's civilizing influence and of the manner in which at all levels there was an attempt to substitute law, justice, and regular procedure for brute force and ignorance.

My criticisms are minor and few. The density of the material makes for stiff reading, which is not assisted by a style somewhat lacking in liveliness. There are too many short factual sentences. It is a relief to digress in pursuit of the varying fortunes of Christina of Markyate (p. 202 ff.) or of the oblate of Watton Priory, whose adolescent prurience led to the castration of her lover and some miraculous interventions far exceeding her deserts (p. 213 ff.). For these days there are surprisingly few misprints. I noted *en passant* the following: page 75, "dissentation"; page 106 note 3, "Hereford"; page 219, "Helose"; page 231 note 71, "scolastic." The publishers could well have provided a more interesting format and an attractive binding or jacket.

Students at all levels will find much of interest, and those who turn to the footnotes, a wealth of further reading.

ROY MARTIN HAINES
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WILLIAM E. KAPELLE. *The Norman Conquest of the North: The Region and its Transformation, 1000-1135*. Chapel Hill: University of North Carolina Press. 1979. Pp. 329. \$19.00.

This is a nicely conceived and clearly written little monograph. The subject is of more than local significance, for the Anglo-Norman state fashioned in the eleventh century was the birthplace of many of today's most important political institutions. Yet it is indeed of precise, local interest. In gathering together the threads of evidence—or at least a wide skein of them—William E. Kapelle has done us all a service. In a study where every line represents a judgment about difficult evidence, the room for disagreement and criticism is considerable. This reviewer will attempt to balance the need for a brief report of the author's findings against the tempta-

tion to enter into dialogue on specific items of interpretation.

The book is divided neatly at 1066. Four chapters lead up to the conquest, while a further three describe its impact and resolution. Kapelle is writing about a period in which English sources overtake those of other countries in their richness but for an area where this is least fully evidenced. He has unearthed no new sources and provided no new conceptual framework but rather has presented us with a simple narrative. If his theme of a challenge faced and overcome is obvious, it is also basic.

We may forgive the author for not discovering new material but must censure him for overlooking existing work. Three examples may suffice. First, he does not use C. Hart's *Early Charters of Northern England* (1975). There he would have found a narrative entitled "De obsessione Dunelmi" (pp. 143-50) that would have changed his account of the quarrel of Cospatrick and Waltheof over the Earldom of Northumberland (pp. 126-27). Second, he does not do justice to the view that Snorre Sturlason's "Heimskringla" is important for the story of northern military history (although he cites Richard Glover's well-known article in his bibliography). Third, in his account of the siting of Norman castles, he pays no attention to the debate about royal geographic and strategic mastery conducted over the years by such writers as Sidney Painter, R. A. Brown, and John Beeler. In particular, he is careless in his use of a major source, Symeon of Durham. Symeon does not state, for example, that the Conqueror built a castle at Durham for Earl Waltheof in 1072 (p. 127). He states rather that Waltheof founded the castle at that date. More could be said of inadequate or careless scholarship on Kapelle's part. Our final word, however, must be one of gratitude for a need met, a job well done.

M. R. POWICKE
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BARBARA ENGLISH. *The Lords of Holderness, 1086-1260: A Study in Feudal Society*. New York: Oxford University Press, for the University of Hull. 1979. Pp. xii, 275. \$49.50.

In this carefully researched book, Barbara English has provided a valuable addition to the growing collection of local studies for the earlier post-Conquest period (to ca. 1250) for which evidence is notoriously scanty and difficult to use. The lordship of Holderness was part of the patrimony of the counts of Aumale (in the thirteenth century, the Forz or Fortibus family), but it was always viewed and treated by them as a distinct, self-contained unit in a distinctively separate area of the English north. The book therefore has the added value not only of

dealing with a small- to mid-sized lordship but also of distinguishing the particular features of regional northern society over and against more general traits and tendencies.

The initial chapter provides a series of brief political narratives of the successive lords of Holderness down to William de Forz III (d. 1260) and his widow Isabella (d. 1293). This is followed by a series of rigorously institutional and descriptive chapters, in a descending social pattern, dealing with the administrative and the judicial organization of the lordship, knightly families and service, and a final section called "Land and People" treating agricultural communities, sheep farming, fisheries, and boroughs. The first two of the institutional chapters are the most successful. English is able to project administrative arrangements back from, and trace their origins forward to, the better-documented late thirteenth-century period, the age of Isabella de Fortibus, which forms the centerpiece of Noël Denholm-Young's classic *Seignorial Administration in England* (1937). On the judicial side she demonstrates the essential unity of the liberty, as grounded in its origins as a wapentake within Yorkshire, and shows with a good deal of specificity the workings of the privilege of return of writs. The chapter on knightly society similarly supplies useful detail on change in military service and on leading local families, in particular Fauconberg, St. Quintin, and Ros. The final chapter, although full of interesting material, is the least successful, being something of a catchall in a form reminiscent of H. C. Darby's historical geography series, albeit stretched over a period of two centuries. Not much is done with social dynamics—not that the quantity and nature of the evidence would have permitted much by way of such an approach in any event. Might it be hoped, however, that this book presages a larger, more socially oriented study of "The North" as region and as concept? Such a work would be welcome indeed, and on the basis of this book English would be well qualified to undertake it.

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JOHN M. GILBERT. *Hunting and Hunting Reserves in Medieval Scotland*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xi, 447. \$34.50.

Medieval kings were often obsessed by the hunt. It was such an integral part of ruling-class behavior that we are not surprised when the Anglo-Saxon Chronicle says of William I, "he loved the stags as dearly as though he had been their father," or when Geraldus Cambrensis says of Henry II, "would to

God he had been as zealous in his devotions as he was in his sports."

Hunting was more than just leisure or food gathering. It occupies a large chapter in the history of manners, for its rituals and language became part of the process whereby the aristocracy separated itself from the many. Its effect upon medieval economic, agrarian, and legal developments are well known, as the forests and chases became preserves for animals at the cost of men and women, farms, and even villages. Its effects upon bureaucracy and local government are significant, as mores and wishes were converted, by local agents, into a policy that would generally make the sheriff of Nottingham come out ahead of Robin Hood.

All these considerations make the hunt a choice topic for national and regional studies. John M. Gilbert has explored the topic in Scotland from its formal introduction in the twelfth century to the early sixteenth. He discusses the creation of the hunting reserves and the forest law, and he elaborates, in a long section on "Institutional Analysis," on the administrative and legal complexities that grew from the efforts to preserve privilege and monopoly. A long, scholarly appendix on the Forest Laws, with a textual genealogy and a translation, complete the volume's main coverage.

Gilbert shows how the story is really one of constant struggle: varying claims and interests had to be accommodated, and eternal vigilance was the price of continuous exploitation. There was the conflict of royal interest against that of all lesser folk: the extensive creations show who was the winner here. Then there was the conflict of two principles: the right to any animal one slew (since game were *res nullius* in Roman and Carolingian law) versus the right to control what others did on one's land—trespass versus the sanctity of property. Lastly, there was the economic pressure: in a relatively infertile kingdom, profits from arable land, grazing, and timber conflicted with the self-interest of holding on to the great preserves for their intended purpose. Long-range tendencies as well as ephemeral victories are plotted for us.

Two criticisms of this learned and intensive study must be made. Scholarship on the lesser kingdoms of medieval Europe should be at some pains to relate itself to the larger, better known canvas. There are references in the book to the English and continental practices and their influence but few references to the modern scholarship that deals with the topic in other parts of Christendom. Thus, the topic in its Scottish context lacks a comparative framework: it is unnecessarily insular. Furthermore, this book is based on a very traditional Scottish doctoral dissertation. It has almost too much scholarship, that is, too much for a 200-page text to carry or for us to assimilate: 1,700 footnotes, 32 tables, 10 maps,

19 plates, 3 appendixes, and a 17-page bibliography. A simpler and less inclusive monograph might have told us more: Charles Young's recent monograph on English forest law is closer to a holistic picture. The journal article is not an endangered species, and this volume would have been improved by editorial excision. The author does have a good eye for the general (for instance, pp. 255-265 on "The economic and social impact of the hunting reserves") when the parade of facts permits.

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MICHEL ROUCHE. *L'Aquitaine des Wisigoths aux Arabes, 418-781: Naissance d'une région*. (Bibliothèque Générale.) Paris: Éditions de L'École des Hautes Études en Science Sociales; distributed by Éditions Jean Touzot, Paris. 1979. Pp. 776.

This large volume is a product of the justly famous French École des Hautes Études en Sciences Sociales. It is not only a large volume, it is also a scholarly, ponderous one with about one-third of the book devoted to notes and bibliography. Considering that Michel Ruche is dealing with one of the most poorly documented periods in European history, one must conclude that he has exhausted the surviving written records of the time and sought out as thoroughly as possible whatever archeological evidence has been uncovered to date.

About one-third of the text of *L'Aquitaine* is devoted to establishing a political history from the time of the settlement of the Visigoths in southwestern Gaul in 418 to the coronation by Pope Hadrian of Charlemagne's son, Louis, as king of Aquitaine in 781. The guiding theme of the history is indicated by the subtitle of the book, "Naissance d'une région." Despite wide geographical and climatic differences from one part of the region to another, despite the greatly varying ethnic composition of the people, and despite broad linguistic and cultural variations in the population, Ruche argues that the historical experiences of the people of Aquitaine between the early fifth and late eighth centuries were such as to create a distinct region inhabited by a people who had emerged from the fusion of Celts, Latins, Visigoths, and Franks, with some Greek, Syrian, and Jewish admixture. The region had a strong sense of its identity and a language and culture sufficiently different from that of the rest of Francia to constitute a state with strong leanings toward independence. The time that this regional consciousness was evolving was one of supreme importance because it was also the period during which antiquity was transmitted to the Middle Ages.

Ruche's Aquitaine is an area bounded on the north and east by the Loire River (which does not reach quite as far east as the Rhone River), on the west by the Atlantic Ocean, and on the south by the Pyrenees Mountains. (It does not include the area along the Mediterranean, Septimania, which remained Visigothic after the Frankish conquest of Aquitaine in the early sixth century.) This large area covers nearly one-third of modern France.

The remaining portion of Ruche's work is social history of the *Annales* variety. Unfortunately, this is not a particularly successful social history. Although the region came to feel itself a distinct entity, it was composed of many smaller areas, so that in terms of social classes, land tenure, commerce, language, law, even religious saintly cults, it is impossible to arrive at generalizations that hold true for all of Aquitaine. But Ruche, who labored on this material from 1963 to 1978, has worked indefatigably in the archives and has unearthed a voluminous amount of information about life and customs in southwest France in the early Middle Ages. In spite of his efforts, our view of Aquitanian society is clear only at the top, and the lot of the little people remains obscure. The picture is nonetheless more varied than might be expected because, although the civil and ecclesiastical administrations were dominated by the powerful and influential families, commerce was not dead and communities of "oriental" merchants (Greeks, Syrians, and Jews) formed an important part of society and maintained links with other parts of the Mediterranean world.

This volume has been provided with a good index, an impressive bibliography, and a series of truly excellent maps on which a considerable amount of the evidence unearthed by Ruche has been plotted. Students of early medieval history will find this a very helpful work.

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WILLIAM CHESTER JORDAN. *Louis IX and the Challenge of the Crusade: A Study in Rulership*. Princeton: Princeton University Press. 1979. Pp. xv, 291. \$18.50.

William Chester Jordan examines Louis IX's policies from 1244 to 1270 from the viewpoint of the latter's two crusades. He contends that virtually all of Louis's public acts during 1244-48 were preparations for the crusade. Following his return in 1254, the king behaved according to an ideal that resulted from the failure of the crusade. His desire for self-purification influenced his treatment of nobles, bishops, popes, foreign princes, Jews, heretics, coinage, finance, and administrative reorganization. Louis's crusade-induced conversion compelled him to continue and expand his precrusade reforms.

Governmental institutions after his first crusade are best seen not as a development of impersonal structures but as an extension of Louis's firm hand, guided by a sincere wish to mete out Christian justice.

Jordan's analysis of Louis's administration is excellent. While his general thesis is not original, no previous historian has applied the crusade theme to Louis's reforms with as much thoroughness and persuasiveness. Without forcing the sources, Jordan uses the crusade to account for the monarch's interventions in tax collecting, temporal regalia, personnel selection, parlement, and the supervision of *baillis* and lesser officials. The author adroitly incorporates the work of others (such as Q. Griffiths) into his own conclusions. The best and most original parts of the book deal with the *enquêteurs*. Disputing earlier scholars, Jordan shows that Louis employed and monitored *enquêteurs* after 1254 to ensure that his subjects lived up to his high standard of morality. It is Louis's personal approach to government that partially explains the paradox of Louis's authoritarian rule and the relative absence of opposition to it.

The book is less satisfying when it attempts to connect Louis's administrative reforms to his ideal of kingship. The final chapter ("Most Christian King") fails to place Louis's idea of rulership into the context of mendicant spirituality (often misunderstood in the book) and crusade traditions. The book does not fulfill its promise to show how the views of contemporaries relate to Louis's notion of Christian kingship. Jordan's descriptions of how the monarchy's administration actually worked are never synthesized into the broader perspective of Capetian kingship. The important works of Buisson, Boulet-Sautel, and Congar are cited but not discussed.

The author's style is distracting. He misuses semicolons and colons. The sentence structure is often peculiar. Readers who use interlibrary loans will grumble at the absence of page numbers for the articles listed in the bibliography.

But the above criticism refers chiefly to omissions. As it stands, the book is a major contribution to our understanding of Capetian administration. More than previous critiques of Louis, Jordan's fine treatment has the merit of closely associating the king's will to the institutions under his control.

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CHRISTINE RENARDY. *Le monde des maîtres universitaires du diocèse de Liège, 1140-1350: Recherches sur sa composition et ses activités*. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège,

number 227.) Paris: Société d'Édition "Les Belles Lettres." 1979. Pp. 442. 100 fr.

Of all professions in the Middle Ages the *magister*, the university graduate, is the most easily identified. Like the present-day medical doctor or Jesuit, he rarely appeared in legal and public documents without his title. The diocese of Liège, comprising the principalities of Namur, Looz, and Brabant, although producing flourishing schools in the eleventh century, was scarcely a center of learning in the central Middle Ages. The region produced only a handful of reputed intellectuals (for example, Jacques de Vitry, Siger de Brabant, Geoffrey de Fontaines) and no major writings. As municipal archivist of Liège, however, Christine Renardy possesses unsurpassed access to the diocese's documentation, from which she has been able to identify and trace the careers of eighty-nine *magistri* from 1140 to 1200 and about seven hundred from 1200 to 1350. The details of these labors will be presented in a forthcoming *Répertoire biographique*. In the present volume she offers a collective biography of the *magistri* divided into two parts. Because of the paucity of information, the first part, treating 1140-1200, can only be tentative and sketchy, but the second part (1200-1350), aspiring to quantitative techniques, presents the following portrait of the liégeois *magistri*.

Most of them were born in the diocese of Liège, the Empire, or northern France. Only by the fourteenth century did significant numbers originate from southern France and Italy, because of the aggressive policies of the Avignese papacy. They came from predominantly bourgeois, knightly, and urban patrician families, but by the end of the period an aristocratic trend can be perceived. Except for a scant fourteen, all were members of the clergy, and an overwhelming proportion were regular canons. From the beginning, Renardy assumes that all *magistri* were graduates of Paris, Bologna, and Salerno, joined from the mid-thirteenth century by new universities such as Montpellier and Orléans. In my opinion this assumption is untenable for the twelfth century, when local schools also produced *magistri*. Of those whose universities are known in the latter period, most came from Paris, followed by Bologna. Almost half studied law; the rest, arts, medicine, and theology. While some served the emperor and the princes of the region, most received employment in churches. Beginning with the post of *officialis* in 1214, the *magistri* eventually monopolized the ecclesiastical dignities by the second half of the century. In the next century the popes used liégeois positions to support their curial staff as they did throughout Christendom.

Although produced with immense labor, such common features of the liégeois *magistri* offer few

surprises. Nor do they show the full picture, because they omit the mendicant friars, who were both numerous and educated but rarely appeared in public and legal documents. Encouraged by the example of early modernists, medievalists like Renardy persist in pursuing the quest of quantitative history despite the sparseness of medieval sources. Whether their results justify the effort remains an open question.

JOHN W. BALDWIN
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ROGER SABLONIER. *Adel im Wandel: Eine Untersuchung zur sozialen Situation des ostschweizerischen Adels um 1300.* (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 66.) Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. 231. DM 62.

It is refreshing to read a study on a medieval aristocracy that incorporates recent methodologies and questions of interest of both German and French historians, whose regional studies have developed within almost independent scholarly traditions. Roger Sablonier combines the best of both in tracing the evolution of the aristocracy of eastern Switzerland—an area bounded by Zurich, St. Gall, and Constance—from 1200 to 1350. He concludes that the aristocracy experienced a fundamental restructuring in the late thirteenth century because of specific social, economic, and political pressures and that aristocracies on both sides of the Rhine and as far west as Picardy shared a similar pattern of evolution.

In a well-constructed argument, Sablonier proceeds from a careful discussion of methodology—the use and meaning of titles, the relation between names and actual families and their possessions, the static image of the aristocracy in earlier local studies, and the central role of genealogies in any group biography of this sort—to an analysis of individual families and social groupings and finally to an examination of social changes and their causes. Working from a list of 44 noble and 220 knight families that appeared between 1200 and 1350, he finds that both groups were more stratified internally and more porous to outside recruitment than is often imagined and that the aristocracy was restructured by two general processes. First, the knights, most of whom had unfree ancestors, adopted noble titles and came to be considered as part of the aristocracy by 1300; that is, an aristocracy defined by blood lines in 1200 had expanded by 1300 to include all feudal tenants. Second, the total number of nobles and knights fell precipitously in the late thirteenth century, primarily because of economic difficulties and inheritance practices. As substantial economic resources were transferred to the church, to towns-

men, and to some knight families, partible inheritances systematically reduced the fortunes of many nobles and knights and resulted in widespread downward social mobility. One of the few opportunities for social and economic advancement was in service to the rising Habsburg territorial state, and by 1300 an aristocracy of service consisting of individuals from both noble and knight families had coalesced to form a new elite within the aristocracy. At the comparative level, Sablonier's study suggests that the most important determinants in the evolution of medieval aristocracies were their economic resources and the power of regional territorial states.

Minor criticisms might be made of the absence of genealogical tables and index, of the very brief treatment of aristocratic revenues, and of the reliance on figures calculated from nonserial sources. But the substance of Sablonier's contribution remains; he reminds us that the fates of individual families were not necessarily identical with those of the social groups to which they belonged and that fundamental social and economic changes did occur in the thirteenth century, which too often passes as a relatively stable period for aristocracies after their formative phase in the eleventh and twelfth centuries and before the crises of the fourteenth.

THEODORE EVERGATES
Western Maryland College

ERNST SCHUBERT. *König und Reich: Studien zur spätmittelalterlichen deutschen Verfassungsgeschichte.* (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 63.) Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. 419.

The subtitle describes this book's contents better than its title. Ernst Schubert has written a series of studies on the theory and practice of kingship in the late medieval German empire. In the first part of the book, he discusses the idea and ideal of kingship, the structure of the *curia regalis*, the king's relationship to the law, and the financial foundations of the Reich. In the second part, Schubert deals with a basic problem of the late empire: the relationship of *imperium* and *regnum*. He recounts the imperial tradition in the thirteenth through fifteenth centuries and the fate of the emperor's claim to be "lord of the world" when he might have been more correctly called "rex Alamanniae." The meanings of "rich" and "chunich" are explored, and the tensions between the two concepts are probed. Finally, the role of the *Stände* and the Reichstag in the late empire is examined. Schubert appends six short studies of related topics: imperial legends, the idea of nation in the fourteenth century, and "Oberdeutsch" and "Niederdeutsch," among others.

Schubert has filled each of his studies with a

wealth of material. He has synthesized the last fifty years of German scholarship for all the topics that he treats and has read widely in the sources. Although short in length, his chapters often make significant contributions. For example, his chapter on imperial mortgages supplements Landwehr's fundamental work. Schubert has produced a guide to the ideology and structure of the late German empire that will remain the standard for many years.

Sometimes his range of topics results in shallowness and mistakes when he treads on turf less familiar to him. With the exception of German scholarship, his coverage is spotty and his touch less sure. Although he has gathered together a formidable number of texts using expressions such as "plenitudo potestatis," "legibus solutus," "lex animata," and "cultor iustitiae," he is often vague about the canon and Roman law background of these terms. At times, he does not seem to grasp how medieval lawyers understood these concepts. To take one example: his discussion of "princeps legibus solutus" is marred by his failure to make a fundamental distinction of definition: the maxim can mean that the prince is "above the law," but it can simply mean that the prince's legislative sovereignty is not limited by earlier legislation. In this second sense, every legislator, constitutional or not, is "legibus solutus." It does not necessarily define arbitrary or non-constitutional power. Further, he does not distinguish between the prince's ability to change positive law and his subjection to natural law. Every medieval lawyer agreed that the prince was bound by natural law, yet Schubert argues that the lawyers began to reject the maxim "legibus solutus" in the late Middle Ages partly on the basis of a text that alleges a prince is bound by natural law (p. 122, n. 17). Consequently, although Schubert has collected many examples of German kings using the same terminology with which the canonists described papal power, his explanations of these concepts are sometimes off the mark.

In spite of these shortcomings, the book is an important contribution to the history of the late medieval empire.

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ULRICH REULING. *Die Kur in Deutschland und Frankreich: Untersuchungen zur Entwicklung des rechtsförmlichen Wahlaktes bei der Königserhebung im 11. und 12. Jahrhundert.* (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 64.) Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. 221. DM 49.

The work under review began as a dissertation that Ulrich Reuling completed in 1976-77. Its erudition is reflected in the extensive documentation and bib-

liography (pp. 210-21). Unfortunately, an index of persons and places is absent, and one wonders why such an important tool in a historical analysis was omitted.

Three large sections comprise the body of the book. In the first two, Reuling discusses the German and French royal election processes in the tenth and eleventh centuries. In order to evaluate Reuling's thesis, one must keep in mind that the *Kur* is only a portion of the election, namely, the actual vote. The entire process began with the deliberations of the important nobles of the realm as to who should be chosen, and ended with the act of paying homage. It was possible that the latter took at least a year or more, since the candidate had to secure the oath of allegiance from his subjects on a journey throughout the realm. Reuling maintains that the establishment of the Salian dynasty with the election of Konrad II in 1024 was crucial as far as the German election of kings was concerned. Prior to 1024, a king proclaimed his oldest son as heir, and the nobles simply agreed with his choice by an oral acclamation, followed by the coronation of the heir.

In the case of Konrad II, who was one of two major candidates, it became important for someone to exercise the *prima vox*, a task that naturally fell to the "Reichskanzler," who at the time happened to be the archbishop of Mainz. Thus, a precedent was established, and it was eventually in the archdiocese of Mainz that the elections were held and thereafter the archbishop who gave the "Kurspruch" or *prima vox*.

Because of these precedents the German kingship remained elective, a fact clearly discernible with the deposition of Henry IV (1076) and the election of the antiking, Rudolf of Rheinfelden.

As far as France is concerned, Reuling attempts to overthrow the well-known Ulrich Stutz (1910) theory. Stutz had claimed that the election process of 987 and the elevation of Hugh Capet had served the German nobles as precedent. This Reuling rejects since he feels that Hugh Capet could not legally proclaim himself because he was the major candidate and asked the archbishop of Reims to exercise the *prima vox*, a most unusual procedure. However, at the election of Philip I (1059), the then archbishop of Reims, Gervasius, attempted to establish a definite *prima sedis* and *vox* position and copied the German rules and customs of 1024 in his so-called *Memoriale*. In spite of this, the French crown became hereditary; the kings proclaimed their sons as *rex designatus* and the archbishop of Reims simply performed the religious aspects of the coronation. Thus, France evolved dynastic patterns, while Germany did not.

Reuling's arguments whether the German nobles copied the French, or the other way around, are somewhat weak. After all, both Reims and Mainz

were considered by most nobles and the papacy as *prima sedis* in their respective kirgdoms, and the documentary evidence does not clearly point one way or the other.

The last chapter begins with the effects of the investiture controversy on the German *Kur*. Reuling finds that the question of choice became a crucial element for the future and that the role of Mainz was clearly recognizable in the selection of Friedrich Barbarossa in 1152. Since the participation of the minor nobles and "populus" (*Gefolge*) became less significant, a "Reichsfürstenstand" was discernible with the selection of Otto IV (1208), and in time a public proclamation was considered sufficient to legalize the choice of the *Kur*. Furthermore, the candidates considered themselves as *rex*, with all legal rights, as soon as the *regem eligitur* had taken place and before the other ceremonies, such as *acclamatur*, *benedicitur*, and *coronatur*, had been performed. All this led the way for the establishment, in the fourteenth century, of the German "Kurkollegium" or Seven Prince Electors, for which we already have an excellent study by Mitteis, and Reuling offers no new insights to this topic.

Throughout the volume, Reuling is careful to emphasize the legal aspects of the election processes in both Germany and France, and it is a pity that reviews such as this, with a minimum of allotted space, do not permit a deeper analysis of this work. The book is well organized, very readable, and comprises an important contribution to the still necessary and ongoing research about royal elections in the Middle Ages.

TETA E. MOEHS

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IVAN BOZHILOV. *Anonimăt na Khaze: Bălgariia i Vizantiia na dolni Dunav v kraia na X vek* ["L'Anonyme de Hase": Bulgaria and Byzantium on the Lower Danube at the End of the Tenth Century]. Sofia: Izdatelstvo na Bălgarskata Akademiia na Naukite. 1979. Pp. 220. 2.30 lv.

In 1819, as notes to his edition of Leo the Deacon, Charles-Benoit Hase published three fragments by an anonymous author from, according to Hase, the tenth or eleventh century. In them a Byzantine official describes his alliance with one group of unnamed barbarians against other barbarians somewhere near the Dniepr River. Since 1819 scholars have debated who these barbarians were, where exactly they lived, and when these events occurred.

Recently Ihor Ševčenko (in *Dumbarton Oaks Papers* [1971]) concluded the text was a forgery perpetrated by Hase. Now Ivan Bozhilov, one of Bulgaria's leading medievalists, has published the text with a detailed commentary attempting to date

events and identify peoples and places. He believes the text is authentic.

Bozhilov concludes that the barbarian "emperor" north of the Danube with whom the anonymous author makes an alliance was the Bulgarian ruler Samuel (976-1014). Bozhilov treats Ševčenko's thesis only in an appendix. (For an English translation of this, see *Byzantino Bulgaria*, 5 [1978]: 245-59.) Though he meets certain of Ševčenko's objections, he ignores many others. Thus, whether or not the text is authentic is still unresolved. Clearly scholars should not make use of any information found therein until its authenticity is demonstrated. However, supporting authenticity, in my opinion, is the text's vagueness. Why would a forger not present a more important and interesting text and provide specific information to demonstrate whatever cause he sought to advance?

Granting authenticity, I also doubt Bozhilov's conclusion that a Byzantine official would ally with Samuel, a major Byzantine enemy. My own, admitted superficial, study of the text suggests it concerns Sviatoslav of Kiev's invasion (967-71) of Bulgaria. Sviatoslav's men would, then, be the ravaging barbarians of Fragment II, who caused the Byzantine official threatened by them to think—as the Byzantines actually did—of allying with the Bulgarian ruler (Peter or Boris II, who succeeded Peter during 967) against the Kievans. Peter held territory north of the Danube and his imperial title was recognized by Byzantium. However, before anything can be done with the text (including my Sviatoslav hypothesis), it must be determined whether or not the text is authentic; if it is a forgery all these other issues disappear.

Regardless of the text's authenticity, Bozhilov's thorough and scholarly commentary on the peoples and history of the region north of the Danube contains much of value. My main criticism of the commentary is its lack of a map.

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CLIVE FOSS. *Ephesus after Antiquity: A Late Antique, Byzantine and Turkish City*. New York: Cambridge University Press. 1979. Pp. x, 218. \$34.50.

Ephesus was a flourishing commercial center of the ancient world, famous for its temple of Artemis (Diana) and revered by Christians because of the visit of St. Paul and the longer stays of St. John and of the Seven Holy Sleepers. Its history in antiquity is well known. What happened to the city after that? This is what Clive Foss sets out to recount, and he recounts it extremely well. While making use of written sources, he relies chiefly on archeological evidence and also on personal observation. Many of

the excellent photographs in the book were taken by him.

After the late third century, the history of Ephesus and the structure of the book fall neatly into three periods: late antiquity, Byzantine, Turkish. As the excavations make clear, Ephesus continued to be a large and prosperous city. It was the site of one ecumenical council and of another that began as such. It was prominent in both civil and ecclesiastical spheres. Suddenly, however, ruin fell upon the city in the wake of the Persian invasions in 614. Ephesus devolved into a small fortified town, and, as its harbor silted up, it lost its importance as a port. The seven centuries of Byzantine rule were undistinguished, although Ephesus had some importance as a provincial capital. In 1304 the Turks moved in, making it the capital of their emirate of Aydin, and it again became a busy port, a commercial center, and a pirate base, known, because of its connection with St. John, as Theologo, Altoluogo, or Ayasuluk. Its prosperity continued after the Ottoman conquest of 1425, but then declined until, by the nineteenth century, it was almost deserted. Now known as Selçuk, it has recovered some of its former importance.

The story of Ephesus is well told and well illustrated in this book, which should be of interest to scholars in several disciplines. One hesitates to criticize a work of this quality, but a good map of the area would have been very helpful to the reader. There are some typographical errors, and, in note 82 on page 163, the more recent edition should be cited: *Studi Veneziani*, 12 (1970): 243-65. The relatively slim volume is attractively presented. The notes and bibliography seem complete, and there is a good index. All told, it is an interesting and exemplary piece of scholarship.

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MODERN EUROPE

ROBERT WOHL. *The Generation of 1914*. Cambridge: Harvard University Press. 1979. Pp. ix, 307. \$17.50.

Robert Wohl has written a remarkable comparative account of the intellectual impact of the First World War in five European countries. His study of "the generation of 1914" combines collective biography with intellectual history. The book presents both a narrative of the experiences of the Great War generation's educated middle-class elite and an analysis of the concept of generations as undertaken by several intellectuals in France, Germany, England, Spain, and Italy. As a "study of mentalities," Wohl's book recounts the individual and collective stories of intellectuals, most of whom are characterized as literary intellectuals, who consciously

created an image of themselves, or who were identified by others, as belonging to the generation of 1914.

The source material for the book has been drawn from a wide range of literary and scholarly sources as well as personal papers and interviews. In probing the contents of this material, Wohl seeks to penetrate the "poetic and political imagery" and the "propositions of social theories" with the purpose of determining which factors accounted for the emergence of the young intellectuals and what made possible the life experiences that inspired their literary and theoretical productions. Through this approach Wohl undertakes the task of sketching individual portraits of the intellectuals who embodied the regenerative "spirit" of this period and of sketching collective portraits of an important era in twentieth-century European society and history, in an effort to "rescue the generation of 1914 from the shadowland of myth and to restore it to the realm of history" (p. 2).

Wohl studies the careers and the writings of European intellectuals born between 1880 and 1900, tracing their development through the experienced effects of the war to the postwar period of the 1930s. He characterizes "the generation of 1914" as a specific instance of a generational idea—that of social or historical generations. Wohl contends that the notion of the "discontinuity of age-groups" was central to the generational ideas of certain of these writers and has a tendency to represent their generation as "unique, sacrificed and lost." Before the Great War youth began to organize and collectively to challenge adult authority, thereby evoking the dichotomy between the older generation and "youth." At the outbreak of the war, these young intellectuals perceived themselves as sharing a common destiny—that of national regeneration and of renewing its spiritual resources. Several of them perceived the war as promising to bring about desired changes in the future. During the first few years following the war, however, the returning young combatants were disillusioned and disappointed. The changes they had envisioned were not realized. The cities and villages to which they had returned in 1919 were different from those they had left in 1914. For the young veterans, a contemporary recalled in 1930, "disillusion came in with peace, not with war; peace at first was the futile state" (pp. 109, 225). Many of these intellectuals therefore were convinced that the world of their childhood was dead and that a new postwar world was being born.

Wohl portrays the most representative members of the generation of 1914 as individuals searching to accommodate their literary and theoretical ideals to the practical necessities of life. He concludes that a tremendous gap remained between society as dreamed by intellectuals and society as lived.

This is an imaginative and stimulating book. It rests on a solid mastery of historical literature and offers an analysis of "the generation of 1914" that is perceptive and persuasive.

OLIVER W. HOLMES
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CHARLES CRUICKSHANK. *Deception in World War II*. New York: Oxford University Press. 1979. Pp. 248. \$13.95.

Although this book is a comparatively minor work on the topic of military and wartime diplomatic deceptions and although the several parts are strung together like beads rather than having a consistently independent thesis, it does demonstrate that there are tricks to the trade. Charles Cruickshank shows that the Allies in the Second World War made many foul tips, hints, and suggestions—some of them very hazardous—to the enemy. Most of them are not, however, revealed here for the first time, claims in the introduction and on the dust jacket to the contrary notwithstanding.

Grand stratagems, the objective of which would be the turning of strategic flanks, either military or psychological or both, are not the author's principal preoccupation but rather operational deception and cover-up, including camouflage. Each campaign fought or planned by the Allies either jointly or separately—the war against Japan is not included—seems detached from the rest, and there is missing a cohesive element or substance that would give deception an independent status or quality, if only to convince persons not generally interested in the inner problems of the military profession that stratagems are as much a permanent feature, a *constant*, of warfare as combat itself. Without a doubt, the book is well written. It has a journalistic flair for movement, flowing without floating off the ground. Yet *Deception in World War II* might have included cover operations in the war against Japan or Japan's own most strategic deception operation on the psychological-political level—the Japanese peace mission to Washington in late 1941—which the author nowhere mentions.

Chapters such as "Deceiving the Invader," "Deception in the Desert," "Deceptive Operations, 1943," "The Overall Deception Plan, 1944," and "German Deception" pretty much tell the story. Still, without meaning to labor the point, the author never really deals with the *inside* of these operations, or of any others he handles, in order to reveal their really intriguing parts, how they worked, and just how they were planned or how they looked as planned. For example, Cruickshank says that "the notional Fourth British Army had been brought from Scotland" (p. 180) while the invasion of Nor-

mandy was in progress, but he does not describe for the nontrained reader how a "notional" division—it could be a corps, an army, or even a group of armies, as in the case of the twelfth and first army groups of the American armed forces—was, in fact, brought down. Moreover, at this late date these operations are nothing new. They have been adequately described in other monographs.

Apart from all that, this commentator is drawn to the conclusion, much against his free will but not against his better judgment, that the inability of this book to stand up to a work like *Bodyguard of Lies*, for example, is due in part to its too heavy reliance on the *highest* primary sources (such as "Air Ministry Correspondence," "War Cabinet Minutes," "Military Headquarters Papers, SHAEF," "Department of State," "Records of the U.S. Joint Chiefs of Staff," and the like) and not enough, evidently, on the field operational ones—of the units, in particular, that operated the "notions." If Cruickshank did use them, he must have decided to pass them over or cover them up, according to the more limited, but less interesting, purpose he had set for himself.

Good as far as it goes, it is not good enough, inasmuch as there is missing a *spirit*, if that is the right word for it in the matter of so hard-nosed a military theme.

ALBERT NORMAN
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D. M. PALLISER. *Tudor York*. (Oxford Historical Monographs.) New York: Oxford University Press. 1979. Pp. x, 327. \$32.50.

York was the traditional provincial capital of northern England until the Industrial Revolution and one of the country's largest cities. D. M. Palliser's excellent study begins with the city's regional role, its topography, and housing and describes its relationship with the state chronologically, moving from disorder and conservatism earlier in the century to acquiescence in the Elizabethan establishment. He describes the rather static municipal constitution and in a particularly useful chapter analyzes the nature of the ruling elite. The central section is concerned with economic issues—population size (about eight thousand in 1548 and eleven thousand by 1600, though the sources for York demography are more uncertain than most), epidemics, migration, and social structure are prominent themes. Freeman's records reveal an economy with a modest industrial element but strength in shop-keeping and service trades; commerce depended less on overseas trade *via* Hull than on interregional traffic by road and river. An effective chapter outlines the religious history of York, especially the im-

pact of the Reformation on a city stuffed with ecclesiastical institutions.

Very relevant to the continuing debate on the extent of urban "decay" at this time are the chapters that consider two distinct phases in the general history of York. The first, dated 1460–1560, was marked by depression and depopulation caused by the migration of most of the textile industry and the loss of much foreign trade. The author sees this phase continuing until 1560, but there does seem to be evidence of an upswing after the 1520s, masked by severe epidemics in the 1550s that were demographic rather than economic disasters. Palliser maintains a most judicious attitude to his evidence, distinguishing carefully between exaggerated self-interest and genuine complaint. The phase of recovery ran from 1560 into the seventeenth century. Elizabethan York found a new strength in its role as an administrative, social, and shopping center, and population, though perhaps not quite prosperity, returned to its old level. Despite the change and uncertainty of this century, the author rightly stresses the essential stability of society and the continuity discernible beneath the turmoil.

Palliser can spring few surprises on us, with substantial studies of his own and others on various aspects of York's history already in print; he works the separate elements into an admirably well-balanced and convincing whole, characterized by clarity of thought and writing and precision in the verification of detailed fact. The attempt to treat the subject both topically and chronologically leads to some unavoidable repetition, and many of the topics discussed need to be pursued by the serious enquirer back to the specialist's works and the author's dissertation: some readers will welcome the detailed references, but others might have preferred more of the detail to have been incorporated in a book that ranged less widely. Here is a welcome addition to the small number of model studies of early modern towns. It will be a standard work for urban historians and should supply valuable material for many other sixteenth-century specialists.

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JOHN BELLAMY. *The Tudor Law of Treason: An Introduction*. (Studies in Social History.) Buffalo, N.Y.: University of Toronto Press. 1979. Pp. 305. \$15.00.

"Treason" or "traitor" in various grammatical usages appear 345 times in the plays and poems of the immortal bard; the only works in which neither of the terms can be found are *The Merry Wives of Windsor*, *Midsummer Night's Dream*, and *Twelfth Night*. Admittedly, exactly half of the occurrences are in the nine historical plays, from *Richard II* to *Henry VIII*,

and in a number of Shakespeare's works in which the terms are used jocularly or figuratively (though one of the most hackneyed references, "I speak no treason," is from *Romeo and Juliet*, act 3, scene 5). Faced with terms of such common coin in Tudor England, in an age suckled on Macbeth's lament that "treason has done his worst" and the image of uneasy heads under usurped crowns, the historian of sixteenth-century treason has his work cut out for him. John Bellamy makes a fine try, but he is not quite "tickled with good success." It is not for want of research; there are no significant sources for Tudor treason (save, perhaps, literary and polemical works) that he does not command. This is admirable because until Bellamy searched the King's Bench materials they had not yielded their secrets.

The shortcomings of this introduction to the Tudor law of treason grow from the complexities of the subject. Treason, more specifically high treason, was a crime at law, with origins in common law—as Bellamy demonstrated in his earlier book, *The Law of Treason in the Later Middle Ages* (1970)—and development by statute. Under the Tudors, statutory efflorescence was prodigal, and although the author meticulously dissects the statutes that at various times comprised Tudor treason law, the two chapters devoted to "The Scope of Treason" lack a systematic categorization of the offenses and clear exposition of the often subtle distinctions among them. (Incidentally, 28 Henry VIII c. 9 is *not* one of the treason statutes as indicated on page 63 where it is an obvious misprint for 28 Henry VIII c. 7.) The author also fails to state, as he must, that high treason was distinguished from all other crimes against the state and individuals by its penalties—drawing, hanging, quartering; forfeiture of goods and chattels, lands and tenements; and corruption of blood—and not much else. An unfortunate consequence of this failing is that "misdeeds touching on treason" slips into "treason" (pp. 183–87), whereas the crimes actually treated are sedition and some embryonic subversions (the latter remain only a term of art in English law). Maintaining the distinction would also have avoided the misinformed and misleading point about the ineffectiveness of "conciliar proceedings" and "why no treason trials were staged under them" (p. 266 n. 1)—the King's Council, or more accurately, the Star Chamber had no jurisdiction in treason because it could not touch life or limb.

In the later chapters, when he turns to procedure and punishment, Bellamy slights the political context for all Tudor treason trials, whether of queens and great officers or seminarians and rebels. He attempts generalizations as to the justness of the treason laws and their enforcement by relying on generalizations about Tudor policies rather than the specifics of time and circumstances, the perceived

threat of the offences, and the danger posed by the offenders. This deficiency is compounded by a tendency to "try" the cases again, making large and as yet still unsupported assumptions about rules of evidence, the role of regal interest in the causes, and the function of the sixteenth-century jury. Too favorable a conclusion as to Tudor justice in treason cases is drawn, in part, from the wretched few cases of acquittal (the only notable one being that of Nicholas Throckmorton under Mary). Moreover, much of the discrete information, all of it valuable, provided in these chapters would have the appearance of greater reliability if the author demonstrated a broader grasp of sixteenth-century law. "Possessions" is a poor shortcut for the proper formula, goods and chattels, lands and tenements; "forfeiture" with respect to felony should not be used where escheat is meant; commissions of oyer and terminer and their operation are clearer than the treatment of them in chapter 3 would indicate; the use of torture and the development of pretrial examination receive more accurate treatment in two recent books by John Langbein (*Torture and the Law of Proof* [1977] and *Prosecuting Crime in the Renaissance* [1974]).

These criticisms voiced, it would be both unjust and ungracious not to acknowledge how useful this introduction to the Tudor law of treason is. It lays open the subject as nothing else before has, it is a genuine contribution to a still-neglected era in English legal history, and it invites further work. For years to come, it will repay close study. Every scholar of Tudor England will have recourse to it: "all the gift doth stretch itself as 'tis received" (*All's Well*, act 2, scene 1).

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STANFORD E. LEHMBERG. *The Later Parliaments of Henry VIII, 1536-1547*. New York: Cambridge University Press. 1977. Pp. ix, 379. \$34.95.

In the years 1529 to 1545 the English Parliament met sixteen times, virtually once a year, and filled the statute book with an unprecedented spate of legislation. Some of these acts dealt with the religious revolution that was being promoted by the king, but a great many others reflected the far-reaching plans of Thomas Cromwell and his collaborators. Altogether, the parliamentary achievement of these years marks a milestone in the history of the institution. In an earlier volume Stanford E. Lehmberg examined in close detail the history of the parliament that sat eight times between 1529 and 1536 (*The Reformation Parliament, 1529-36* [1970]). In this

work he continues his study through the remaining four parliaments elected in the years 1536 to 1547; they met for nine active sessions. Not surprisingly, much of their business was connected with the continuing problems of religious alteration, but the bulk of social and economic legislation was little diminished even after the disappearance of Cromwell in 1540.

Lehmberg has been meticulous in his research and has sought every fragment of evidence that he could uncover. The results of his labors are both tantalizing and satisfying. All too often we have a glimpse of what lies beneath the formal actions of the houses, just enough to suggest the realities of the power struggle at court or the aspirations of the country but not sufficient to allow us to confirm our guesses. Nevertheless, the author is able to give a convincing sense of the large directions in which events moved. Cromwell's continued management, both of elections and of legislation, is quite visible. More surprising is the continued flow of new legislation in the early 1540s after the removal of his directing hand. That governmental—that is, conciliar—impetus lay behind much of the legislation is apparent, but it is also clear that there was a counterthrust of opinion from the floor of the houses that often led to significant compromise. What is most startling is the complete absence of resistance to the extremely heavy taxation of the war years in the forties. The contrast with the massive (and successful) taxpayers' resistance in the 1520s stands out, and no easy explanation suggests itself.

Lehmberg's careful examination of the large body of private acts is particularly valuable since it gives us a much better sense of the actual functioning of the houses and a clearer vision of the way in which contemporaries regarded the parliamentary institution. The importance of Parliament's activities to the healthy functioning of the busy Tudor state stands out as does the fruitful collaboration between the crown and the county elites, on whose shoulders rested responsibility for the implementation of the measures devised at Westminster.

The author's careful attention to the details of procedure and of convention enables him to display an institution still in the making. Its habits are far from fixed; there is still experimentation and variation and an underlying uncertainty about the nature and functions in a time of radical change. The historian of these years is not blessed with the more abundant materials available for the Elizabethan era, but in many ways Lehmberg's restrained account of a story that can be told only imperfectly is more revealing of the quotidian character of the Tudor Parliament than the lively volumes of Neale's parliamentary studies.

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ROSEMARY O'DAY. *The English Clergy: The Emergence and Consolidation of a Profession, 1558-1642*. Leicester: Leicester University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xvi, 272. \$30.00.

Before the Reformation, Rosemary O'Day argues, England had no clerical profession in the modern sense of the term. Instead, there was a motley collection of graduate administrators and academics who happened to be in orders, largely nongraduate rectors and vicars serving parochial cures, and a host of ill-educated curates and men in minor orders who never held benefices of their own. Although bound together by a common dedication to celibacy and by the traditional conception of the clergy as a mediating priesthood, they lacked both a uniform career pattern and a shared background of professional training. The Reformation, however, led to a rather different and more unified conception of the clergy's function in society. Clergymen at all levels were now expected to be highly trained members of a pastoral ministry, a professional vanguard qualified to preach, teach, and interpret what the laity could only read. Through the efforts of reforming bishops, and through a tremendous expansion in higher education, this goal was at least partially achieved. By the 1640s, most clergymen were university graduates pursuing pastoral careers, and the clergy had become a highly trained profession with a stronger sense of vocational unity.

Although the author's case for professional consolidation is on the whole persuasive, it is probably a bit overstated. The clergy's continued cultivation of their glebe lands, together with the end of clerical celibacy, undoubtedly blurred the distinction between clerics and laymen, while Puritanism and a growing inequality in clerical incomes tended to divide the clergy from within. Even if in most respects the clergy now formed a modern profession, they were much less consolidated, socially and ideologically, than O'Day generally implies. Certainly the author goes too far in describing the seventeenth-century clergy as "one social group" (p. 189), when in practice there were still enormous variations in clerical life styles—variations that tended to coincide with the clergy's varied family backgrounds. More seriously, the author does not show us exactly what the clergy learned when they flocked to the universities or whether their newly won degrees actually made them more effective preachers and pastors. Additional research is clearly needed on the curriculum of England's early modern colleges.

The study is quite valuable, however, in documenting the ways in which a transformation of the clergy was at least attempted. In this regard, O'Day provides much useful information on the reforming activities of the Marian exiles on Elizabeth's episco-

pal bench, on the encouragement they and others gave to prophesyings as a means of furthering the clergy's learning, and on the stricter standards that were eventually applied to candidates for holy orders. She also fully investigates the extent to which reforms were impeded by an antiquated patronage system and by the crown's religious and political conservatism. Her findings may offer few genuine surprises, but they do deepen our understanding of the English Church and its struggle to adapt itself to the Reformation.

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B. G. BLACKWOOD. *The Lancashire Gentry and the Great Rebellion, 1640-60*. (Remains, Historical and Literary, Connected with the Palatine Counties of Lancaster and Chester, Third Series, number 25.) Manchester: Manchester University Press, for the Chetham Society; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. xiii, 184. \$25.00.

B. G. Blackwood has written an extremely well-documented, quantitative study of Lancashire's gentry, a very heterogeneous group. (The book contains 979 notes, 56 tables, and 4 appendixes.) Although the focus is on the 774 gentry families of 1642, Blackwood ranges over the whole seventeenth century. He retains a sense of perspective by frequently noting similarities and differences between the gentry of Lancashire and of other counties. Actually, Blackwood has superbly synthesized the state of our current knowledge on the entire English gentry during the seventeenth century.

Only a minority of Lancashire gentry families took sides during the Civil War. Royalist gentry outnumbered Parliamentary gentry about two to one. Compared to Royalist gentry, Parliamentary gentry were newer (postmedieval), more socially mobile, less wealthy, and more active in official capacities. Blackwood is refreshingly unique in that he avoids exaggerating minor and statistically insignificant differences, which the above were, between Parliamentary and Royalist gentry. In Lancashire only two major differences divided the gentry: education and religion. Parliamentary gentry were better educated, and most Royalist gentry were Catholic while most Parliamentary gentry were Puritan. Blackwood's contribution to the history of the Great Rebellion is to point out that religion, not economics, was the more divisive factor in Lancashire.

Those seeking evidence for a rising or declining gentry as a major cause of revolution may be disappointed. Both groups represented a minority of the gentry. And only 4 percent more of those rising sup-

ported Parliament than supported the king, and only 10 percent more of those declining or in financial difficulty supported the king.

Did the revolution lead to great social change? Political power shifted only slightly, and there was little redistribution of property. Blackwood argues that minor social changes, such as some increased participation by nongentry in local government, might also have occurred if the Royalists had won the Civil War. Slightly more than half of both Parliamentary and Royalist gentry declined between 1660 and 1695. The most important cause for the decline of Parliamentary gentry was exclusion from power after the Restoration, and for decline of Royalist gentry it was the lack of male heirs.

An invaluable by-product of Blackwood's tome is his inclusion of suggestions about which documents to consult and how to analyze them in order to determine social structure. Unquestionably, Blackwood has set the standard by which studies of the gentry of other counties will be judged. Rare are similarly sized books with as much data so concisely, lucidly, and persuasively presented.

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H. C. TOMLINSON. *Guns and Government: The Ordnance Office under the Later Stuarts*. (Royal Historical Society Studies in History Series, number 15.) London: Royal Historical Society. 1979. Pp. xiii, 268. \$40.80.

Early modern bureaucracy was an untidy blend of public and personal interest: appointment secured by patronage and purchase; use of personal servants and deputies; conflict of interest when officers supplied the goods they requisitioned; inadequate means of financing. Nonetheless it worked, and why it worked relates to the functions it fulfilled in England, providing offices and income to members of the political elite as well as carrying out government policy. H. C. Tomlinson has written a meticulous study of the ordnance office under the later Stuarts that addresses the question of how the department worked if not the way it related to the society it served.

In the later Stuart period, earlier practices in the ordnance office of family service, pluralism, and patronage continued. But major changes occurred. Life tenure, which was so conspicuous in early seventeenth-century government and proved so detrimental to reform under the early Stuarts, changed in the 1670s to office holding at pleasure. Salaries that began to replace traditional fees in the Cromwellian period were ended at the Restoration but reinstituted in the later 1660s. Purchase of office declined. Masters of the Ordnance subject to parlia-

mentary pressure rotated in office, but positions in the middle and lower ranks were increasingly stable as private servants became public officials and many of the technical offices were filled with qualified engineers. In short, the later Stuart ordnance office became increasingly professionalized. Tomlinson points out that most of the principal officers in the period were younger sons of the gentry and more than half of the chief officers had parliamentary connections.

Despite complaints by contemporaries and later historians that the ordnance office was corrupt, Tomlinson convinces the reader that confusion in command, lack of technology, and inadequacy of credit contributed greatly to the department's inability to fulfill its orders. As the bureaucracy expanded under the pressures of continuing warfare, the relation of departments became blurred. The manufacture of cannons, small arms, and gunpowder required lengthy processes that lagged behind military needs. After the Glorious Revolution, Parliament kept the ordnance office on a very tight budget, inadequate, Tomlinson argues, to the needs of the wars in which England engaged itself.

If there is a caveat about this carefully researched book, it is that it does not raise questions that go beyond the office itself. Why did the bureaucracy become more professional in the late seventeenth century? Although recent work on Colbert suggests that he too had to work within the limits of early modern bureaucracy, to what extent did English administrators try to emulate the French? Or did strategies of the early seventeenth century and the Cromwellian period shape later administrative practices? What was the complex interrelationship of war with the development and structure of bureaucracy? How did the connections of the officers with Parliament and the landed aristocracy relate to the politics of the period? Tomlinson has nonetheless provided a very useful picture of the work of the ordnance office and the pressures with which it contended in late Stuart government.

LINDA LEVY PECK
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EVELINE CRUICKSHANKS. *Political Untouchables: The Tories and the '45*. New York: Holmes and Meier. 1979. Pp. vi, 166. \$27.00.

In *Political Untouchables* Eveline Cruickshanks makes the case as eloquently as she can for identifying the Tory Party as Jacobite between 1715 and 1745. For those who know her chapter in the History of Parliament volumes directed by Romney Sedgwick, this will come as no surprise. The prospect of a Jacobite threat as a military challenge to the Hanoverians is one matter, the extent to which the Jacobites dominated the Tories another. Here Cruick-

shanks is contested by B. W. Hill (1976) and most other scholars of the period who are disinclined to accept that the party rank and file were committed Jacobites. Moreover, Cruickshanks (pp. 26ff.) maintains that the Tories (Jacobites) refused to cooperate with the opposition Whigs at crucial moments, thus saving Walpole, while Hill contends (pp. 227–28) that in the final analysis it was the opposition Whigs who would not make common cause with the Tories when it could mean the destruction of a Whig government, albeit one led by Walpole.

There is no question that many Tories and even some Whigs professed an attachment to the exiled Stuarts. To translate this verbal flirtation with the Jacobite cause into avowed support for military action against George I or George II is another matter. Here a natural skepticism must prevail. The actual number of those who would even commit themselves in writing as revealed by Cruickshanks's indefatigable labors is not great. Moreover, none of these were prepared to act without direct military support from France. There is the rub. Inasmuch as that military support was either not forthcoming or on the several occasions when it was planned the invasion was aborted, the sentiments of the English Tories were not really put to the test. Certainly the two uprisings of 1715 and 1745 did not reveal widespread support for the Jacobite cause in England. The issue of military assistance is critical. This was clearly recognized by the governments of George I and George II, for there were so few standing troops in Britain that a well-organized invasion force could easily overwhelm their defenses, however loyal the people.

Cruickshanks has employed some interesting new material, notably the diary of Dudley Ryder, the attorney general, to illustrate Walpole's own concern. It seems to have been real enough. The fact that the prevailing sentiment of the electorate was Tory rather than Whig is also undeniable. But even Cruickshanks must admit: "How widespread was the wish for a restoration at this time [1745] is not a question that, on the face of it, can ever be answered" (p. 45).

Cruickshanks has performed a useful service in identifying those Tories with Jacobite sympathies and chronicling their flirtations, their expressions of support, and their activities on behalf of the Pretenders. She also explains reasonably how Tories, banned from public office for several generations by Hanoverian sovereigns, might naturally seek favor from the only alternative source, the Pretender. But to brand the whole Tory Party as Jacobite on the basis of protestations without substance and negative evidence (pp. 45–46) strains the evidence too far. In the final analysis Cruickshanks herself is too good a scholar to maintain that.

HENRY L. SNYDER
Louisiana State University

FRANKLIN WICKWIRE and MARY WICKWIRE. *Cornwallis: The Imperial Years*. Chapel Hill: University of North Carolina Press. 1980. Pp. xi, 340. \$16.50.

With this volume Franklin Wickwire and Mary Wickwire conclude their study of Cornwallis, which they began a decade ago with *Cornwallis: The American Adventure* (1970). We now have for the first time a complete account of the career of this curious and interesting figure. The authors have chosen a rather traditional "life and times" approach to their subject and make little attempt at the now fashionable probing of the psyche. There is, however, a tantalizing hint of stress and complexity underlying the stolid exterior in the way Cornwallis pined for his Suffolk estate whenever he was abroad but was invariably restless to be away again once actually there.

During the years covered by this volume Cornwallis filled a number of important appointments. The two most significant were governor general of India (1786–93) and lord lieutenant of Ireland (1798–1800)—striking testimony to the fact that the political world never allowed the memory of Yorktown to shake its confidence in him. That confidence was not based on his abilities—the authors bluntly rate his brains as second class—but rather on his character. He was a Victorian before the fact, devoted to duty, crown, and empire and much given to expressing himself on those subjects with a truly Curzonian ponderousness. Cornwallis, in fact, foreshadowed the proconsular era to come when gentlemen amateurs, "all-rounders," presided with justice, firmness, and detached complacency over those the empire was called upon to rule. Much of the success of the "Cornwallis reforms" in India (success, incidentally, much less complete than the authors are disposed to allow) was due to Cornwallis's good luck in finding like-minded proto-Victorians in the ranks of the East India Company's civil administration—men like John Shore, Charles Grant, and Jonathan Duncan. They provided much of the drive, and most of the ideas, behind the governor general's reforms, and they remained concerned with Indian administration and its improvement after Cornwallis had moved on to other things. It is interesting that where Cornwallis's style did not evoke similar answering echoes—his dealings with the officer corps of the company's army or, later, with the ascendancy in Ireland are cases in point—his success was minimal.

Cornwallis's imperial career, especially its Indian chapter, is a revealing symptom of the shifting attitudes that were about to transform the colorful, buccaneering mercantile empire of the eighteenth century into the aloof, high-minded, bureaucratic structure over which his imperial successors presided. The Wickwires have given us a very good case study of this sea change in the way the British

viewed their overseas possessions, a useful contribution to the literature on the subject even if at times it does seem a bit marmoreal.

RAYMOND CALLAHAN
University of Delaware

CLIVE EMSLEY. *British Society and the French Wars, 1793-1815*. Totowa, N.J.: Rowman and Littlefield. 1979. Pp. viii, 216. \$19.50.

The years from 1793 to 1815 are crucial in shaping modern British society. Yet the domestic history of Britain during the wars with France at the end of the eighteenth and beginning of the nineteenth centuries has been a neglected subject. When historians have dealt with these years they have usually focused on the impact of the Industrial Revolution. Clive Emsley provides a fresh perspective: to him the wars themselves supply the key to understanding important changes in this period.

It is tempting to argue with Emsley's contention that Britain's wars with revolutionary and Napoleonic France provided more of a "common experience" for the British than did the Industrial Revolution during the same years. But such argument is fruitless, for, as this book clearly if unintentionally demonstrates, it is impossible to separate the effects of war and economic change. They are hopelessly intertwined. Still, even if Emsley fails to show "how far changing attitudes may be attributed, at least partly, to the wars" (p. 4), he has written a valuable book.

Emsley has drawn rich material from government documents, newspapers, novels, caricatures, and especially from local record offices. His research is prodigious, and he has used it well. The writing is clear, the examples well chosen and illuminating. In only 182 pages he ranges over an astounding variety of subjects. He is concerned with the response of the central government, with the demands war placed on local administration, with recruiting for army, navy, and militia, with the fluctuations in the economy, with Martello Towers and the impact of taxation, with lower-class unrest and middle-class organizations, with patriotism and propaganda, with Ireland, indeed, with everything from the collection of statistics to the threat of revolution. Every reader will object that some pet subject has been ignored. There is little here, for example, on women, or on the dissenters. But what is remarkable is that Emsley manages to discuss so much so well in such a brief book.

Still, the central problem of measuring the effects of the wars remains. And while Emsley does establish that these wars were different, more nearly "total" than earlier eighteenth-century struggles, what is surprising is how little long-term impact such a major conflict produced. Some administrative re-

form was forced on the national government, though perhaps no more than would have occurred in peacetime. Local government, despite the unprecedented tasks assigned it during the wars, quickly reverted to "its easy going eighteenth century pattern" (p. 178). Emsley provides interesting evidence on the rising consciousness of both working and middle classes, but whether this resulted directly from the wars remains unproven.

D. W. Brogan's famous comment on Jane Austen notwithstanding, the French wars did affect the lives of Britons of all classes. How much basic change, in attitudes or institutions, they produced is another question. This question, despite Emsley's admirable effort, remains unanswered.

JAMES L. MCKELVEY
University of Connecticut

MAXINE BERG. *The Machinery Question and the Making of Political Economy, 1815-1848*. New York: Cambridge University Press. 1980. Pp. x, 379. \$35.00.

This is an ambitious book. Early industrial machinery and the origins of political economy are hard topics to relate. Maxine Berg tries to do this and argues that political economy was a product of the machine age. While the desirability of machinery was being debated, the discipline of political economy was being formed.

A down-to-earth subject such as technology and a highly abstract one such as political economy are difficult to reconcile. It is the chief merit of Berg's book that this assignment has been accomplished in a generally successful fashion. It is no easy task to deal with characters as different as the subtle David Ricardo and the nearly illiterate George Stephenson. In this respect, Berg's book is vital and breaks new ground.

Berg is at her best when dealing with political economy. The short chapter on the radicals is her worst since much of it is devoted to the socialist, Robert Owen, and it does not consider most of the major figures in the radical political movement. Because she sees William Cobbett as a radical, Berg is uncomfortable with his views on machinery; but when we consider Cobbett as a utopian reactionary, his attitude toward machinery becomes intelligible. Similarly, a person who is not mentioned in the book, John Cartwright, was able to combine an appreciation of machinery with a sturdy middle-of-the-road political radicalism. Berg has a shaky grasp of radical and working-class history, and it leads her to misspell consistently the name of Gravener Henson. Also, the two last chapters in the book on social reformers and Engels and J. S. Mill are not as detailed as they should be in the light of their importance to her argument. The book badly needs a good concluding chapter, which should

summarize a work that deals with issues as complex and wide ranging as this one.

It is impossible to give Berg credit for completely succeeding in her effort. But on her behalf I am happy to say that she has set herself a difficult task for a historian born in 1950. The book has developed from an Oxford doctoral thesis, and it is refreshing to find a young historian who is not afraid to tackle the big questions. There is much that she does not yet know about late Hanoverian and early Victorian history, but her book deserves an alpha for ambition, even if it rates a less high mark for accomplishment. As Robert Browning said, a man's reach should exceed his grasp, else what's a heaven for?

JOHN W. OSBORNE
Rutgers University,
University College

NORMAN GASH. *Aristocracy and People: Britain, 1815-1865*. (The New History of England.) Cambridge: Harvard University Press. 1979. Pp. 375. \$20.00.

This is one in a current series, "The New History of England," edited by Norman Gash and A. G. Dickens. This volume is a substantial work of scholarship that attempts to synthesize the recent contributions to the study of the early nineteenth century. Norman Gash reinterprets the period of the Industrial Revolution by giving more adequate attention to the growth in population, the rise of cities, and the economic difficulties attending the wide fluctuations in the business cycle. Heretofore these subjects have been subordinate to industrialism and the rise of the factory system, an interpretation that disregarded the continuing predominance of agriculture.

Norman Gash's new synthesis is a challenge to the older interpretations that stress social conflict and the class struggle, which were supposed to have characterized the period of the Industrial Revolution. Gash denies the existence of the "two nations"—the rich and the poor—as depicted in Disraeli's novel *Sybil*. Instead of this stark distinction between two hostile classes Gash thinks "An immense and complex gradation of classes stretched from the very rich to the very poor" (p. 2). Gash contends that social divisions were vertical, rather than horizontal, running downward through the rich and poor alike. The historical gulf separating Anglicans from Dissenters, he thinks, was greater than any division separating social classes. Rather than celebrate the triumph of the middle classes, Gash features "the success of the aristocracy and the gentry in retaining both the substance of their traditional political power and the social deference of other influential classes" (p. 8). The inherent social

solidarity, Gash argues, saved England from the revolutions that occurred on the Continent in 1830 and 1848.

The author has organized his complex materials into two introductory analytical chapters, seven chronological chapters, one chapter on war and foreign policy, and a final summary chapter and conclusion. The two general introductory chapters should be required reading for students of American history because they so clearly describe the social and economic conditions of the people, their government, and their religion. Although most Americans know the general features of the central government and the parliamentary system, they inadequately comprehend the complexities of local government as conducted by parish officials and the justices of the peace. Gash not only treats amply the system of local government but also describes clearly the organization of religion, explaining the decline of the established church and the rise of the dissenting denominations. This careful study will surely become required reading for students of English history and will displace some of the previous accounts of the period that lack Gash's sympathetic understanding of the whole of British society.

R. G. COWHERD
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MICHAEL DUREY. *The Return of the Plague: British Society and the Cholera, 1831-2*. Dublin: Gill and Macmillan or Humanities Press, Atlantic Highlands, N.J. 1979. Pp. 269. \$37.50.

Despite its title, this is a calm and unemphatic survey of the immediate impact of the first Asian cholera epidemic in Britain in 1831-32. Michael Durey's approach typifies a peculiarly British way of handling the subject. From the informed, measured contemporary narratives by Gaultier, Greenhow, and Shapter to the admirable modern studies by R. A. Lewis (1952), Norman Longmate (1966), and R. J. Morris (1976), the accent has been on the stability and resilience of British society; by contrast, French writing on the subject is replete with bizarre alarums, excursions, and forecasts—unfulfilled—of national doom. Of course the cholera in Britain was much less destructive, and it engendered much less fear, misery, and violence than in France, Hungary, and Russia. Nonetheless, it killed about thirty-one thousand people in Britain during the year beginning October 1831.

Many of Durey's findings about the spread of the disease, its devastating impact on small communities, and the chicanery of local mercantile interests in suppressing reports of outbreaks and, in seaports especially, of resisting quarantine are familiar from the work of Longmate and Morris. But

Durey's attempts to test French theories about cholera as a cause of unrest and the assumed relationship between the cholera and the Reform Bill agitation provide new insights. He argues persuasively that cholera scares had little to do with the reform agitations because the cholera twice abated just when the agitations became fiercest, while the radicals failed to give a political thrust to the anxieties about cholera among the middling and working classes. He shows, too, that cholera had no apparent links in 1831-32 with industrial struggles. Hence Durey implicitly rejects, so far as Britain is concerned, Louis Chevalier's hypothesis about cholera inducing political turbulence among the dangerous classes. Such unrest as did develop arose from fears about body snatching and the Anatomy Act rather than from cholera. Durey's case is convincing for 1831-32, but one wonders about longer-term effects, particularly in relation to the popular opposition to the New Poor Law in the mid-1830s.

Durey also has a fresh approach to the medical men and their treatments. He shows why intravenous saline therapy was resorted to so hesitantly, and he exonerates the surgeons of overzealous blood letting by lancet, although he ignores the likelihood that this procedure, amid the filth that accompanied cholera, must have transmitted serious cross-infections. He raises but unfortunately does not develop the larger issue of governments having to act on expert advice when the experts lack the solutions and have an interest in disguising the results of their advice and in shifting the blame. But perhaps this legerdemain is a precondition of social resilience.

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JOHN M. EYLER. *Victorian Social Medicine: The Ideas and Methods of William Farr*. Baltimore: Johns Hopkins University Press. 1979. Pp. x, 262. \$19.50.

The subtitle of this book accurately describes its contents, for it is, first and foremost, a study of the thinking and methods of the British government statistician and social thinker, William Farr. Born in 1807 and trained for medical practice, Farr taught himself elementary mathematics and statistics and, in 1839, went to work in the recently created General Register Office, where he remained until his resignation in 1880. John M. Eyler's study is not biography but intellectual history, the exploration of Farr's ideas and his contributions to the sanitarian movement and to the emerging science of social statistics.

Eyler devotes one chapter of his work to a description of the state of statistics in the 1830s, when Farr entered this field of endeavor. He then focuses his lens more closely on the General Register Office

and the character of official records of birth and death in the early Victorian years. The remainder of the study explores Farr's thinking in detail. Eyler pays special attention to several components of Farr's work: the importance of the life table and actuarial methods in Farr's statistical analyses; Farr's medical ideas, especially his theories of disease; his treatment of environmental factors in morbidity and mortality; and his role in sanitary reform in the army and hospitals. Integral to Eyler's treatment is the way Farr brought together ideas about medicine, social science, and social policy.

Eyler argues convincingly for a vision of Farr as a proponent of an objective statistical and social science. In so doing, he offers a useful corrective to the view that early Victorian statisticians were merely reform propagandists or self-serving bureaucrats, as suggested in M. J. Cullen's *Statistical Movement in Early Victorian Britain* (1975). Perhaps in reaction against what he sees as Cullen's reductionism, Eyler underplays the importance of the social and institutional environment in which Farr moved. Attentive, for example, to Farr's medical theories, Eyler seems to underestimate the importance of the rifts and rivalries in the medico-professional world that might have influenced Farr's ideas and shaped his experience as a general practitioner-turned-bureaucrat dealing with the titans of the Victorian medical establishment.

Because of Eyler's meticulous research and his detailed exploration of Farr's assumptions as well as his statistical methods, his work should provide a useful guide to scholars working with Farr's voluminous publications and the records of the General Register Office. (The usefulness of this work would have been enhanced by including a guide to the charts and graphs reproduced in the book and by a more thorough index.) Readers will also find in Farr's story an important chapter in the history of medical statistics, Victorian public health, and the development of the social sciences in Britain.

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A. J. YOUNGSON. *The Scientific Revolution in Victorian Medicine*. New York: Holmes and Meier. 1979. Pp. 237. \$22.50.

Despite the implications of its title, this is, in fact, a study of the reception of anesthesia and antiseptics in England and Scotland, ca. 1846-80. While these innovations are hardly new historical materials, A. J. Youngson is correct in recognizing that there are no other short comparative histories that promise broader generalizations about the process of innovation in medicine. His book is short and clearly

written. The sampling of early British opinions and arguments about anesthesia and antiseptics drawn from the medical press—mainly *The Lancet*—is well chosen and usually adequately explained. This survey of published medical opinion is the strongest part of the text.

As the author moves away from surgical procedures and medical polemics, however, his account weakens. His interpretation ignores several important recent historical studies of the Victorian medical profession and its institutions. Youngson also shows a regrettable lack of sympathy with past biological and medical theory and with what he sees as the inability of Victorian medical men to "think scientifically." He continues: "One wonders sometimes if many of them thought at all" (p. 17). His account is the least convincing in generalizing about the relationship of science to medical innovation. Anesthesia seems a poor choice because, as the author himself explains, this innovation involved no theoretical change. Victorians had no knowledge of how or why anesthetic agents produced their physiological effects, and the early development of anesthesia was largely a matter of empirical tinkering in surgical and obstetric practice.

The advent of antiseptics is a much more appropriate case study because, as Youngson quite properly insists, the successful use of antiseptics required a fundamental conceptual change, nothing less than the acceptance of the germ theory of disease. Even though he gives considerable attention to the reception of Lister's methods, the author does not adequately explore either the process by which this intellectual change was effected or the nature of that change. We are told little about the previous medical theories of inflammation, although at least one of the spokesmen Youngson quotes refers to the contrast of Hunter's and Lister's views on this subject (p. 222). Youngson also treats the dominant anticontagionist, miasmatic, and chemical theories of diseases cursorily.

In his final chapter Youngson proposes a scheme or model, drawn from his two case studies, to explain the reception of medical innovation. Of the ten key factors he identifies, only two, he concludes, are primarily scientific. The others are social or cultural. Despite this realization, the author has chosen an approach that severely limits his ability to analyze those forces not addressed directly in the Victorian medical literature. A striking illustration is Youngson's discussion of the significant fact that greater resistance to anesthesia was encountered in obstetrics than in surgery. A "miscellaneous assortment of medical, sociological, and ethical and religious objections" is blamed for the lag in obstetrics. The suggestion is raised that Victorian attitudes toward women might have been the crux of the matter (p. 215), but in a few sentences this important issue is allowed to slip away unexplored.

The work also displays evidence of careless editing: nineteenth-century events are occasionally transported into the twentieth century (pp. 64, 75), and words are senselessly repeated in quotation (p. 195). Misstatements of fact, such as the century in which the stethoscope appeared (p. 19) or the name of the originator of cellular pathology (p. 20), may perhaps be explained similarly.

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TONY MASON. *Association Football and English Society, 1863-1915*. Brighton, Sussex: Harvester Press or Humanities Press, Atlantic Highlands, N.J. 1980. Pp. x, 278. \$36.25.

In the occupational table on page 93 the author heads the skilled-worker column with the trade of mason. This book justifies such an apparent case of egotism! One reason for this being the best published work on English soccer is that Tony Mason has made extensive use of previously underused source material. For a start he has ploughed through the sporting press, a time-consuming and frequently boring task: how many pages had he to turn before tedium was alleviated by the Lancashire cup-final referee who officiated beneath a raised umbrella or by the advertisement for a professional who could play music as well as soccer? Another mainly untapped source was the shareholders' and directors' lists of clubs that adopted company status. Mason uses these effectively to examine the occupational background of those who owned and controlled professional teams. (The same files at Companies House, however, also hold many annual reports and financial statements that he virtually ignores.) Although access was granted to the records of the Football Association, the Football League, and the Professional Footballers Association, the clubs themselves proved difficult to get into. Nevertheless, Mason was able to obtain data from Derby County and from Aston Villa, possibly the most successful of the pre-1915 clubs.

Mason blends this material into a highly readable history covering such aspects as the origin and development of professional clubs; the players, including an original section on schoolboy soccer; and the size, composition, and behavior of soccer crowds. He also produces a useful survey of the sporting press. Primarily, however, he is concerned with the degree to which soccer was a working-class game. His task is not easy. Apart from the injury list at the Ibrox disaster—and this was in Scotland—he was unable to improve on the many contemporary observations that the spectators were mainly working class. As for players, he discovered the occupations of only 165 professionals, though this limited

sample was dominated by skilled workingmen. His analysis of shareholders reveals that the middle class was in the fore but that a significant proportion of shares was held by workingmen. Here Mason's presentation hampers his discussion: a table dealing with one club, an unsuccessful one at that, and a narrative mentioning some others is a less-than-perfect substitute for a table dealing with all clubs. Turning to the power structure in soccer, he shows that club directors, like the Football Association Council, were clearly middle class, though he highlights a possible dichotomy in their attitude toward professional soccer.

Finally, I have two minor quibbles. First, photographs and maps are left to stand by themselves with little reference in the text. Second, Mason is somewhat dismissive of theoreticians without justifying his stance, though perhaps his empirical study par excellence is sufficient grounds.

WRAY VAMPLEW

Flinders University of South Australia

F. D. DOW. *Cromwellian Scotland, 1651-1660*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xii, 361. \$37.50.

F. D. Dow has provided us with a detailed narrative of the Cromwellian occupation of Scotland from the conquest following 1651 to the restoration of the monarchy in 1660. The narrative fleshes out the picture created three quarters of a century ago by such distinguished scholars as Gardiner, Firth, Terry, and Douglas. Like these earlier scholars, Dow's viewpoint (though not her sympathies) is that of the army and the English authorities. Again like these scholars she approaches the Cromwellians not as religious radicals or even as social revolutionaries but as administrators and soldiers.

Not surprisingly in these circumstances, Dow does nothing to alter the long-established view of the Cromwellian experience in Scotland. The English forces initially expected to establish the Commonwealth in Scotland by freeing the commoners from the nobility, the greater gentry, and the ministers. This policy failed completely, and its most visible result was to drive financially strapped gentlemen into revolt with Glencairn. Subsequently, less radical policies were pursued by Monk and Broghill, which produced an accommodation with many of the traditional elites. Significant elements within these elites became increasingly involved—with the army—in a system of local and central administration that at times worked remarkably effectively. At the same time efforts to establish the Gillespie wing of the divided kirk consistently failed, but by the latter half of the decade the government found itself courted by all factions while becoming the captive

of none. The ultimate testimony to Cromwellian success was, ironically, Scotland's stability just before and during Monk's march south into the disintegrating English republic.

Now all of this is very familiar ground indeed—and ground from which Dow can never bring herself to depart. She never wonders why the covenant was so centrally important to Scottish saints, in marked contrast to the English saints who were their conquerors. She never wonders how Scotsmen understood the meaning of their massive misfortunes, although they certainly spent a lot of time thinking and talking about it. Nor does she ever ask how Englishmen interpreted their stupendous success. Where in the world, we might wonder, did they come up with the idea that the first task of the army was the liberation of the Scottish commons? If the aspirations of the Cromwellians in Scotland emerge only in the most elementary way, those of the Scottish Cromwellians barely emerge at all. It is regrettable that Dow did not consult the Linlithgow manuscripts and James Hope's correspondence in London and Edinburgh: if nothing else the material indicates that the Scottish role in "settling the judicatures" was rather greater than Dow suggests. The mind of Warriston remains as elusive to Dow as it was to his editors early in this century. Argyll is as much an enigma as he was to scholars seventy-five years ago, and we must await Edward Cowan for a serious, modern study of the great marquis. The story repeats itself with Jaffray, Gillespie, Strachan, Kerr, Rutherford, and so many of the figures of the age. We have the bare events, but we are far from the world of either conqueror or conquered, covenanter or collaborator. *Cromwellian Scotland* offers a meticulous and competent narrative, but it does little of moment to carry us beyond the great pioneers.

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ALAN B. CAMPBELL. *The Lanarkshire Miners: A Social History of Their Trade Unions, 1775-1874*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xi, 354. \$45.00.

This book is the most detailed social history of workers in a single British industry to be written so far by one of a group of able young British labor historians who were trained in the 1970s at the Centre for the Study of Social History at the University of Warwick in England. Alan B. Campbell's argument, briefly stated, is that the history of coal-mining unionism in western Scotland from 1775 to 1874 can best be seen in terms of the rise and fall of the philosophy and work habits of what he calls the independent collier. This phase began with the

Emancipation Act of 1799, which delivered Scottish miners from their neo-feudal status of "degraded slaves" under the pre-existing system; it ended in the 1870s when the policies associated with the "honorable men" position, which the miners had built up, became intellectually bankrupt as well as impossible to enforce in a rapidly changing, capital-intensive industry. This enables the author to show us the need to view the miner not as an unskilled, brutish proletarian, besotted by drink and isolated from all humanizing influences—a vision commonly held of him by middle-class opinion in the nineteenth century—but as a proud, highly skilled worker practicing an ancient craft and struggling to defend his dignity and workplace autonomy in the face of an increasingly systematized, machine-oriented, and bureaucratic form of enterprise. This central aspect of the author's argument is admirably and convincingly conveyed as he describes for us the separate contracts into which each collier entered with the mineowner, his hiring of his own underground helpers, and his refusal to accept workplace discipline. Admirable, too, is the way in which Campbell explains the collapse of this independent tradition, which resulted from—among other things—the impractical use of selective strikes by British National Miners Association President Alexander MacDonald and his excessive reliance on the employers' goodwill in the depression of the mid-1870s. The careful analysis of wage rates and intelligent use of census materials to give us a statistical portrait of several of the mining villages around Glasgow, are also to be commended.

This last section of the study, however, points to some faults. Campbell uses a comparative analysis of two mining and iron-making towns near Glasgow, Coatbridge—with its rapid growth and its large influx of Irish—and Larkhall—with its fewer immigrants, slower growth, and hence more stable union—to make several of his points. Here, a somewhat deeper, community-oriented analysis of friendly societies, leisure pursuits, diet, housing, and other extra-workplace activities and of the physical and geological attributes of the mining seams themselves (which profoundly affected work habits and wage levels) would have enlivened the narrative as well as strengthened the author's case.

For American readers the most suggestive parts of Campbell's book will probably be (1) those that concern the inhibiting effects of religious and ethnic differences on class formation, which are usually thought of as an American but not a British phenomenon; (2) the analogy between MacDonaldism and the philosophy of the Knights of Labor; and (3) the comparative significance of the rural ties that many Lanarkshire miners retained when they moved into mining towns, especially when viewed in the context of U.S. homestead legislation and the

migration of many Scottish miners to the American Midwest after the Civil War. But even if they confine themselves to the trade union portions of this book, readers will find it a more sophisticated piece of analysis than any yet written on the American coalmining industry, with the possible exception of Katherine Harvey's work on the Maryland miners, who also numbered some Scottish immigrants among them.

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NORMAN MURRAY. *The Scottish Hand Loom Weavers, 1790-1850: A Social History*. Edinburgh: John Donald Publishers; distributed by P. D. Meany, Port Credit, Ontario. 1978. Pp. viii, 269. \$36.00.

Contemporary accounts and historical interpretations of the Industrial Revolution in Britain have traditionally portrayed the hand loom weavers as victims of inevitable economic forces, whose imprudent choices led to impoverishment, machine breaking, and alcoholism. In the present study, Norman Murray draws on records from local archives and weavers' societies as well as the usual parliamentary reports to specify a variety in the weavers' lives and labor obscured by general narratives. Whether the Industrial Revolution in Scotland is explained as the product of a breakthrough in one sector or development across a broad front of the economy, the textile industry must be considered crucial in the process.

Murray demonstrates that, during the early stages of transformation, hand loom weavers played a vital industrial role. They adopted new techniques and organizational forms that increased productivity. During the "Golden Age" of the hand loom trade, 1790 to 1812, hand loom weavers had a major impact on the local and national economies because their money and real wages increased as their numbers did. Falling profits and failure of the first hand loom weavers' strike in 1812 marked the beginning of a period of fluctuating trade and generally falling wages for the weavers. Although workers continued to enter the hand loom industry, after 1812 the weavers became less significant for their purchasing power than for their efforts to survive, attempts at unionism, and assimilation of radicalism.

Murray's research is thorough and his handling of evidence judicious. He avoids the optimistic bias of Duncan Bythell's *The Hand Loom Weavers: A Study of the English Cotton Industry during the Industrial Revolution* (1969). But Murray's book lacks a cogent interpretive or comparative framework and consequently does not define its place in the histo-

riography of the Industrial Revolution. He eschews any direct reference to the standard-of-living debate on the English working class during industrialization. In fact, he refers to the hand loom weavers in England as "elsewhere." Still, he discusses the questions important to that debate. Was there really a Golden Age for the hand loom weaver? Why did workers continue to enter the industry after weavers' wages began to fall? Was it possible for the spouse and children to add to the family income by weaving? Was the hand loom weaver's poverty a function of income level or imprudent budgeting? Although Murray's history is balanced, he leaves no doubt that the hand loom weavers' displacement and disappearance were accompanied by primary poverty and paradox.

The story of the hand loom weaver is pervaded by historical irony: the weavers' very adaptiveness contributed to their decline. The weavers' efforts to cope with falling wages by working longer hours or adding the labor of their family only contributed to overstocking and pushed piece rates down further. Their attempts to control entry to the hand loom trade was imitated by the power loom operatives; when the hand loom weavers tried to transfer to the power loom, the way was blocked. Their efforts to unionize were hindered not only by the courts and middle-class press but also by the dispersed and heterogeneous organization of the trade that had allowed them to survive early challenges. Murray's measured judgments and wide scholarship, indicated in the chapter notes and full bibliography, add immensely to our understanding of workers' adaptiveness to changes in their industry, life, and culture.

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DONALD HARMAN AKENSON. *Between Two Revolutions: Islandmagee, County Antrim, 1798-1920*. Hamden, Conn.: Archon Books. 1979. Pp. 221. \$17.50.

Donald Harman Akenson's book is written in a spirit of self-criticism. He believes that he and other professional Irish historians have disastrously neglected Irish local history in favor of an over-politicized "national" perspective. He has picked a somewhat isolated, largely Presbyterian Ulster community as a corrective. The author succeeds completely in his main objective. This book is a triumphant riposte to the millenarian version of nineteenth-century Irish history.

Much of the book is taken up with the problem that has plagued professional historians for years: the evolution of the Ulster Presbyterian community from its significant commitment to the ideals of the revolutionary United Irishmen in 1798 to the anti-Irish nationalist, pro-British Empire political stance

of 1886. Akenson gives us a fascinating study of one William McClelland who, at twenty-two years old, led the rising in 1798 and then, after a period abroad, returned to become a member of the Islandmagee Yeomanry and generally a substantial, respectable figure in the community until his death in 1859.

Akenson acknowledges, however, that such a widespread shift of allegiance by a large group requires more than a personal biographic explanation. Most explanations of the behavior of Ulster Protestants in general introduce various factors: the general phenomenon of the uneven development of Irish capitalism, the growth of Belfast, and the rise of Orangeism and evangelical religion. Akenson accepts much of this but argues that his small community's evolution must be explained in a different way. Akenson claims that these influences played little role in Islandmagee. His thesis is that in 1798, as in 1886-1921, the Islanders were above all defending the integrity of the Ulster Scots regional culture against incursions by external agencies.

There is much that is attractive in Akenson's argument. His authoritative account of the local economy is a further bonus. Yet, for all that, it is not fully convincing. There is a danger too that much of the exercise is simply knocking down straw men—for example, the discussion of landlord absenteeism. This leads to a neglect of the real questions: it is not surprising that Presbyterian tenant farmers did not join the Fenian-inspired Land League, but what was their relationship to the revival of a specifically Ulster (more openly moderate and constitutional) tenant-right radical tradition provoked by the Land League movement in 1879-82? Akenson tells us nothing on this score. Yet this was a critical period in the formation of two opposing national allegiances in Ireland.

Nor is the author's dismissal of broader explanations entirely satisfactory. Belfast's prosperity undoubtedly improved the fortunes of its immediate agricultural hinterland. Akenson's account of the local Orange Order is suspiciously apolitical and anodyne. In short, in their new-found enthusiasm for Irish local history, Irish scholars are in danger of producing a *depoliticized social history* and thus leaving many of the really important questions unanswered.

PAUL BEW
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SAMUEL CLARK. *Social Origins of the Irish Land War*. Princeton: Princeton University Press. 1979. Pp. x, 418. Cloth \$25.00, paper \$8.95.

In examining the Irish Land War of the late 1870s and early 1880s from a sociological perspective, em-

phasizing particularly mobilization theory, Samuel Clark disagrees with James S. Donnelly, Jr., *The Land and the People of Nineteenth Century Cork* (1975), and Joseph Lee, *The Modernization of Irish Society, 1848-1914* (1973), who argue that rising expectations inspired the Land League.

Contrasting pre- and post-Famine Irish agitations against British colonialism, Clark identifies the urban middle class and the Catholic clergy as the nucleus of early national movements such as Catholic Emancipation and Repeal. With the exception of the antitithe effort, agrarian protests were local and fragmented. Following the Famine, national schools, railroads, newspapers, towns, and a livestock agrarian economy resulted in national economic and cultural integration. This new situation, coupled with the Famine-related decline in the number of agricultural laborers, coalesced the interests of large and small tenant farmers, creating a new challenging collectivity. The agricultural depression of the late 1870s mobilized this collectivity and, for the first time, deeply involved the economically underdeveloped west of Ireland in a national movement.

Because of their kinship and credit ties with farmers, shopkeepers, some of whom had Fenian associations, played an important leadership role in the National Land League and became more significant than priests. The influence of the town and large farmer elements in the Land League kept it from antiurbanism, which has been so common in agrarian radicalism outside of Ireland, and cultivated an alliance with Parnellite nationalism. The challenging collectivity did not destroy landlordism but did disable it, preparing the way for peasant proprietorship. In the wake of the 1881 Land Act, Parnell enlisted the challenging collectivity as the core of Home Rule nationalism. It has continued as an influential element in Irish politics.

Sociological models illuminate aspects of the Irish historical process, but they also narrow the vision. The challenging collectivity thesis supplements rather than invalidates rising expectations as a source of the Land War. Paul Bew's, *Land and the National Question in Ireland, 1858-82* (1979) reveals tensions and conflicts between large and small farmers that Clark overlooks. Thomas N. Brown's *Irish-American Nationalism* (1966) suggests that New Departure nationalism might have been more important than the agricultural depression in mobilizing the new challenging collectivity.

Despite some of the limitations that his methodology imposes, Clark's book is a major contribution to the literature of the Irish Land Question. Solidly researched, exceptionally well written and organized, it punctures many myths and intelligently and perceptively presents and analyzes a vast

amount of new information on a rapidly changing rural Ireland.

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N. M. SUTHERLAND. *The Huguenot Struggle for Recognition*. New Haven: Yale University Press. 1980. Pp. x, 394. \$25.00.

N. M. Sutherland has numerous publications on aspects of French history during the Reformation to her credit. Her stated purpose in writing the present book is to examine "the relations between the Protestants, the Catholics, and the crown in sixteenth-century France" (p. ix). She has chosen to do this largely by focusing on the royal edicts relating to the Protestants from 1525 to 1598, using archival and printed sources. The result is an account of royal religious policy in the context of political circumstances at home and abroad, leading to the uncontroversial conclusion that all the French kings of the period were less concerned with doctrinal questions than with the problem of maintaining the authority and *bonne police* of the crown in a religiously divided kingdom.

Although the basic story is a familiar one, it has not often been told in English. Sutherland's version brings the old classics up to date, adds some new insights, and incorporates much recent work done in France. Since any narrative of conflicting interests, personalities, and diplomatic negotiations is necessarily complex, it could be wished that Sutherland's prose were less dense and that she had been more mindful of nonspecialist readers.

Among the particularly useful parts of the book are the chapter elucidating the sometimes misunderstood religious policy of François I, the chapter detailing the development of the conspiracy of Amboise, the sections dealing with the French Protestant-Netherlands connection, and the analysis of Henri IV's difficulties with his former coreligionists. It is also noteworthy that Catherine de Medici appears throughout the book in a sympathetic light as a *politique* working for peace where possible: a reassessment of Catherine that we could have expected from Sutherland, who has defused the myth of the "wicked Italian queen" in a bibliographical article in *Sixteenth Century Journal*, 9 (1978).

The edicts of religion issued by the French crown between 1525 and 1598 are listed, briefly summarized, and explained in the appendix. Considering that many of them are not easily accessible to students, it is regrettable that at least short selections from the texts themselves were not included. This may have been a publisher's decision for reasons of

economy. There are, however, copious footnotes and a full bibliography of sources and secondary works.

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LUCIANO GUERCI. *Libertà degli antichi e libertà dei moderni: Sparta, Atene e i "philosophes" nella Francia del Settecento.* (La Storia e le Idee. Esperienze, number 51.) Naples: Guida Editori. 1979. Pp. 279. L. 8,500.

DANIELE MENOZZI. *Lecture politiche di Gesù: Dall'Antico Regime alla Rivoluzione.* (Istituto per le Scienze Religiose di Bologna. Testi e Ricerche di Scienze Religiose, number 15.) Brescia: Paideia Editrice. 1979. Pp. 262. L. 8,000.

Both of these works by Italian scholars dealing with eighteenth-century French thought are clear, thoroughly researched, and well organized. Moreover, both confirm Croce's dictum that all history is contemporary. Either of these books is profitable by itself, but the two are most illuminating when read together.

Acknowledging his debt to the scholarship of Venturi and Momigliano, Luciano Guerri takes as his point of departure a distinction that Benjamin Constant made in the early nineteenth century between ancient liberty (typical of small states, devoted to warfare, based upon total equality, essentially collective, antithetical to commerce, and often held to have been perfected by Lycurgus) and modern freedom (found in larger countries, generally pacific, based upon equal rights and unequal fortunes, dedicated to the private liberty of the individual, thoroughly commercial, and sometimes held to have been anticipated by the institutions of Solon). Guerri sets the true beginning of his study at mid-century, when two authors who did much to enliven the quarrel between the "ancients" and the "moderns" had an immense impact: Montesquieu, who suggested that the "virtue" of ancient republics had little or no relevance to the contemporary world, and Rousseau, whose writings helped to create the "myth of Sparta." Guerri deliberately says relatively little about these two and concentrates instead on the arguments between the champions and critics of Laconia down to the eve of the Revolution, when new developments gave a different urgency to the dispute. Lacedaemonophiles such as Mably, Helvétius, or Jaucourt were often vehement critics of monarchical society. Hating inequality, they appealed to the leveling tendencies in the legislation of Lycurgus (usually ignoring the condition of the Helots!); suspicious of luxury and economic expansion, they praised the proverbial austerity of the Lacedaemonians; detesting political

instability and mistrusting the fickleness of the common people as much as the haughtiness of the optimates, they valued the "balanced" regime that Lycurgus had established.

The critics of Sparta were generally proponents of economic growth rather than of austere virtue smacking of monasticism, of individual rather than collective freedom, and of culture rather than military prowess. Some, such as the Physiocrats, Voltaire, and Condillac, appealed to the model of Attica and praised Solon as an enlightened pragmatist. Some modernist writings, notably Robinet's *Dictionnaire universel* (1777-83), denied the applicability of ancient republican principles to contemporary events and sought the best example of a commonwealth in North America rather than Greece. Still, the debate between the Spartan "ancients" and the Athenian "moderns" was lively in 1789.

Daniele Menozzi addresses, in a similar fashion, the way in which eighteenth-century Frenchmen interpreted Jesus to suit their ideas and how those ideas, in turn, were shaped by opinions about him. Beginning with Enlightenment circles, Menozzi investigates Diderot's and Boulanger's opinion that Jesus was a "seditious" protophilosophe, the argument of Voltaire that he was an exponent of toleration, the tendency of Rousseau to see Jesus as an earlier Jean-Jacques, and the communist interpretation (long found among millenarians) of Meslier. French Catholicism was divided between those who preached an obedient and beneficent Jesus and philosophic Christians like Turgot who saw in his teachings a confirmation of their own values. In the early days of the Revolution, Jesus was depicted as a "patriot," although later the refractory clergy often proclaimed a monarchical Christ, while radical works like Maréchal's *Almanach des républicains* (1793) portrayed the "sans-culotte of Nazareth." Needless to say, Napoleon's rise to power meant official encouragement for exhortations to see Jesus as the obedient one who had told his followers to give Caesar all things of this world.

Peter Gay rightly called attention to the importance of pagan antiquity for the Enlightenment. It is no less essential, however, to remember that an often sympathetic appreciation of Christian antiquity was a most significant component of eighteenth-century thought, not least among the philosophes. Both Guerri and Menozzi brilliantly show the interplay of interpretations of the past, ideas and conflicts of the present, and programs for action in the future. Together, the twentieth-century Italian authors shed much light on eighteenth-century France.

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HAROLD T. PARKER. *The Bureau of Commerce in 1781 and Its Policies With Respect to French Industry*. (The Bureau of Manufactures During the French Revolution and Under Napoleon.) Durham, N.C.: Carolina Academic Press. 1979. Pp. xi, 206. \$14.95.

This is the first volume of a planned multivolume study of the Bureau of Manufactures during the French Revolution and under Napoleon I. This initial study is devoted to its predecessor under the *ancien régime*, the Bureau of Commerce. The title indicates 1781 as the focal point of the study, but the policies of Jean-Baptiste Colbert are discussed as well as the persistence of those policies in the eighteenth century.

Harold T. Parker begins with a description of the organization and the work of the bureau in 1781 and introduces the four intendants of commerce and their views and policies. Colbertist mercantilism had become a serious hindrance to industrial expansion by the mid-eighteenth century, and it came under attack by the proponents of free trade, notably Vincent de Gournay and the Physiocratic school. But the Bureau of Commerce, influenced by the pragmatic outlook of the director general of finances, Jacques Necker, adopted an "interim paradigm between mercantilism and laissez-faire" (p. 31). The remainder of the book is a detailed description of the working out of this "middle way." The bureau encouraged industrial expansion by preventing combinations of workers, by providing loans and subsidies to innovating enterprises, and even by granting monopolies that encouraged rather than thwarted expansion. The bureau sought to eliminate internal trade barriers, to enlist the aid of scientists, and to encourage new inventions and more efficient techniques. A lengthy chapter investigates the application of these policies to each of six major industries: the textile, metallurgical, glass and pottery, chemical, paper, and leather industries.

What is of particular value in this study is Parker's diligent exploration of about ninety cartons in the F¹² series ("Commerce and Industry") at the Archives Nationales. He finds that "the bureau tended to develop policies in response to particular industrial necessities and opportunities rather than from broad-gauged theoretical considerations" (p. 160). Thus we are led beyond the clichés so often used in discussions of economic policy (mercantilism, laissez-faire), and we can see how the bureau grappled with concrete situations. And yet, Parker is not unconcerned about the problem of interpretation. In the final chapter he observes that the trend in the century was inexorably toward individualism and freedom. "Economic and social reality was moving, but the bureau was moving with it in general policies, in industrial strategies, and in individual encouragements" (p. 189). In a broader per-

spective, Parker is interested in the problem of continuity and change in the institutions of the *ancien régime* and the Revolution. The history of a bureau is a suitable framework for illuminating that problem.

This work is certainly a welcome contribution to our knowledge of French administrative history in the eighteenth century. Scholars in the field will look forward to the appearance of the succeeding volumes.

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DAVID P. JORDAN. *The King's Trial: The French Revolution vs. Louis XVI*. Berkeley and Los Angeles: University of California Press. 1979. Pp. xx, 275. \$14.95.

David P. Jordan's agreeably written, attractively illustrated, thoroughly indexed, and meticulously printed volume tells the scholarly reader virtually nothing new about the overthrow, imprisonment, trial, and execution of Louis XVI. The disappointment is all the greater because the author has devoted considerable effort to mastering the enormous number of printed sources dealing with the critical period of the French Revolution between August 10, 1792, and January 21, 1793.

In his preface Jordan describes how, during a stay in Paris, he came across a copy of Albert Soboul's *Le Procès de Louis XVI* and how, fascinated by the subject, he determined to write a book about it. More specifically, he wanted to examine the members of the National Convention "who grappled with the enormous questions of sovereignty, regicide, and revolution." They were, he declares, "men with something important to say and they said it in the most compelling language I had ever read. History . . . had contrived to make an ideal subject for a book; and yet there were no books devoted to the history of the trial and death of the king" (pp. xii-xiii).

The author's intent is certainly legitimate, but his methods raise some objections. Jordan has deliberately attempted what he calls an "old-fashioned book," "a narrative history of the kind that has fallen out of favor with historians," one intended to "tell an important story and . . . to tell it with some concern for style" (p. xiv). Modeling his study on Garrett Mattingly's *The Armada*, Jordan has crafted a narrative that stresses drama, emphasizes personalities, and employs epigrammatic and chronological chapter headings (for example, "The King Must Die: The Manège, January 18-20, 1793"). Unlike Mattingly, however, Jordan has discovered nothing new in the archives. Rather, he is content to rework and elaborate upon the printed sources (newspa-

pers, pamphlets, correspondence, speeches, memoirs) in retelling the story of Louis XVI's final days. The author has disdained footnotes, claiming that "any student of the French Revolution would be familiar with the sources for the trial" and that notes "would only interrupt the narrative flow" (p. xiv). The popular reader may rejoice, but the critic will find it difficult to check the references, which are clustered by chapter at the end of the volume.

Certainly the narrative does flow swiftly and makes effective use of contemporary sources to convey a sense of atmosphere and to delineate personality. Jordan's portraits of the fallen king, his defender DeSèze, and members of the Convention are incisive. Occasionally, the scholarly reader will discover pieces of unfamiliar information, but these tend to be *petite histoire* such as the routine of the royal family during their captivity in the Temple. Wider questions, such as public opinion in the provinces about events in Paris, are neglected. Moreover, there are some curious lapses in the author's knowledge. When Jordan indicates that no important regicide is honored with the name of a Parisian street, he ignores the Rue Danton on the Left Bank. Although he attended a memorial mass at the Church of Saint-Germain-l'Auxerrois on January 21, 1973, he seems unaware of the Chapelle Expiatoire, built to commemorate the martyred Louis XVI and Marie Antoinette.

However literate *The King's Trial* may be, scholars interested in the quarrel between Girondins and Montagnards or in voting patterns in the Convention are better advised to refer directly to two of Jordan's sources—M. J. Sydenham's *The Girondins* and Alison Patrick's *The Men of the First French Republic*. For their part, popular readers will find Saul Padover's *The Life and Death of Louis XVI*, which Jordan appears not to know, just as exciting a guide through the cobblestone streets leading to the scaffold where the guillotine stands waiting.

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FRANÇOIS GENDRON. *La Jeunesse Dorée: Épisodes de la Révolution française*. Preface by ALBERT SOBOUL. Québec: Presses de l'Université du Québec. 1979. Pp. xiv, 446.

In this detailed narrative of reactionary politics in Paris after Robespierre's fall, François Gendron fleshes out and documents the long-standing views of "classic revolutionary historiography." His research was designed to correct or substantiate the first-person accounts that have long shaped our understanding of this important subject. Among other things, Gendron treats the eruption of anti-Jacobin streetfighting; the war to mobilize respectable opin-

ion against terrorists that was waged in newspapers, pamphlets, and theaters; and the confrontations between an increasingly desperate and hungry populace and a raucous and insolent "gilded youth." The culminating episodes in this agonized phase of the French Revolution are re-examined from the other side of the barricades. First came the popular insurrections of *germinal-prairial* Year III (previously chronicled from the sans-culotte viewpoint by Kare Tonnesson, among others), in which the *jeunesse dorée* served as an inept and vengeful auxiliary of the Convention. A few months later, in the rebellion of *vendémiaire* Year IV, these *muscadins* turned against the Convention, which was belatedly attempting to halt the reaction and stabilize the polity by perpetuating itself in office. Assessing to what extent the *jeunesse dorée* bullied the Convention as opposed to being manipulated by Thermidorian deputies for their own ends, Gendron's nuanced conclusion cannot be summarized in a line or two, except to say that the role of the *jeunesse* was indeed potent. Here is one catch label that was no myth.

Like Tonnesson, Gendron tells the story with a loving attention to detail, undaunted by the complexity of dealing with over twelve hundred individuals (to judge by the index) and with all forty-eight Paris sections (despite Gendron's demonstration that the weight of reaction lay in the Right Bank's western districts). Specialists will long mine this erudite book for illustrative material, and perhaps only they can appreciate the extraordinary labors that went into it. For in the absence of any ready-made files devoted to the *jeunesse dorée*, Gendron had recourse to the massive alphabetical files of the Convention's Committee of General Security (whose history, incidentally, has never been written). In this repository for the arrests and interrogations of both the Terror in the Year II and the repression of left and then right in the Year III, there are "36,000 dossiers, stored in 348 cartons, which we have gone through one by one"—not to mention police constables' reports and section papers elsewhere!

How distressing that such scholarly zeal is so narrowly focused. Surely the main justification for going over ground already worked by Tonnesson is to see the right more clearly. But, wrapping himself too tightly in the mantle of "classic revolutionary historiography," Gendron is prone to the faults that certain revisionists—with unfair exaggeration—attribute to that tradition. Above all there is an inappropriate partisanship. To be sure, positive sympathy is often a great asset, as in Richard Cobb's decidedly partisan, passionate, and evocative *Les Armées révolutionnaires*. But extreme hostility is another matter. Gendron repeatedly indulges the temptation to excoriate these unappealing and occasionally repellent *muscadins*. Obviously no one

would expect a historian today to celebrate the *jeunesse dorée* or even to forgive them. Surely one might hope to understand them.

This is largely a vain hope in Gendron's work, the more so because of his tenacious adherence to a narrative mode and his penchant to orchestrate sequences of inevitable events. He should instead have emulated Albert Soboul's example in part 2 of *Les sans-culottes parisiens*, in which a memorable analytic portrait of sans-culotte militants is drawn with emphasis on the relation of their social background to their political style and on their motivations and aspirations. Gendron instead gives us one-dimensional villains.

Perhaps one problem is that there is no firm social base on which to peg such analysis. As the book progresses, the social axis shifts from classic strata of the bourgeoisie (the professions, mercantile community, finance, arts and letters, public administration) to a vague amalgam of those strata with various types of *petits bourgeois* who seem scarcely distinguishable from a list of sans-culotte artisans and shopkeepers. In any case, quantifying occupational groupings in this period is extremely tricky, and Gendron has enough individual case studies to evoke a feel for this rightist youth. But he tells us precious little about their motivations and aspirations, where they came from or what happened to them subsequently, or what they wished for France or for themselves. He ought to have dealt more fully with the problem of generations (which he rejects as a tenable hypothesis at one point but implicitly puts forward elsewhere) and with draft evasion. Most fundamentally, we are not told to what extent these militants were the relatives of the Terror's victims and thus engaged in a settling of accounts that was not entirely odious. In short, the *muscadins* ought to be seen in their own right—just as Lefebvre saw the peasants and as Soboul and others have studied the sans-culottes.

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JEAN FAURY. *Cléricalisme et anticléricalisme dans le Tarn (1848-1900)*. (Publications de l'Université de Toulouse-Le Mirail, series A, number 41.) Toulouse: Service des Publications de l'Université de Toulouse-Le Mirail. 1980. Pp. 532. 170 fr.

Jean Faury's *thèse de doctorat* provides reinforcement for the view of Yves-Marie Hilaire and Eugen Weber that the anticlerical issue in nineteenth-century France was critical on the local level and much more than a political gambit. This examination of the Tarn, a particularly suitable department for study because it produced Jean Jaurès and Emile Combes while retaining a high level of religious

practice, begins with the Second Republic, which generated the Falloux Law, and ends with the denouement of the Dreyfus affair, which led to the abrogation of the 1801 Concordat.

Faury rejects as misleading the traditional view that clericalism stimulated an opposing anticlericalism. Instead, he describes the evolution of two competing conceptions of society after 1815. The Catholic Church recalled the excesses committed against it during the Revolution and attempted to prevent their recurrence by tapping every possible source of support. The church founded schools, promoted mass pilgrimages, exploited the popular press, multiplied forms of public piety and social work, and supported the election of political candidates congenial to it. Simultaneously, a social theory that called for the reduction of the influence of religion was developing with its roots in the Enlightenment and the results of the Revolution. The refusal of either side to comprehend the fears of the other or to compromise led to a radicalization of each. The battle lines formed between the unyielding clericalism of the parish priests and the militantly anticlerical freemasons.

The confrontation had political implications, as the church supported royalism while the anticlericals backed republicans and, ultimately, socialists. By the end of the century, however, there was a dissociation of religion and politics. Religious practice remained high, but the electoral influence of the church disappeared. At the time of the most vehement anticlericalism in politics on the national level, the clash in the Tarn had been determined, with neither side a clear victor.

Faury sustains his argument well and claims to base it on research in the departmental archives of the Tarn, the Archives Nationales, the papers of the archiepiscopal diocese of Albi, the records of freemasonry's Grand Orient, and the popular press. To verify this research is impossible because of the unpardonable decision of the publisher to eliminate a detailed list of archival and manuscript sources and almost every footnote.

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ALAIN CORBIN. *Les filles de nocé: Misère sexuelle et prostitution (19^e et 20^e siècles)*. (Collection Historique.) Paris: Aubier Montaigne. 1978. Pp. 571.

To most historians of nineteenth-century France, the word "prostitute" conjures up images from the century's imaginative literature—*Fantine*, *Carmen*, *Gervaise*, *Nana*, *la dame aux camélias*—and perhaps some impressions from the chroniclers of society and *mœurs* such as Restif de la Bretonne, the Goncourt brothers, Maxime du Camp, and de Mau-

passant. Although recognized as an important social fact or evil, nineteenth-century French prostitution has lacked systematic and scholarly study. Alain Corbin's contribution, the work of an *historien de métier*, rests on extensive primary and secondary publications as well as unpublished public health records and police reports from Paris and other parts of France. The volume focuses primarily on the first four decades of the Third Republic but spans the broader period from the Restoration to the Gaullist epoch.

During the nineteenth century, prostitution seems to have been accepted by most individuals—at least most men—as necessary and useful but sometimes socially inconvenient. Thus the Restoration and the July Monarchy sought to enclose the prostitute in a series of concealed places—*maison de tolérance*, hospital, prison—making her available to her clientele and the regulatory arm of the state but not easily visible to polite society. From the 1880s, private groups and some republicans began to argue for the abolition of the system of regulation, while other public figures expressed growing concern over an alleged spread of venereal disease and argued for more control rather than less.

Some women registered with the authorities, which meant periodic sanitary examinations and less police harassment, but many sought to retain a primary identity as domestics and shop clerks, claiming that they resorted to soliciting only to meet unusual expenses. Corbin argues that the needs of men who patronized prostitutes changed during the century from a "seminal drain" to an experience in "seduction." The generalization may hold for some of the wealthier clients he describes, but it is hard to understand how a hasty coupling in the back room of a tavern could sustain even the fiction of "seduction" or "romance."

The Marxists were wrong in accusing the bourgeoisie of deflowering the daughters of the working class. Young women who worked as prostitutes had almost always lost their virginity to partners of the same social rank. On the other hand, the clients of prostitutes were often of a higher social and economic station than the women who served them, and middle-class values, such as postponement of marriage, certainly contributed to the need for prostitution.

Feminists and others may well be unhappy about Corbin's failure to criticize the exploitation of one sex by another. Accepting moral neutrality as the historian's proper posture, one would still have liked greater curiosity about the psychological aspects of prostitution. For example, Corbin mentions that married men may have chosen sexual intercourse with prostitutes as relief from the practice of coitus interruptus in the marital bed. The idea is intriguing, but Corbin declines to pursue it. Another

subject left unexplored is that of female homosexuality among prostitutes. Corbin alleges that nineteenth-century men felt great anxiety at the possibility of *tribadisme*—a conclusion that sounds plausible but is neither developed nor well documented.

Occasionally Corbin's *esprit de classement* becomes difficult to support, as in a chapter describing the categories of *demi-mondaine*, *femmes galantes*, *soupeuses de restaurant de nuit*, *femmes d'attente*, and *filles entretenues*. Since all these women were engaged in providing sexual services for money, the attention given to establishing their categories seems misplaced. Corbin's volume would have profited from reduced length, good scholarship being not inconsistent with economy of thought and language. These criticisms noted, however, the work deserves attention as a thorough exploration of a subject previously left to fiction, polemic, and *la petite histoire*.

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PAUL MAZGAJ. *The Action Française and Revolutionary Syndicalism*. Chapel Hill: University of North Carolina Press. 1979. Pp. ix, 281. \$19.00.

This book deals with the interaction between the syndicalist left and the royalist right between the Dreyfus affair and World War I. It challenges pro-syndicalist historiography that contrasts the professional Socialist politician, corrupted by careerism and proximity to the bourgeoisie, to the incorruptible worker-militant, divorced from the parliamentary arena. In detailing the "seamier side of heroic syndicalism," Paul Mazgaj exposes the "neo-Machiavellianism" of certain syndicalist leaders who, disdaining "petit bourgeois moral considerations," anticipated the revolutionary left's denunciation of Social Democrats as "social fascists" during the 1930s. Syndicalist leaders like Emile Janvion, Francis Delaisi, and—for a time—Gustave Hervé were willing to ally with the royalist *Action Française* (AF) in an attempt to drive a wedge between the Dreyfusard alliance of liberals, socialists, and moderate syndicalists.

Between 1906 and 1912 French syndicalism suffered a series of defeats at the hands of the Clemenceau ministry that left the movement in disarray. Some "desperate revolutionaries" came to feel that the royalists could be of service and that the republic was a worse enemy of the worker than the monarchy. This coincided with a crisis within the AF where Maurras needed to placate the impatient youth of the *Camelots du roi* who took his talk of a coup d'état seriously. In 1911 they were excited by the prospect of a revolutionary left-right coalition based on antisemitism that would, they hoped, destroy their common enemy, the republic.

Contacts were established between the AF and certain syndicalist ultras, some of whom accepted royalist money. For a time, Hervé's *Guerre sociale* and Janvion's *Terre libre* newspapers echoed royalist condemnations of the Dreyfusard alliance and launched antisemitic campaigns accusing Rothschild of betraying the workers and *L'Humanité* of doing the same because of Jewish financing.

Unfortunately, Mazgaj's account is very slippery, repeatedly insinuating far more than it proves. Basically, we learn a good deal about a handful of syndicalist renegades but little about the syndicalist response as a whole or about the response of workers themselves. We are told that Maurras "counted on" help from the revolutionary left (p. 93) and that, by the summer of 1910, Janvion "had some cause to feel optimistic" (p. 128)—as if substantial syndicalist collaboration (never forthcoming) were really expected. It may be that many workers were indeed antisemitic, or potentially so, but no numerical evidence is ever given. Mazgaj asserts that young royalists were "serious" (p. 54) and "sincere" (p. 170) in their commitment to a proworker social policy without proving it; rank opportunism seems a much likelier explanation. The AF may have opposed the "stodgy conservatism" of the parliamentary right but only its political not its economic conservatism (Mazgaj suggests it was both). An inattentive reader might be misled by the way Mazgaj slides back and forth between monarchist hopes for an opening to the left (pp. 141–42) and what actually happened. Misleading, too, is the citing of one hyperbolic soul in 1911 who contended that "the fashion is no longer with the principles of 1789 but with the acts of violence of the C.G.T. and the Camelots du roi" (p. 150). It was certainly not the "fashion" with the overwhelming majority of the French electorate. Nor does Mazgaj's convoluted language—Socialists were "not immune from the contagion of suspicion" (p. 162) of being antisemitic—prove that they *were* antisemitic.

Mazgaj seldom dwells on his own evidence when it contradicts his image of syndicalism as a potentially corrupt mass movement. We learn in passing, for example, that a major antisemitic "workers rally" of the era had to be held in the Latin Quarter to ensure a decent crowd (of bourgeois students and royalists?), that in 1911 the Socialist party congress declared it would "never compromise itself with the antisemitic slime," that syndicalists who considered entering into relations with the AF "were not disposed to be candid even with other militants," and that the AF's Léon Daudet confessed that attempts at proselytizing workers met with only "modest" success (pp. 157–58, 161, 218, 144). When, in 1909, the AF's Maurice Pujo claimed the left's revolutionaries were "on our side," syndicalist Victor Méric replied contemptuously "Tu parles" and pointed

out that his followers opposed the republic "on different grounds and with opposite aims" (p. 101).

For Mazgaj, nevertheless, the possibility of royalist nationalism and revolutionary syndicalism combining on the basis of popular resentment against the Jew was a very "real" one before 1912 (when the debate over the three-year military draft law split them irretrievably). Had this "enormous potential" been realized, the "conventional wisdom" that royalists and syndicalists could never unite on a positive program would have been disproven. But Mazgaj, despite his readiness to disprove, his meticulous research into police and press reports, and his ingenious writing style, leaves that conventional wisdom largely unscathed.

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WILLIAM D. IRVINE. *French Conservatism in Crisis: The Republican Federation of France in the 1930s*. Baton Rouge: Louisiana State University Press. 1979. Pp. xx, 256. \$17.50.

Descended from the right-wing of the *progressistes*, the Republican Federation remained "the largest and most important French conservative party" in the 1920s–30s under the leadership of Lorrainer Louis Marin, whose papers constitute a major source for this book. As William D. Irvine describes it, the party consisted of *modérés* and *notables*, with few *militants*, and its deputies were less *ministre* with each passing year. That structure worked until the crises of the 1930s. As *modérés*, Republican Federation deputies were Catholic, socially and economically conservative, ardently nationalistic, and committed to the principles of the Third Republic if that meant the republic of Méline. As *notables* they came from both industrial wealth (for example, François de Wendel) as well as the landed and titled variety (for example, eleven counts, five marquises, four barons, two dukes, and one prince). As the book's excellent maps demonstrate, they were strongest in the classic regions of the French right: the northeast, the west, the well-to-do *arrondissements* of Paris, and the southern edge of the Massif Central. *Militants* were rarely needed to win elections in such areas where traditional voters looked to their *notables* for leadership. In turn, Federation *notables* disdained mass politics and seldom needed or heeded the *militants* in the party. Irvine is to be applauded for rescuing such dull *modérés* from oblivion, since *enragés* such as Barrès, Doriot, Drieu, Maurras, and the like are clearly much more fun.

What makes these *modérés* interesting and what makes Irvine's book especially useful are the Republican Federation's changing beliefs and action in the face of the antiparliamentary leagues of "the

so-called 'new' Right," the Popular Front, and the advance of Hitler's Germany. To those events, "the Federation's response was that of a threatened and frightened elite, searching desperately for some way to restore social and political stability." "As a result of the Popular Front, the Republican Federation evolved in a direction that would have horrified the founders of the party." This new direction turned the Federation from its alliance with the democratic center to a flirtation with the leagues of the "new' Right." It also led the Federation from its traditional hostility to Germany to appeasement of Hitler's Reich.

Having no allies or militants of its own and perceiving imminent revolution in the Popular Front, the "old" right Republican Federation turned to the militants of the "new" right, notably the *Jeunesses Patriotes* and the *Croix de Feu*. Irvine makes clear that the Federation's leaders ran the 80,000-member *Jeunesses Patriotes*, that ties with the *Croix de Feu* were close, "that violent street demonstrations were not at all foreign to the mentality of some in the Federation," and that the Federation's program and beliefs were almost identical with those of the leagues. The only requirement for cooperation was the "docile acceptance" by the leagues "of the Federation's political leadership" and the division of labor between the streets and the parliament. Only when the leagues abandoned their "docile acceptance" and allotted place in the division of labor by converting themselves from paramilitary leagues into political parties did the *notables* of the Federation become alarmed, especially when it and de la Rocque's *Parti Social Français* competed for the same voters.

In foreign policy, the Federation's new direction led it to appeasement, of course. Irvine follows Charles Michaud closely in pointing out that most Federation members came to appeasement by following Louis Marin's "conditional nationalism" while a minority adhered to a "resigned nationalism." Irvine adds two more ingredients to this familiar story. One is that Marin's anti-*mûnichois* reputation is ill founded. More important is the relationship of appeasement to domestic politics. "The Republican Federation abandoned its unbending nationalism from fear of the social and political consequences of the Popular Front. Although fear of war or fear of losing played a part in the determination of the party's attitude . . . the major determinant was the fear of revolution" [emphasis added].

If Irvine's solidly researched and conceptualized book has a fault, it is in erring on the side of caution. The author does not push as far as his evidence seems to have allowed the link between "old" right conservatives and "new" right fascists. Even in his persistent use of "new' right" instead of "fascism," Irvine blurs the message of his excellent

book. Perhaps he did not want to become embroiled in the continuing debate over defining "fascism," but most readers will find that he writes about the coming together of conservatives and fascists just the same. The book will give little comfort to those who insist either on the "foreign" or the left-wing origins of French fascism. Rather, it demonstrates the ease with which French conservatives, fearful of the Popular Front, abandoned democratic practice to associate themselves with what one customarily calls "fascism."

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JEFFERY A. GUNSBURG. *Divided and Conquered: The French High Command and the Defeat of the West, 1940* (Contributions in Military History, number 18.) Westport, Conn.: Greenwood Press. 1979. Pp. xxiii, 303. \$25.00.

J.-B. Duroselle chose Montesquieu to close his recent book *La Décadence*: regimes typically collapse from a multitude of general causes; accordingly, when one is removed by a single battle, it is because underlying causes have left that regime vulnerable to a single blow. Jeffery A. Gunsburg is wary of Montesquieu and of Duroselle's thesis of decadence in prewar France. The collapse of France is to be explained in large measure by the battle of May 1940. To the extent that general causes are to be invoked at all, it should be in the recognition that this was an Allied defeat for which some peculiar French malady offers inadequate explanation.

According to Gunsburg, France was not badly prepared for war. Its defense experts understood blitzkrieg tactics, the perilous course of Belgian policy, and the potential hazards of the Ardennes sector. French war materiel was greater in numbers and quality than traditional views have allowed and French doctrine on the use of armored forces and aircraft was not that far removed from the German. Finally, the Dyle Plan, with its Breda variant, boldly addressed the conditions of an allied war in which Dutch, Belgian, British, and French troops would fight on a common front that had to accord with their respective national interests. The risks entailed by General Gamelin's plan were not prompted by senility but by a desire to forge an effective military coalition.

This is not the first time such views have been advanced, but those sympathetic to Gunsburg's central thesis will welcome his contribution to the debate on 1940. Noteworthy is his extensive documentation from the French army and air archives, which have long been closed to researchers. Indeed, the special conditions of his access to these

documents may explain his failure to provide series or carton references—a drawback for other scholars.

Despite the broader historiographical context into which it deserves to be placed, this book is primarily an exercise in military history; over a third of it addresses the days from 10 to 21 May. As such, it will be a treat for campaign specialists. Others, however, might prefer more guidance and reflective comment. More effort might have been made to show where and to what extent the new archival material substantially alters previous interpretations of the French war effort; this dimension is incompletely filled by the very brief bibliographical essay. Similarly, one might welcome instruction on how the enigmatic but central figure of Gamelin, that so often indecisive man, ever steered himself to the risk-plagued option of the Breda maneuver. Finally, the reader may be perplexed by a conclusion that places ultimate responsibility for the collapse not on the generals, with whom the book is concerned, but on the civilian leaders, with whom it is not. Montesquieu, it seems, has a lingering presence.

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HELEN NADER. *The Mendoza Family in the Spanish Renaissance, 1350 to 1550*. New Brunswick, N.J.: Rutgers University Press. 1979. Pp. xiv, 275. \$19.50.

Family history and prosopography, central to understanding medieval Europe, have been sadly neglected in Spain. Helen Nader has rummaged through the impressive documentary remains of Castile's celebrated Mendoza clan and presented both a panoramic family history and a broadly ranging reassessment of late medieval Spain: the role of the baronial class as repository for creative literature and art, the tainted pseudomodernism of their *letrado* supplanters, the meaning of the Renaissance for Spanish culture and society, and the roots of that religiosity that gave Renaissance Spain the Inquisition and the Morisco tragedy. Ultimately *The Mendoza Family* studies the choice of self-images by succeeding generations of the medieval establishment. Nader brings fresh, erudite, ringingly expressed perceptions. She re-sites the medieval problematic, rejecting received interpretations. Her work is revisionist and is bound to be, in whole and in part, controversial.

The standard view sees the corrupt feudal *caballeros* replaced in state administration in the 1480s by a university-trained meritocracy who then guided Spain into Renaissance classical humanism and the modern state. Nader opposes to this a much earlier Renaissance, more humane and truly Spanish, illustrated by the major Mendoza figures. The

post-Black Death century saw the triumph of an upwardly mobile new aristocracy, in mutually beneficial partnership with the illegitimate Trastamar dynasty—pragmatic, tolerant, cultivated, relatively peaceful. Their households were the educational and scholarly focuses, and their historical writings (Latin influenced but always vernacular) expressed their naturalistic-humanistic views of man and state. Their values, techniques, and behavior identify them with their contemporaries of Renaissance Florence; their wider world tied directly into the cultivation at papal Avignon. Supporting the throne, amassing estates and honors, and extending the clan, the Mendozas helped lead the “true” Renaissance. Ferdinand and Isabella ushered in an antipathetic stratum: the “lettered” university-trained bureaucrat, who was fully Latinate, imitative, and alien in inspiration; inflexibly theological, Scholastic, and Roman Law in fundamental outlook; medieval-providentialist despite the armor of Cicero; and reformist, authoritarian, and intolerant. The “Caballero Renaissance,” in stasis after 1450, conspired in its own defeat. The Mendozas became dispersed, eclectic, devotional, and architectural rather than literary; their leaders were too traditional to operate effectively in the new power balance and program.

An abstract summary does not do justice to this interlocking set of theses: the focus remains on Mendoza eminences such as Diego Hurtado, Pedro López de Ayala, Fernán Pérez de Guzmán, Santillana, and “The Great Cardinal.” An unexpectedly rich treatment features conquered Granada's Captain-General Tendilla, last paladin of the Mendozas' consistent intellectual and career patterns. Because this work is done on so broad a canvas, sometimes in primary colors, one is tempted to quarrel with details or emphases or officiously to intrude supplementary bibliography like José Szmolka Clare's articles around the Tendilla correspondence in *Andalucía medieval* (1978). This would be a pity. In the tradition of the burgeoning American school of medieval Spanish history, Nader's is a distinguished contribution. Solidly based on family archives and the plethora of pertinent publications, *The Mendoza Family* fascinates as pioneering dynastic chronicle and challenges as historiography.

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FRANCISCO CHACÓN JIMÉNEZ. *Murcia en la centuria del quinientos*. Murcia: Universidad de Murcia. 1979. Pp. 534.

Francisco Chacón Jiménez has deliberately fashioned his study of sixteenth-century Murcia to con-

form to the *Annales* style of regional history. His model is Bartolomé Bennassar's *Valladolid au Siècle d'Or*. He therefore incorporates into his book a massive amount of information, much of which is quantitative. He ranges over various topics from physical and human geography, demography, and wage and price movements to local feasts and festivals. These he allots to one of three almost predictable sections: "Man and his Environment," "Economic Life," and "Social Relations and City Government." His intent is to sew these fragments together in an effort to create a "total history."

His book is also designed to afford a new and different perspective on the socioeconomic history of sixteenth-century Spain. The view from Murcia—a medium-size city located in the extreme southeast of Spain with a population of some 15,000 in 1591—is a view from a city that was fundamentally agrarian. Half of its working population was directly involved in agricultural production. Moreover, the agriculture of Murcia was highly specialized. Most of the land was devoted to the cultivation of the mulberry, and the principal commodity produced and exported was raw silk. As a consequence, it was also a dependent economy. Murcia had very little secondary industry; hence its purpose was to feed into the more developed, urban, manufacturing economies of Granada and Toledo. In addition, since it was virtually a monoculture, it relied for subsistence on grain imports from La Mancha, Andalusia, and Valencia, or even from abroad. These dependencies appear to have made life relatively expensive but not unduly precarious. The author finds that the city and its region escaped the severe economic and demographic crisis that afflicted most of Castile in the 1590s.

This finding underlines once again the marked regional variations in economic performance that typified early modern European societies. This important conclusion complements a host of other interesting observations. For these and for its descriptive detail, this book makes a useful contribution to sixteenth-century Spanish studies; it has several major defects, however. By its failure to meet Bennassar's exacting standards, it proves that "total history" must be more than the sum of its parts, more than an aggregation of fact. By its failure to address the most significant historical questions relating to Murcia's economic development, it emphasizes how important it is for historians to select their chronology wisely. Murcia and its region underwent a significant structural change sometime between the late fourteenth century and the mid-sixteenth century when it was transformed from a mixed agricultural economy to a highly specialized one. It became a kind of "colonial" economy that, by the late sixteenth century, relied heavily on a kind of colonial labor force. *Morisco* laborers, many of them re-

cent immigrants after the Revolt of the Alpujarras, were crucial to silk production, but these were the very *moriscos* expelled from Spain in 1609. Because Chacón Jiménez concentrates almost exclusively on the second half of the sixteenth century, he fails either to elucidate the mechanics of this structural change or to determine the consequences of the expulsion. It is as if he has conceived of an interesting plot and assembled a strong cast of characters but failed to write the dramatic narrative his material requires.

CHARLES J. JAGO
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V. VÁZQUEZ DE PRADA, editor. *Historia Económica y Social de España*. Volume 3, *Los Siglos XVI y XVII*. Madrid: Confederación Española de Cajas de Ahorros. 1978. Pp. 756.

It has been over twenty years since Jaime Vicens Vives and his students attempted a comprehensive review of the research on Spanish economic and social history in the Habsburg era. Stimulated in part by their enthusiasm, scholars have since told us a great deal more, and we have needed for some time a new analysis of this literature like the one that V. Vázquez de Prada has now published.

The author is a distinguished economic historian who has done important work on Spanish commercial relations with the Low Countries and on Spanish industry. He is completely familiar with the contemporary investigation of all aspects of the economy and society of Habsburg Spain, and he does an excellent job of summarizing clearly the results. Both students who want a guide to this complex body of research and accomplished scholars seeking to identify important topics for further study can turn to this book with profit. As the author notes, Castile dominates his presentation because that kingdom has received the most study, but Vázquez is successful in highlighting the important variations among the major regions of Spain. The carefully selected illustrations not only reveal a great deal about the physical resources, technology, social conditions, and culture but also provide a good orientation to the types of documents available.

Although the individual chapters are generally good, the overall organization of the book creates genuine barriers for understanding Habsburg Spain. Because Vázquez believes government action decisively shaped economic and social developments, he begins with a section on the political-administrative framework and on the state treasury. This order is questionable even if one accepts his thesis since, without a concrete examination of the physical resources, technology, and transportation

system available to Spaniards in the sixteenth and seventeenth centuries, it is impossible to evaluate claims made for the impact of taxation or other government policies. Yet the reader is not given this essential information until the second half of the book. The third and final part on the economy begins with chapters on economic policy and thought even though the government never had a genuine economic policy and the theoretical economists had no significant influence. Discussion of population levels also precedes, and is isolated from, information about the resources to support them (in section 2 on population and society).

Although a short review is no place to argue the point, the book in fact contains good evidence that, except for seventeenth-century currency manipulation that is the subject of an excellent chapter near the end of the text, government financial actions were not usually a decisive factor in Spanish economic and social developments but merely responses to changes motivated by other forces. Vázquez's organization as well as a somewhat anachronistic treatment of Spain within its modern geographical framework without sufficient reference to its intimate connections to America, Italy, and the Low Countries makes it impossible for the author to argue effectively for his general thesis. Unfortunately, this lack of forceful argument weakens the book's ability to motivate the exciting new research that early modern Spanish history merits.

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MIGUEL ARTOLA *et al.* *Los ferrocarriles en España, 1844-1943*. Volume 1, *El estado y los ferrocarriles*. (Servicio de Estudios del Banco de España.) Madrid: Banco de España. 1979. Pp. 458.

RAFAEL ANES and PEDRO TEDDE. *Los ferrocarriles en España, 1844-1943*. Volume 2, *Los ferrocarriles y la economía*. (Servicio de Estudios del Banco de España.) Madrid: Banco de España. 1978. Pp. 562. 500 ptas.

Railroad history is fast becoming one of the most active fields in Spanish economic historiography. The debate is lively and the research is unearthing much new material. In December 1979 the Spanish State Railway Company (RENFE) sponsored an international conference on European railroad history where many interesting papers were read (among them some by contributors to the volumes under review) and some general conclusions that apply across political borders emerged.

Los ferrocarriles en España is one by-product of this recent surge in institutional interest in Spanish railroad history. The work of a team of six distinguished researchers under the direction of Miguel

Artola, it has been sponsored and published by the Bank of Spain. As everywhere else, railroad history in Spain is a field full of controversy, and the authors of this work have not tried to avoid debate. Rather, as the conclusions make clear, the purpose of the book has been to test "a certain number of propositions that could be considered as generally admitted ideas" (p. 515). In fact, while doing so, the authors have written the most up-to-date history of the Spanish railroads. As to the testing itself, the results are not clear, and the debate will no doubt continue.

The first volume comprises three main contributions. The first, by Diego Mateo, is a study of early state policies relating to the railroads from the early blueprints in the 1830s to 1877, when the second general railway law was issued. This is a fine piece of political and institutional history, narrative rather than demonstrative. The second contribution, by R. Cordero and F. Menéndez is rather difficult to characterize: it tries to be a "technical" work, dealing with such topics as "the rationality of the network," "the locomotive as a vaporizing artifact," or "the locomotive as a vehicle." In fact, the most interesting and controversial section is the one dealing with the rationality of the network layout. The layout, unmistakably radial with Madrid at its center, has been criticized, mostly by Catalan writers, as inefficient and politically motivated. Cordero and Menéndez set out to test this statement, something that is impossible without further specification since, as the authors admit at the outset, the consumer's optimum is not the same as the operator's and neither of them coincides with the builder's. One might add that the state's optimum may differ from all the others. Another hopeless task they undertake is that of convincing the reader that the decision to build the network with a wider gauge than the European standard, and to stick to it to this day, was a wise decision. They overestimate the costs of converting, and the cost of changing trains at the French border for passengers and merchandise is hardly mentioned, let alone measured. The work, however, is full of interesting historical and technical information.

The third contribution, by Miguel Artola, deals with "the action of the state." It synthesizes the relationship between the state and the railroad companies during the time period covered by the book. As a summary, this essay has merit, but it fails in that most of its key statements remain unmonstrated. For instance, the first chapter, which deals with the contribution of the state to the building of the lines, is based upon the premise that "the mobilization of savings to be invested in the building of railroads is only possible through the mediation of the state" (p. 345). This is a strong statement that seems to be offered as an axiom; it may very

possibly be true, but the problem of railroad finance has many very controversial aspects that should have been discussed. In this connection, one also misses a little effort at comparative history: there is hardly any mention in this essay of the state policies of other European countries, whose results might help us understand the Spanish case and whose methods may have been imitated or rejected by the Spanish authorities. Elsewhere Artola tries to estimate the returns obtained by the state from the railroads; but this very interesting exercise is marred because the author, in obtaining a total figure for accumulated tax receipts, fails to deflate and lumps together pesetas of 1880 with pesetas of 1935. The wide price fluctuations that intervened, especially in a country whose monetary standard was entirely fiduciary, make Artola's final figure almost meaningless.

There is a third serious problem with Artola's essay, that of repetition. The way this book was conceived, it was difficult to avoid some overlapping, but the problem is most evident in this piece, which reiterates topics covered by parts of all the other essays. All this, however, should not make us forget the merits of this contribution: a thorough scholarship in the use of sources (but not of bibliography; Artola does not cite any non-Spanish books, only one book published after 1936, and less than half a dozen in all); a courageous attempt at quantification and synthesis on many topics; and some useful and original analyses, such as the one on rate policies and the already mentioned one on the state's gains from the railroads.

The second volume is composed of two works (plus the general conclusions), one by Pedro Tedde on the history of the two main railroad companies and the other by Rafael Anes on the relationship between the railroads and the economy. Both cover the period from roughly 1855 to 1936.

Tedde's work, in addition to narrating the economic history of the two largest companies (Norte and MZA), sets out to explain the paradox of the extremely low profitability of Spanish railroads. The paradox lies in the fact that the physical characteristics of the Iberian peninsula made transportation a serious economic bottleneck that only the railroad could break. Unlike the United States or England, Spain had no viable waterway network that could compete with the railroads. How can one then explain the chronic liquidity problems of the companies and the extremely low yield of their shares? Tedde's explanation is a circumstantial one: the problems of the companies were the result of a succession of economic and political events largely beyond their control: the agricultural depression of the 1880s and 1890s, the depreciation of the peseta during the 1890s, and cost inflation after 1910. Therefore, the network that, aside from a few short

lines, was started in 1855 with state support became increasingly dependent upon government subsidies and was finally nationalized in 1942 when it became clear that the companies were unable to rebuild and repair the damage caused by the civil war.

This explanation may not convince everybody, but Tedde makes a good case for it. He offers a masterly narrative and a cogent economic explanation for the need of the two companies to expand and absorb smaller networks: expansion helped reduce average costs. In practice, however, this rational policy proved disastrous, especially to Norte, because the financial effort they had to make to purchase the smaller companies saddled them with an unmanageable bonded debt that their meager profits could not fully repay. So we are back where we started: if they were performing such a crucial task, why were their profits so low? Other possible, simpler explanations, such as bad management, bad planning, or bad government policies have not been fully tested. But these disagreements in no way diminish the value of Tedde's work, which is excellent. It covers many other important topics, especially company finances, with brilliance and ingenuity.

In his section on the interrelationship between the railroads and the economy, Anes attempts to establish the backward and forward linkages of the railroads: the companies sold passenger and merchandise transportation and bought coal and iron. Anes has measured the transportation output of the three largest companies (Norte, MZA, and the Andalusian company, a distant third), related this output to the other variables in the economy, and used railroad services as an indicator of overall economic activity. The results are interesting and, fortunately, offer no great surprises in either the broad cyclical fluctuations or the structural patterns; the Spanish economy was predominantly rural until the mid-twentieth century, and the composition of merchandise transported confirms this. As to the backward linkages of the railroads, Anes estimates that, by the early twentieth century, they consumed about one-third of the Spanish output of coal and iron. Anes's work is less controversial, simpler, and more straightforward than Tedde's. It ranks comparably in quality; it is obvious that they have collaborated closely.

In summary, these are useful volumes that greatly advance our knowledge of the history of Spanish railroads and economy. The first volume is more of a mixed bag while the second, more straightforward economic history, seems to this reviewer the better of the two. In any case, this is far from the last word on the topic; it is at best a partial and provisional synthesis. Few of the most fashionable issues, such as the question of social

are mentioned; many of the problems posed are not solved. For the time being, however, it is indispensable for anyone interested in the topic of Spanish railroads.

The book includes a detailed index, three statistical appendixes, and several illustrations.

GABRIEL TORTELLA

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University of Valencia*

CAROLYN P. BOYD. *Praetorian Politics in Liberal Spain*. Chapel Hill: University of North Carolina Press. 1979. Pp. xvii, 376. \$24.00.

This work illuminates one of the most complex periods in modern Spanish history, that of the breakdown of parliamentary government from the crisis of 1917 to Primo de Rivera's coup in 1923. Carolyn P. Boyd has wisely selected to concentrate on the most manageable of the many causes contributing to the breakdown: relations between the civilian governments and the army. The result is a clear and reasoned analysis of both civilian and military institutions that adds greatly to our understanding of the period.

Traditionally, historians have viewed the Spanish army's intervention in domestic politics—both in 1923 and 1936—as the result of the civilian governments' inability to provide effective stability. In fact, as Boyd points out, the army and the civilian politicians were closely involved with each other from the time of the 1876 Restoration settlement that had supposedly ended nineteenth-century praetorianism. After 1900, the politicians used the army to maintain themselves in power during domestic crises and to achieve foreign policy successes to gain popular support. Furthermore, the governments undermined army professionalism by politicizing promotions. But, as Spanish society modernized, this close relationship was both intensified and threatened by the growth of the bourgeoisie and the urban working class.

When World War I broke out, Spain could not take sides because the army was viewed as too weak. This perception, along with economic dislocations, worker unrest, and Catalan regionalism, produced the crisis of 1917 when junior army officers formed professional organizations to secure concessions. The government weathered the crisis, but, with the postwar breakdown of order in Barcelona and the military defeat at Anual in Morocco in 1921, a new crisis was created: the middle class deserted the regime. Boyd shows how the regime tried to regain confidence by strengthening its bonds with the army, but, when popular opinion opposed this move, the government turned against the army. The officers responded with Primo's coup, and the

regime then found itself without popular support: "Put in the simplest terms, the parties of the parliamentary monarchy created the praetorian army that later destroyed them" (p. xii).

Boyd describes the army structure and budget battles, the pressures on officers, and the army's attempts to avoid the politicization that made it a convenient substitute target for proletarian attacks. The army rejected its role as an instrument for solving domestic problems, but, because of its weaknesses, the size of the officer corps, and generally poor economic position, the military leaders came to see intervention in political affairs as the most obvious solution to their problems.

Boyd's careful discussion of these issues, her fine handling of sources, and her sound analysis make this study indispensable for understanding Spanish civil-military relations.

JOSE M. SANCHEZ

St. Louis University

LOUIS STEIN. *Beyond Death and Exile: The Spanish Republicans in France, 1939-1955*. Cambridge: Harvard University Press. 1979. Pp. viii, 306. \$20.00.

The last fifty years have produced (and are still producing) many millions of refugees and displaced persons throughout the world. Among these were the tens of thousands of Spaniards (both civilian and military) that in 1939 fled before the northward sweep of the victorious armies of Nationalist Spain. Although the refugees of the Spanish Popular Front were to be scattered from Russia to Central and South America, Louis Stein concentrates on the largest group, which fled into and remained in France. This is the story of these Spaniards from their arrival through World War II and its aftermath.

Drawing from the writings of the communists, anarchists, and other Popular Front leaders, a sympathetic left-wing press of France, a hostile and suspicious right-wing press, and personal interviews, the author paints a vivid picture of the shabby treatment of the Spaniards by their reluctant hosts. Then not many months later, with the outbreak of the Second World War, thousands of the former Spanish soldiers were formed into Spanish units of the French Army, Foreign Legion, and labor battalions to be used in various regions of France, including the front facing Germany and Belgium. These Spaniards fought from the Arctic in Norway through the fall of France. Some were evacuated at Dunkirk. Others were overrun by the Germans and impressed into the Todt Organization or sent to concentration camps.

Many of the Spaniards in unoccupied France were sent to German forced labor camps; some re-

turned to Spain to face criminal charges. But others were formed into resistance units. After the German invasion of Russia, thousands more swelled the partisan groups of France where they proved themselves apt in sabotage and assassination. Contact with the British Special Operations Executive (S.O.E.) is mentioned but not explored. It is doubtful if this is possible. It is estimated that 60,000 Spanish guerrillas operated in France. Other thousands formed Spanish units in the reborn French Army. Some landed in Normandy in August 1944 and fought in the dash through France to be the first troops to enter Paris. From there the Spaniards continued to Strasbourg, across the Rhine, and on to Berchtesgaden.

For the former Spanish Popular Front soldiers, the period of 1945-46 is described as one of "euphoria," but the period of 1949-56 and the Nationalist rapprochement with the West is viewed as a period of "stalemate, reversal, and final betrayal." Acts of violence and terror committed in Spain during the 1950s and 1960s by forces based in France are hardly examined. The work is an interesting addition to the library of the Spanish conflict and its consequences as well as a little-known aspect of World War II. Stein might have added that while these Spaniards were fighting in Germany other Spaniards were fighting the Russians in Berlin.

RAYMOND L. PROCTOR
University of Idaho

L. DE JONG. *Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog* [The Kingdom of the Netherlands in the Second World War]. Volume 9, *London*. In two parts. The Hague: Martinus Nijhoff, for the Rijks-instituut voor Oorlogsdocumentatie, Amsterdam. 1979. Pp. x, 752; vii, 753-1,584.

This magisterial study of the Netherlands in the most recent world war has reached a crucial point with this ninth volume (in two separate books). L. de Jong dedicates these 1,584 pages to the Dutch government-in-exile in London and its role in mounting a war effort for the Allies and developing a relationship with the occupied homeland. The end result is probably the most imposing, thoroughly documented, and interesting portrait of one of the lesser nation-states run over by Hitler and of its governmental activities abroad from May 1940 to June 1944.

The strengths of these volumes are in the non-political and diplomatic area: the Dutch contributions to Allied war making, the loss of the Dutch East Indies, and communications, propaganda, and espionage-intelligence operations. Admitting that the total Dutch assistance to the war mobilization was minor, de Jong nevertheless examines finely the rebuilding process of the army and navy and the

construction of an air arm, mostly through British help and financing and U.S. training sites. He insists that the part played by the Dutch merchant fleet became a major contribution in the significant Allied movement of materiel. His engrossing depiction of Dutch involvement in South Asia against Japan is highlighted by Dutch lack of preparation and mistakes. There are no new findings here, but de Jong succeeds in filling in the gaps through laborious research in the state archives. For specialists on the war, occupied territories, and dislodged governments, the author is most valuable for his information and insight into London-Netherlands relationships. The S.O.E.-Dutch connection with its "Englandspiel" focal point is made tragically clear, illustrating early failures at coordination and cooperation. The section on the C.I.O. (Intelligence) under van't Sant further shows the difficulties of joint operations. Yet de Jong details the use of radio for propaganda and its effectiveness and the fruitful post-1943 establishment of ties with the Dutch resistance. The posture and actions of London on the question of Jews are treated in detail, and this part of the series becomes a valuable addition to our comprehension of the unique Dutch responses in this area.

The management of foreign policy by the Dutch in London and questions of politics are certainly addressed but often with disappointing brevity or strange simplicity. The issues of the first prime minister, de Geer (a collaborator), the political place of Queen Wilhelmina, Gerbrandy's accommodating cabinet politics, and van Kleffens's involvement in Benelux diplomacy are not convincingly covered in detail. Given its historic role in later European integration, this last-mentioned defect is striking. Perhaps least complete is a definition and elaboration of the Dutch government's role in nonmilitary Atlantic cooperation. Certainly the Anglo-American nexus of power and policy making is emphasized, but the interaction among the Dutch and their friends (on postwar reconstruction, a customs union, and colonies questions) is severely limited by space. The next volumes, however, may fill in some of these voids.

In conclusion, these books are indispensable tools for the historians of World War II and most related questions. Along with Werner Warmbrunn's study of Dutch life on the continent, de Jong's remarkable contribution deserves careful attention. If every nation had its de Jong, our knowledge and understanding of that war era would be marked by more books of real value and distinction.

PIERRE-HENRI LAURENT
Tufts University

LEO TANDRUP. *Mod triumf eller tragedie: En politisk-diplomatisk studie over forløbet af den dansk-svenske magtkamp*

fra Kalmarkrigen til Kejserkrigen. volume 1, *Scenen og de agerende. Tiden fra 1612 til 1621*; volume 2, *Tiden fra 1621 til 1625* [Toward Triumph or Tragedy: A Political-Diplomatic Study of the Course of the Danish-Swedish Power Struggle from the War of Kalmar to the Imperial War. Volume 1, Scenes and Actors, 1612–21; volume 2, 1621–25]. Summary in English. (Skrifter Udgivet af Jysk Selskab for Historie, number 35.) Aarhus: Universitetsforlaget i Aarhus. 1979. Pp. 510; 573. 216.45 KR.

Toward Triumph or Tragedy is a study of the political, diplomatic struggle for power between Denmark and Sweden from 1612 to 1625. There is no way the fourteen-page summary in English, nor this review, can do justice to this detailed piece of research and analysis. More the shame, because it deals with two of the leading participants in the Thirty Years' War who ultimately sank to the level of third-rate powers.

The title tells the story. Christian IV's entry into the Imperial Wars was to be a triumph but became a tragedy. Leo Tandrup explains why. He prefaces his "Konklusion" with a statement by George F. Kennan: "History does not forgive us our national mistakes because they are explicable in terms of our domestic politics. . . . A nation which excuses its own failures by the sacred untouchableness of its own habits can excuse itself into complete disaster" (vol. 2, p. 517). The "sacred untouchableness" was the insistence of the Danish aristocratic council that their monarch, Christian IV (1588–1648), not be allowed to establish a strong and independent foreign policy. Since this would threaten their own hegemony in Danish society, they refused to fund military measures necessary for the security of the kingdom. "The severely restricted support which the Council gave the royal Swedish and German policy in the years leading up to the Imperial War impeded Christian in the pursuit of a consistent activist policy, which could have prevented Sweden from making such crucial gains in the eastern Baltic, and at the same time could have secured Christian a positive sphere of influence in northern Germany. But for the Council the immediate interest of the Estate (Aristocracy) weighed more heavily than the future security of the nation. Largely for this reason the Council strongly opposed the Imperial War, which Christian was forced to engage in at a time when circumstances, after all, could have been better" (vol. 2, p. 543).

Denmark's defeat by the Imperial forces not only thrust Sweden into the leadership of the Protestant League and caused the defeat of the Catholic armies but also enabled Sweden to consolidate its eastern Baltic possessions as a prelude to the dismemberment of Denmark in 1645 and 1658. If the title of this work means anything, Tandrup sees this

as a tragedy. Judging from the Swedish experience as a great power, one wonders how much longer Denmark might have postponed a tragedy.

Beyond its thoroughness, there are several other virtues to this work. It is the first to integrate the domestic scene in Denmark with foreign affairs. Much attention is given to the internal and external economic conflicts in Denmark and Sweden as well as between them, ranging from the northern tip of the Scandinavian peninsula to the southern shores of the Baltic. The reader also gets the long view of the Danish-Swedish struggle for control of the North and the Baltic.

Of special interest in volume 1 is the historiographic chapter covering both Danish and Swedish studies and a chapter that analyzes the "principal actors" and the basis for their policies. The volumes are handsomely bound, well illustrated, and each has both a personal name and place name index. Learn to read Danish!

PLAYFORD V. THORSON
University of North Dakota

MICHAEL ROBERTS. *The Swedish Imperial Experience, 1560–1718*. (The Wiles Lectures Given at the Queen's University of Belfast, 1977.) New York: Cambridge University Press. 1979. Pp. x, 156. \$17.95.

The numerous and interesting aspects of Swedish empire building in the sixteenth and seventeenth centuries have been competently investigated before. Without adding to our information, Michael Roberts now presents a masterly, most readable new summary and analysis. He contrasts the views of the older Swedish historians who speak of an "empire for defense" with those of the "new school" who emphasize the economic-military causations and implications. Certainly dangers threatened Sweden from the side of Denmark, Poland, and Russia; yet personal ambitions of the Swedish rulers, temptations created by an existing power vacuum, economic advantages for a state playing increasingly the role of entrepreneur, territorial conquests luring a nobility that at home was losing its rights vis-à-vis the throne—all these factors may overshadow the role that "defense" played. Sweden's ambitions could, however, be realized only if "war fed on itself"—which it did.

Roberts deals further with the modernization of Sweden's military administration and tactical innovations, with its entrepreneurs, lawyers, and clergy, and with its "mystique." He emphasizes that Sweden's reputation for a political system that provided for the "freedom" of its people and for a measure of representative institutions aided in achieving its goals. Peculiarly, in this connection Roberts puts

no accent on religion, as if Lutheranism was not a prime force helping Sweden in its bid for power.

Roberts then investigates the effects of Swedish rule—insignificant in Germany, considerable in Livonia and Ingermanland, dangerous for Sweden's neighbors, and incisive for the Swedes themselves. One of the most important consequences was the long, disruptive struggle for the *reduktion* of the estates in Sweden and Livonia.

Roberts suggests that the "empire" was held together chiefly by commerce and the military, less by law, and certainly not by tradition. Perhaps the energy of the kings, the greed of the nobles, and the weakness and discord of the neighbors merit more emphasis, especially since, according to him, trade was no success, conquests did not serve defense but constituted a liability, power and prestige were at best temporary achievements, naval control of the Baltic was never gained, and financial health, especially during peacetime, was endangered. Indeed, not even the conqueror himself gained major lasting benefits.

Some objections to Roberts's presentation result from his using almost exclusively Swedish materials. Statements that most of the war booty flowed back into the local economies or that Swedish rule contrasted with "oppressive" conditions elsewhere (the word "oppressive" is overly used) are questionable. The role of the Finns in the armies (and their reputation for magical powers) is not mentioned, nor is the lasting memory of the brutality of the armies. (For centuries afterward, parents used the cry, "The Swedes are coming!" to scare naughty children.) Yet, the presentation is lucid, convincing, a tribute to the author, and a gain for the student of European history.

There is no index, bibliography, or maps.

WALTHER KIRCHNER
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GERHARD SCHORMANN. *Hexenprozesse in Nordwestdeutschland*. (Quellen und Darstellungen zur Geschichte Niedersachsens, number 87.) Hildesheim: August Lax Verlagsbuchhandlung. 1977. Pp. viii, 182. DM 40.

This study pulls together a remarkably complex body of information from the archives of the entire region of northwestern Germany, including Lower Saxony, Bremen, Oldenburg, Schaumburg, the Westphalian territories, and a host of cities as well. So difficult was this work of assemblage that a large portion of the book is given over to outlining what can (and often cannot) be known about the witch trials throughout this area. At this level of detail, Gerhard Schormann corrects numerous false conclusions that have flourished like hardy perennials

from the days of Soldan to our own. He demonstrates, for example, that the legendary savagery of Heinrich Julius of Braunschweig-Wolfenbüttel has been grossly exaggerated. Unfortunately, Schormann is content to leave this analysis without pushing for more general conclusions, noting only that witchcraft trials tended to become more severe as one moved westward from Lower Saxony into Westphalia and that Westphalian trials also involved large numbers of men.

Another peculiarity of Schormann's study is that his thought is so dominated by a theological definition of witchcraft (apostasy, pact with the devil) that he regularly distinguishes "true" witchcraft trials from trials for mere sorcery. This distinction has been common enough in the historical literature of the last one hundred years, but the research of Norman Cohn, Richard Kieckhefer, and Robert Muchembled have pretty well proved that the real distinction is between learned conceptions of witchcraft as diabolism and popular notions of witchcraft as harmful magic. As a result, Schormann does not connect his study to the growing literature on popular culture, although his study of legal consilia does contribute to our understanding of what can be called the imperialism of the learned.

Schormann also looks for periodic patterns among the chaos of his data and finds three peak periods of witch hunting: 1590, 1630, and 1655. The important task remains of making sense of such patterns in Germany as a whole, and indeed throughout Europe. Schormann presents an excellent section on legal procedures in northwestern Germany, again destroying a number of commonly held assumptions. In a final section he also comments on the most commonly defended general explanations for the phenomenon of witchcraft trials. By now it is no surprise that Schormann finds them all wanting, at least when applied to his region. It is an effective work of demolition, one that reminds us how much of the past is still a mystery.

H. C. ERIK MIDELFORT
University of Virginia

PAUL GOTTFRIED. *Conservative Millenarians: The Romantic Experience in Bavaria*. New York: Fordham University Press. 1979. Pp. 176. \$22.50.

Reading this book reminded me of a wisecrack that used to circulate among visitors to Munich concerning the *Feldhermhalle*, the pantheon of the military leaders of Bavaria, which stands in the center of the city displaying statues of Tilly and Wrede. There have been only two Bavarian heroes, it was whispered maliciously, and of these one was not a Bavarian, while the other was not a hero. This mild but unkind joke suddenly popped into my head as I

thought about the romantic conservatives whom Paul Gottfried describes. On the face of it, Bavaria does not appear to have been one of the great centers of romanticism, artistically or philosophically. Not only is it hard to think of a leading figure in painting, music, or literature who was born there, but even among the conservative social theorists the big names all came from other countries. De Maistre was a Savoyard, Haller a Swiss, Müller a Prussian. The Bavarian romantic philosophers were, most of them, second-raters, so that the author has reinforced his book with thinkers who were half-Bavarian or non-Bavarian, half-romantic or even nonromantic. Baader and Döllinger were natives of Bavaria, Görres and Schelling at least lived there, and Novalis and Friedrich Schlegel were total strangers. Similarly, it is hard to think of Ludwig I, that royal good-time Charlie with his palaces and mistresses, as a serious romantic figure. What we have here, then, is an analysis of a group of Central European philosophers and the impact of their ideas on a state that was an intellectual backwater.

But if the thinkers described in this book are generally minor, the author describes them with sympathy, intelligence, and literary grace, perhaps greater than they deserve. He has squeezed every bit of significance out of them, really more than they possess. He deals first with the religious revival at the beginning of the nineteenth century, the age of Montgelas, the portents of romanticism, and the Catholic renewal under Ludwig I. Then comes an analysis of the dominant ideas of the romantic conservatives, their quest for historic origins, the eschatology implicit in their teachings, and their war against liberalism. The author examines their intellectual positions perceptively and persuasively. He rejects the view, under sharp attack from many other writers, that romantic conservatism was only a defensive and unimaginative response to the assault against feudal institutions. He denies, as most scholars now deny, that it must bear major responsibility for the irrational cults and totalitarian movements of the twentieth century. The importance he attaches to what the romantics had to say about the problems of modern society, on the other hand, may be a little excessive. As he himself concedes, "there is an obvious gulf between their fire-eating rhetoric . . . and the academic and journalistic quarrels by which they hoped to advance 'the truly living, the eternal, and the good'" (p. 148). The contribution of this book lies basically in enhancing our understanding of romanticism by examining the effect of its ideas on one small corner of Central Europe.

THEODORE S. HAMEROW
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CHARLES E. MCCLELLAND. *State, Society, and University in Germany, 1700-1914*. New York: Cambridge University Press. 1980. Pp. ix, 381. \$29.95.

In 1868, when Matthew Arnold wanted to convey to his English readers the peculiar virtues of German higher education, he quoted a recent pamphlet on the subject by Heinrich von Sybel. The German university, Sybel wrote, enables a student to "qualify himself, through the strict service of science, to be of avail in the service of his country." Service to science *and* to the nation—here, in a phrase, is the source of the German university's great achievements and of its most persistent problems.

Charles E. McClelland's book traces the historical development of these achievements and problems, from the first stirrings of educational reform in the eighteenth century until the outbreak of the First World War. McClelland begins with a brief description of the universities' dilapidated condition around 1700 and then shows how Göttingen emerged as a new model of educational excellence; the next section concerns the revolutionary era and the establishment of the University of Berlin; from there we turn to the exciting period between the onset of reaction in 1819 and the *Reichsgründung*, with particular emphasis on the growth of a "research ethic" and of professorial prestige and political influence; finally, there is a long section on the imperial universities, which combined impressive material growth with a decline in academic autonomy and a lamentable narrowing of the professoriate's political vision. McClelland tells the complex story with great skill; his control of the literature is impressive; the writing is always clear and often graceful. Readers will, I think, be especially grateful for the information provided on the size and composition of professoriate and student body, the financial condition of universities, and the changing patterns of academic interest.

Three essential themes emerge from McClelland's analysis. First, the bureaucratic state played a fundamental and usually decisive role in the evolution of universities. Second, the universities' most characteristic social function was their role in the affirmation of elites, a process that provided limited opportunities for some new groups but usually favored those already well established in the economic or status hierarchies. Third, although the universities certainly contributed to the growth of German political power and material prosperity, their relationship to "modernization" was always complicated and problematic. This was true of Göttingen in the eighteenth century, where scholarly accomplishments were combined with an emphasis on teaching courtly skills, and of Berlin in the nine-

teenth, where progressive reformers insisted on the importance of neoclassical ideas in the formation of a *Bildungs* elite.

Since McClelland must build on the foundation of existing scholarship, his account is sometimes uneven. We learn more about a few, well-known universities than about the majority of lesser ones. There is less information than one would like about the peculiar problems and accomplishments of natural science as an academic enterprise. And the treatment of some subjects, most notably the evolution of *Bildung* as ideal and ideology, seems elusive and incomplete. But these are the unavoidable flaws of a work that concerns itself with a subject as broad and rich as McClelland's. They do not detract from the service he has rendered to historians of Germany and of higher education by providing a guide to the current state of our knowledge and by pointing us toward the questions that remain to be addressed.

JAMES J. SHEEHAN
Stanford University

PETER JOERISSEN. *Kunsterziehung und Kunstwissenschaft im wilhelminischen Deutschland, 1871-1918*. (Dissertationen zur Kunstgeschichte, number 10.) Cologne: Böhlau Verlag. 1979. Pp. xi, 424. DM 68.

Among the many recent efforts to analyze the educational philosophies and practices of German schools in the nineteenth and twentieth centuries, this revised and shortened doctoral dissertation offers a very informative and suggestive approach to a relatively neglected topic. Peter Joerissen has sought to integrate a vast amount of polemical essays, ministerial investigations, and decrees as well as recent general historical interpretations into a single analysis of art education and the development of popular esthetic theory in imperial Germany. Although his efforts are understandably fragmented by the nature of such a study, and this detailed monograph falls short of its goal, it makes a valiant attempt at describing the relationships between art as a subject of instruction, the expectations of educational theorists for a heightened awareness of esthetic standards, and the intense, competitive, nationalistic world of imperial Prussia.

Joerissen, following an unfortunate tradition of other German histories of education, offers a largely Prusso-centric narrative. That other regions of Germany, notably Württemberg, Baden, and Bavaria, had alternative traditions in art education or differing educational philosophies about the proper role of drawing instruction in the public primary and secondary school systems should have been taken into account. Despite its details, his study will re-

quire other supplementary analyses before we can understand the entire Germanic orientation to this topic.

Within his narrative of Prussian administration, Joerissen jumps back and forth from the *Volksschule* to the secondary schools, often with very little clarification for the reader and without clear chronological development. Many Prussian *Volksschulen* and higher schools alike had instruction in drawing, both creative and mechanical, long before its formal, mandatory introduction into the curriculum in 1872 with the Falk Laws. The author could have offered at least a summary of the earlier efforts—none less than Heinrich Pestalozzi and Adolph Diesterweg endorsed the need for the development of a child's esthetic sense and hand-eye coordination. Joerissen's evidence seems to indicate that art was to serve patriotic pride and economic gain: many Prussians were ashamed at the relatively poor taste in form and design on display in German products at the international expositions. Further, drawing education "must have a substantially moderating influence on the vulgar customs of the present day" (p. 28), according to one antisocialist educator writing in 1879. This would indicate that political ideology and education for social control also entered this area of the curriculum.

The monograph contains twenty-four illustrations from contemporary drawing lesson books, a very full bibliography, and extraordinarily detailed footnotes. Had the author integrated his research into the general framework of educational experiences in imperial Germany, the work would have been definitive. As it now stands, it is a thorough description of the administrative goals and conflicts in the teaching of drawing in the Prussian public schools. Whether these goals were actually accomplished beyond the administrative decrees and the ambitions of the idealistic theoreticians remains to be investigated.

DOUGLAS R. SKOPP
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TONI PIERENKEMPER. *Die westfälischen Schwerindustriellen, 1852-1913: Soziale Struktur und unternehmerischer Erfolg*. (Kritische Studien zur Geschichtswissenschaft, number 36.) Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. xi, 268. DM 58.

This investigation of Ruhr industrialists tabulates evidence on the careers of 248 entrepreneurs in mining and in iron- and steelmaking. Toni Pierenkemper chose these 248 from 857 entrepreneurs active during the period 1852 to 1913 because he found adequate information on them. Although claiming

that they constitute a fairly good sample of heavy industrialists in the Westphalian Ruhr, he notes that men from larger, mixed coal-mining and metal-producing firms are heavily overrepresented. The geographical boundaries of his study follow, certainly for the sake of convenience, political boundaries: he omits the western end of the Ruhr district (the end that includes Essen, Mülheim, Oberhausen, and Duisburg) because it belonged to the Rhineland province of Prussia. Pierenkemper's sources consist almost exclusively of published works containing information on the industrialists or their firms: registers, statistical surveys, business periodicals, *Festschriften* issued by the firms, and biographical entries in reference works.

His findings will sound familiar to historians of modern Germany: most Ruhr industrialists came from the upper social and economic strata, were well educated, and were born in the district or in neighboring areas. One conclusion runs counter to a common assertion of popular and scholarly literature: Pierenkemper has uncovered little evidence that entrepreneurs had their origins among artisans and craftsmen, but his monograph is too limited in scope, method, and sources for us to dismiss as mere myth the persistent tale of the craftsman who became an industrialist during the early phases of industrialization. Pierenkemper suggests that merchant and entrepreneurial families supplied most of the entrepreneurs. He speculates that the wealthy and socially well situated had better access to the capital market than craftsmen and artisans. Before we can do much with this idea, we need additional research on the Ruhr and other areas undergoing and completing basic industrialization. Pierenkemper's book provides little evidence for his suggestion that favorable social origins were more important to the founding of an enterprise than to its survival.

Most of the last third of the book is devoted to the characteristics of the successful entrepreneur. Critical to Pierenkemper's definition of success is the survival of the man's firm. As in the earlier chapters, he concedes cheerfully that his criteria pose serious problems, but in this instance he slights the difficulties: we need more knowledge of the relationships among firms; we need more research into German business history before we can virtually equate the disappearance of a firm with the failure of an industrialist, especially during the era of cartelization and concentration that began in the 1870s. Pierenkemper's endeavor to define his crucial terms in such a way as to facilitate the collection of quantifiable data occasionally becomes self-defeating, as another example also brings out: what he has to tell us about the politics of the industrialists is vague except for a tantalizingly brief presentation of some readily accessible, quantifiable information

indicating that many were active in communal politics and industry associations.

Model building derived from the literature on the functions of managers and entrepreneurs dominates Pierenkemper's work and circumscribes his findings. Although modest, his contribution to historical inquiry is very welcome. It is to be hoped that he and others will unearth more material on such important questions as the social origins and situation of German industrialists in the nineteenth century.

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OTTO BORST. *Schule des Schwabenlands: Geschichte der Universität Stuttgart*. (Die Universität Stuttgart, number 1.) Stuttgart: Deutsche Verlags-Anstalt. 1979. Pp. 510. DM 48.

This chatty, semipopular history of the technical university at Stuttgart traces the development of the institution from its founding as a *Gewerbeschule* in 1829, through its emergence as one of the leading *technische Hochschulen* of Germany by the 1890s, and then in increasingly less detail down to the present. Otto Borst's work is the only general history of the institution, and its strengths lie in the author's strong sense for local color and the vividness of his character sketches. As a scholarly source the work has liabilities. It rarely attempts to portray the Stuttgart polytechnic against the general development of similar institutions across Germany; it shows minimal concern with some of the larger historiographical questions raised in other recent literature on universities and technical schools; and it is less than systematic in its accounts of issues like funding, enrollments, and curricula. Historians will find the book a readable introduction to a previously little-known institution but of little theoretical significance to the historiography of higher learning in Germany.

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KENNETH R. CALKINS. *Hugo Haase: Democrat and Revolutionary*. Durham, N.C.: Carolina Academic Press. 1979. Pp. x, 254. Cloth \$14.95, paper \$7.50.

This is the first substantial biography of the co-chairman of the Social Democratic Party of Germany from 1911 to 1916 and founder and leader of the schismatic Independent Social Democratic Party from 1917 until his assassination late in 1919.

With the Haase papers a casualty of the Nazi period, Kenneth R. Calkins has relied principally upon party and Reichstag debates, the papers and memoirs of Haase's associates, and the selection of letters and speeches published more than fifty years ago by Haase's son, Ernst, to illuminate his subject's mature political ideas and career. Hence a scant fifteen pages are devoted to the first forty-seven years of Haase's life. The result is a useful political biography, but one that conveys only an indistinct image of Haase the man.

Calkins's findings confirm previous judgments by historians of the German left. A self-effacing and unambitious man of high ethical ideals, Haase strove mightily for party unity whenever that could be squared with his Marxist conscience. In the short run, at least, that made him an appropriate leader of German Social Democracy at a time when it was becoming polarized between a growing reformist faction and a revolutionary opposition. Next to Karl Kautsky the most prominent spokesman for a declining centrist group that sought to reconcile gradualists and radicals, Haase found himself in a still more difficult position once the issue of supporting Germany's policies in World War I drove the two factions further apart. Haase's growing conviction that the Kaiser and his generals were fighting an imperialist war and were therefore unwilling to settle for a compromise peace finally overrode his devotion to socialist unity and impelled him to join like-minded comrades in breaking party discipline. As leader of the Independents, however, Haase once again found his capacity for conciliation and diplomacy strained to the limit; the new party, united in its opposition to the war, was divided in nearly everything else. Its subsequent disintegration probably would have occurred even if the assassin's bullet had not found its mark.

There is much to recommend Calkins's view of Haase as a fundamentally tragic figure. He devoted his life to promoting socialism, democracy, and justice, only to see them deeply compromised by a series of disasters over which he had little or no control. But Calkins presents more evidence than he acknowledges of Haase's incapacity for practical politics. Although Haase was no dogmatic Marxist, his political and ethical absolutism contributed to the catastrophic radicalization of the Social Democratic left and to the hopeless revolt of the radicals against Friedrich Ebert's postwar provisional government. His last-minute efforts to avoid violence could not halt the train of events he himself had helped set in motion. It may be that Haase's legal defense of workers and party activists accused of political crimes—a dimension of his career to which Calkins devotes little attention—made a more significant positive contribution to the history of Ger-

man socialism than anything he did as a party leader.

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REINHARD SCHIFFERS. *Der Hauptausschuss des Deutschen Reichstags, 1915-1918: Formen und Bereiche der Kooperation zwischen Parlament und Regierung.* (Beiträge zur Geschichte der Parlamentarismus und der Politischen Parteien, number 67.) Düsseldorf: Droste Verlag. 1979. Pp. 305. DM 68.

Among the factors furthering the establishment of parliamentary government in Germany during the First World War was the work undertaken by parliamentary committees. The changing rights, functions, and increasing influence of the main parliamentary committee, the *Hauptausschuss* or budget committee, have been examined for the first time by Reinhard Schiffers. He demonstrates how that committee overcame constitutional restrictions and served as the main counterweight to the military leaders by 1917. Relying heavily on the documentary editions of the Kommission für Geschichte des Parlamentarismus (Bonn), such as the editions that presented the minutes of the interparty caucus meetings from 1917 to 1918, Schiffers uses the twenty thousand pages of unpublished minutes of the budget committee to show more fully than previously how interparty cooperation emerged, how the government responded to parliamentary pressures, how crucial precise information was to committee actions, and how parts of the Weimar parliamentary system were laid down by the end of the imperial era.

The two-part study centers on the constitutional, organizational, and personal factors determining the relations between the *Hauptausschuss* and the government as well as the areas of cooperation and conflict between them. Although this thematic approach results in some overlap and in a rather dull text, the author competently brings forth the parameters and factors affecting the potential of the committee's influence. In particular, he shows how the constitutional limits—meeting only when the Reichstag was in session, restrictions on discussion of foreign affairs, restrictions on reviewing military matters—were slowly eroded. Especially interesting and noteworthy is the discussion on the problem of confidentiality versus publicity; the government would share information only if it were treated as secret, whereas the committee members were also under obligations and pressures from their parties and constituents to publicize issues.

Although the study contains few startling conclusions, it does bring forth some new information

and ideas. By November 1916, Schiffers demonstrates, the *Hauptausschuss* had become a "relatively independent instrument" as a parliamentary watchdog over government (p. 259). In this role, as well as the attempt during 1917 to influence and to control military policy, the bourgeois parties took the lead. Similarly, it was not the Social Democrats who effectively employed the threat of withholding budgetary or fiscal support in order to widen the committee's powers. Schiffers shows that the *Hauptausschuss* became a substitute for meetings of parliament per se by 1916 and in some instances a means by which to circumvent calling parliament together. In addition, he convincingly illustrates that the major political parties cooperated through this committee well before the creation of the interparty caucus in 1917; indeed, this committee served as another instrument to integrate political opponents of the Kaiserreich. The consequences for the Weimar Republic were evident in that the constitution of 1919 provided for committees with similar rights, which continued the wartime work of the *Hauptausschuss*.

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HERMANN J. RUPIEPER. *The Cuno Government and Reparations, 1922-1923: Politics and Economics*. (Studies in Contemporary History, number 1.) Boston: Martinus Nijhoff. 1979. Pp. viii, 289. \$24.30.

Walther Rathenau, responsible for reparations in the preceding government, said he was the first of those who had to plunge into the trench so that others could get across. Assassins left him there, dead. Hermann J. Rupieper's account suggests that Wilhelm Cuno, chancellor of Germany from November 1922 to August 1923, served his country similarly, although without the style or martyrdom. Rupieper has added the final and essential link to the detailed narrative of the period begun by Ernst Laubach in *Die Politik der Kabinette Wirth, 1921-1922* (1968).

The monograph efficiently carries out its objective, as stated in the "Conclusions," of "analyz[ing] decision-making processes during a crucial period of the Weimar Republic" (p. 255). Rupieper has mastered the archival sources and shaped the anarchic events into meaningful proportions. But this is not quite what his subtitle had promised, since he has ventured only a short distance into economics. He could better have illuminated German policy with a close analysis of the national income, budget, indebtedness, balance of payments, and rate of inflation—the acute economic problems demanding Cuno's decisions.

Rupieper's study, however, has many more strengths than shortcomings, and its dense factual-

ity documents the objective difficulties the Cuno government faced and puts to the test a new wave of Franco-American scholarship that includes the work of Denise Artaud, Jacques Bari  ty, Georges Soutou, Sally Marks, Stephen A. Schuker, Charles S. Maier, Walter A. McDougall, and Marc Trachtenberg. Hard-working and adventurously heuristic, these latter have raised up masses of new research and such large ideas as France's role on the Rhine, European peace and integration, and the burgeoning of new social hierarchies. Setting their standard by their views of these phenomena, the new historians see reparations as scaled down to secondary magnitude, well within Germany's capacity to pay and unpaid only because of a lack of good will. Yet like Rupieper, whose solid but modest monograph their conceptions would overwhelm, these scholars have failed to produce the requisite economic-mathematical demonstration. The reciprocal testing of the opposed theses promises to continue as other work is done.

As to the specifics of the reparations problem, Rupieper shows, contrary to historians like Erich Eyck who have emphasized the Cuno government's "business ministry" character, that its policy was the logical extension of his predecessor's. Joseph Wirth, Rathenau's chief, had entered office in May 1921 as an exponent of fulfillment but was unable to maintain even the pretense of it by August 1922, when he halted cash payments. Rupieper sees Cuno carrying on in the same way, trying to keep the economy from collapsing and passively waiting for the Allies to soften the rigors of their measures. Relief required a painful catharsis, which included the Ruhr occupation, resistance that was, in fact, more than passive, and the destruction of the mark. The effect was to wear down German recalcitrance, French aggressiveness (and the franc), English patience, and American reluctance to associate itself with European tribulations. With the promise of what became the Dawes Plan, Gustav Stresemann could scramble across the trench on Rathenau's and Cuno's shoulders and lead Germany into the—deceptively—better years of the 1920s.

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HERBERT H  MIG. *Das preussische Zentrum in der Weimarer Republik*. (Ver  ffentlichungen der Kommission f  r Zeitgeschichte, Series B: Forschungen, number 28.) Mainz: Matthias-Gr  newald-Verlag. 1979. Pp. xlvii, 336. DM 68.

Nearly fifty years have now passed since the collapse of Germany's Weimar Republic. Since then we have learned much about that republic. The pe-

cular character of the revolution that gave it birth, the careers and personalities of many of its political leaders, and the final stages of its dissolution are among the major topics that have been studied extensively. Less well known, however, is the Weimar Republic's history at the regional or state level. Herbert Hömig's *Das Preussische Zentrum* seeks partially to fill this gap by providing a political narrative of the Catholic Center Party in Prussia between 1918 and 1933.

Writing within the framework of traditional political history, Hömig meticulously describes the Center's role in the formation and persistence of the Weimar Coalition (an alliance comprising the Center, Socialist, and Democratic Parties) in the Prussian Landtag. A basic factor contributing to the Prussian Center's preference for the Socialists over the conservative German Nationalists, Hömig claims, was the Roman Catholic demand for parity with other confessional groups in the allocation of civil service appointments. This demand threatened the entrenched Protestant, monarchical, and conservative elements in the Prussian bureaucracy and precluded any form of working relationship between the Center and the political right. Cooperation with the Socialists, on the other hand, had its advantages. According to Hömig, the elimination of much of the anti-Catholic discrimination within the Prussian civil service and, above all, the conclusion in 1929 of a Concordat between Prussia and the Holy See represented the undeniable success of this policy.

But the very success of this policy, Hömig argues, bestowed a tragic legacy on political Catholicism in Prussia. When Nazi successes in the state elections of April 1932 and Chancellor Franz von Papen's coup three months later against the Prussian government destroyed the parliamentary basis of the Weimar Coalition in the Landtag, the Center, in a grotesquely misguided attempt to tame the Nazis, employed the old and familiar methods of cooperation that had proven so successful with the Socialists. These efforts, Hömig maintains, only exacerbated the Center's internal strife, intensified demands for an even closer accommodation with the Nazis, and contributed to the party's ignominious collapse in 1933.

For all the importance Hömig attaches to these internal conflicts, he unfortunately does not make them an integral part of his discussion of the Center's tactics, program, and behavior. Nor is he able to do so because he confines his focus to the Center's parliamentary delegation to the Prussian Landtag—the so-called *Fraktion* of 67–89 members—and its activities. This preoccupation with the highest reaches of the political process accords scant attention to a sustained analysis of electoral data, the significance of regional variation, the “grass-roots” di-

mension of party politics, and the reciprocal relationship between the party's plurality of interests and the parliamentary *Fraktion*. It disregards the social context in which political issues are debated and decisions are made. Although Hömig's narrow frame of reference clarifies Centrist motives for participation within the Weimar Coalition in Prussia, it also unduly obscures those economic (conspicuously absent, for example, is any discussion of the hyperinflation or the depression) and ideological factors that played a crucial role in heightening discord within political Catholicism and in weakening the Center's ties to its partners in the coalition.

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KLAUS-JÜRGEN MÜLLER. *Armee, Politik und Gesellschaft in Deutschland, 1933–1945: Studien zum Verhältnis von Armee und NS-System*. (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1979. Pp. 123. DM 12.80.

The three essays in this book reflect Klaus-Jürgen Müller's continuing interest in problematical aspects of the German Army since his earlier magisterial study, *Das Heer und Hitler* (1969). The first essay is an “attempt at a historical interpretation” of the relationship between “Army and Third Reich”; the second offers “reflections and results of new research” on General Ludwig Beck, Chief of the General Staff, 1933–38; the third addresses the “problem of interpretation and analysis” of the military opposition to Hitler.

Biographical methods and “political-ethical and personalistic” approaches (p. 102) to understanding actions of individuals in central positions seem even less useful to Müller now than in his earlier study. He treats his subjects as a group, an “elite,” having convinced himself that “political-moral criteria have no sufficient rational power of explanation” and that the political-moral complex of problems (motivations) was “nonscientific” (pp. 13–14). Instead, he uses terms such as “Junker feudalism,” “Prussian-German military elite,” and “the phenomenon of the total war involving all of society in the era of industry and technology” (p. 8), which might be useful, but whose scientific superiority is not clear as they remain undefined.

Müller's essays explore variations on three themes: (1) The German officer corps of the 1920s and 1930s insisted on being a “political-social leadership elite as well as a military-professional one” (p. 22) and on participation in political decisions. (2) This “elite” cooperated with National Socialism and helped to put Hitler into power, in an “entente” the “elite” thought it had concluded with

Hitler, based on common interests and agreement on policy (pp. 30–33, 48). (3) All major conflicts between the military (army) “elite” and National Socialism were attempts to restore the political role of the “elite” and the “entente” of 1933 and ultimately led a minority of the “elite” to direct opposition and coup d’état (pp. 47–48). The majority of the senior officer corps, meanwhile, continued to agree “fundamentally” with Hitler’s policies, despite their reduction to a “purely functional elite” (p. 48).

The attempt to explain historical events through collective categorizations and theses first distilled from the same events, without conclusive evidence, leads Müller to contradictions. Elite theories, and the virtual exclusion of “political-moral criteria,” cannot explain the revolt against the crimes of the Nazi state. A case might be made for the conspirators acting from a perceived obligation as a “political-moral” elite when they set out, in July 1944, to sacrifice their lives for Germany’s honor, fully conscious that they could save neither the state, nor its territorial integrity, nor their own status. The glaring contrast is the unexplained collapse of the (alleged) political-social elite foundation in large parts of the senior officer corps, in the face of the Commissar Order and the Court Martial Order Barbarossa of 1941: the murder of prisoners and mindless obedience were not ideals in the “Prussian-German tradition” upon which Müller says the “elite” based itself (p. 9). A more general frame of reference for historical explanation, and preferable to untested elite theories, would be the relationships of officer corps to governments in several states over a longer time, based on Carl von Clausewitz’s doctrine of the relationship between “policy” and military leadership. The force of Müller’s argumentation is weakened further by frequent references to works not yet available, particularly one of his own on General Beck, which contains many relevant sources and references and which will not be published (according to information from the publishers) before the end of 1980 at the earliest.

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FRIEDRICH GRUNDMANN. *Agrarpolitik im “Dritten Reich”: Anspruch und Wirklichkeit des Reichserbhofgesetzes*. (Historische Perspektiven, number 14.) Hamburg: Hoffmann and Campe. 1979. Pp. 233. DM 58.

The objectives of this useful, if rather thin, treatise on hereditary farm entailment in the Third Reich are to throw light on the whole National Socialist system of government in practice. Its main theme is the relationship between ideology, meaning the the-

ories of Darré and the “blood and soil” school, to the overall social and economic development of German agriculture under Hitler. Friedrich Grundmann tries to determine to what extent farm legislation was the product of a system of values at least partially inherited from the nineteenth century. How these measures actually functioned after 1933 and what effect the demands of rearmament and industrial re-employment exercised on them is explored in detail. Not surprisingly, the English summary concludes that “the agrarian fanatics could not reach their overall aims” (p. 228).

This conclusion is not exactly original but has at least the merit of reinforcing orthodox opinion as to how “blood and soil” eventually ceded pride of place to pragmatism in the Third Reich. The section that illustrates Darré’s failure to introduce a sweeping debt-relief scheme for entailed farms because of Schacht’s opposition is a good example of this. Similarly, the counterproductive effects of credit restrictions imposed on entailed farms and how these worked against the *Erzeugungsschlacht* are well documented. Peasant dislike for the Hereditary Farm Law of 1933 is again brought out, as is their lack of interest in Hitler’s nebulous schemes for eastern colonization. Informative here is a report from the Cologne area in 1941: rumors of settlement in the east or in Burgundy were causing “eine spürbare Unruhe” in the farm population (pp. 74–75). So much for Lebensraum.

Despite the ineffectiveness and unpopularity of “blood and soil” thinking, the author accepts its overwhelming influence on the actual legislation as conceived in 1933. He thus puts himself firmly among those historians who have emphasized the neo-feudal aspects of NS ideology. Quite explicit in the conclusion is an attack on the concept of National Socialism as some kind of modernizing force, if only by accident. In sum, however much “blood and soil” eventually came to grief, we should not lose sight of how much it originally contributed to holding back modernization on the German farm scene. Grundmann’s arguments are in this respect convincing.

Less satisfactory, however, is the material on the actual farm law itself, which adds little to our knowledge. Only six pages are devoted to the special courts, rather thin even for a monograph. Despite the fact that this work started as a dissertation, there is a good deal of material that is at best only semirelevant: for example, the digression on the Polish minority (p. 122) and the statistics on farm production, which tell us nothing about hereditary farms since the latter’s output was never separately recorded. Equally, the passage on the Wehrbauer concept (p. 121) is potentially misleading as a result of overcompression. Despite the occasional scrap-

piness in presentation, however, this monograph can be recommended as a useful addition to studies of the agrarian history of the Third Reich.

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BENJAMIN B. FERENCZ. *Less Than Slaves: Jewish Forced Labor and the Quest for Compensation*. Cambridge: Harvard University Press. 1979. Pp. xxii, 249. \$15.00.

During World War II Nazi Germany coerced millions of persons to work in the factories and farms of the Third Reich. Most of these workers were foreigners ruthlessly recruited throughout Europe, many were POWs, and some were concentration camp inmates. Their treatment and employment varied from normal to diabolic depending on the racial judgment of the Nazis, the changing fortunes of war, and the whims of accidental circumstances. Still, none suffered more than the Jewish concentration camp inmates. Their fate was clear; for these unfortunates, whom a survivor called "less than slaves," labor and death were almost synonymous. Years after the war, some of the surviving victims tried to win retroactive payment from the prominent German corporations that had so shamelessly exploited them. Benjamin B. Ferencz, a former American prosecutor at the Nuremberg trials and later a leader in the Jewish restitution efforts, describes the legal actions, negotiations, and political pressures that finally compelled several companies grudgingly to make a few meager payments. It is not a very pretty tale, for none of the companies ever acknowledged any legal or moral responsibility for their wartime actions. They claimed they had to accept labor from the government regardless of its origins—a claim that was patently untrue, as the author indicates.

It is not particularly suprising that German corporations were fearful to make payments to their forced workers lest it be taken as a confession of guilt, nor is it surprising that the few firms that made payments did so to improve their corporate image and enhance their prospects for profits. Corporate morality was never very strong in Germany, or for that matter anywhere else. I. G. Farben and the electrical concerns of AEG and Siemens were worried about their sales abroad, Krupp was eager to sidestep the Allied order to sell off his Rheinhausen coal and steel operations, while Rheinmetall's payments came as a result of political pressure from America. They all stalled and tried to minimize the number and amount of the claims, and some, like the Flick concern, never did pay a cent. There was neither shame nor the slightest gesture of atonement. Willy Brandt could kneel before the victims

of Nazism, but not German business. In the end, the five German companies paid 14,878 claimants DM 51,935,095 and closed their books on the matter. By maintaining their innocence, the firms possibly sought to avert further claims by the millions of foreign workers used so cheaply during the war, or perhaps they were continuing the "conspiracy of silence."

This book reveals the callous attitude of German business that supported Hitler and profited thereby, but it does not try to explain the great problems involved. It is essentially the interestingly told narration of the legal attempts by the survivors to achieve compensation. There is no attempt to analyze the relationship of business to Nazism or even the morality and politics of some of the issues that are involved. The historian is not going to find much of use or new here, which is a pity because the author's training and experience obviously gave him the opportunities to study and understand the intricate nature of German business and society that few historians have ever had. Unfortunately Ferencz is too much the advocate and not enough the judge.

Most of the documentation for the book comes from the Nuremberg trials and subsequent legal proceedings, the correspondence between Jewish groups and German corporations, and the news media. The author has marshaled his facts and presented his argument well, but then only a few ever doubted the merits and justice of his case.

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HANS W. GATZKE. *Germany and the United States: A "Special Relationship"?* (American Foreign Policy Library.) Cambridge: Harvard University Press. 1980. Pp. xvi, 314. \$17.50.

The American Foreign Policy Library is designed to provide the interested public with an introduction to the modern history of foreign countries with particular reference to their relationship with the United States. The volume under review is to do this for German-American relations from the beginning of American independence into the mid-1970s. A general description of Germany's past and present is followed by a chapter on German-American relations from 1776 to 1914. The dramatic turn of World War I is appropriately given a chapter to itself, as is the Weimar period, the Hitler years from 1933 to 1939, and the period of World War II. Almost half the book covers the years since 1945 with emphasis on the development of the German Federal Republic, its institutions and history, and its relationship to the United States as well as to

France and Eastern Europe. Some attention is also given to the German Democratic Republic, though in view of the recentness and minimal character of its relations with the United States, the space devoted to it is small. A concluding chapter suggests what has been hinted at repeatedly in the text: life in the Federal Republic of Germany is becoming more Americanized, but there is little influence in the other direction, and the relationship of the two countries, while close and important, has not attained the special character sometimes attributed to Anglo-American ties.

The text is based on broad familiarity with the published literature and presents Hans W. Gatzke's generally balanced views of major issues with no effort at either presenting new findings or arguing controversial questions in detail. Most of the author's judgments—the book is a rather personal one—will seem reasonable to informed readers. If one leaves aside a few minor factual mistakes, only the discussion of the 1920s is not in accord with recent scholarship on reparations and the differential impact of World War I on the economies of Germany and its enemies. For a work on U.S.-German relations, it is unfortunate that the author almost totally misconstrues Hitler's policy toward the United States. Gatzke has failed to assimilate into his account two important policy considerations: (1) Hitler's repeatedly expressed expectation that crushing Russia would free Japan to strike in East Asia and keep the United States tied up there; and (2) Hitler's repeated urging of Japan to attack Britain, accompanied by assurances of support against the United States when necessary. The relatively detailed coverage of the federal republic's new policy toward Eastern Europe would have been more readily understood if the impact on Soviet policy of the developing Sino-Soviet split had been integrated into the narrative.

Scholars will find the book interesting reading but will be left a bit puzzled as to its purpose. The section of suggested readings will be of help to undergraduate students of German history.

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ALAN SKED. *The Survival of the Habsburg Empire: Radetzky, the Imperial Army and the Class War, 1848*. New York: Longman. 1979. Pp. xii, 289. \$33.00.

The subtitle of this study should be the main title, for the survival of the empire owed as much to Windischgrätz, Schwarzenberg, and Nicholas I as to Radetzky. Early chapters deal with the organization and life of the army between 1815 and 1848. Here the author concludes that the Habsburgs had

a tradition of moving all regiments around the monarchy, but he does not accept a deliberately worked out policy of divide and rule. "Those who believe otherwise would have to show why so many troops were at the wrong place at the wrong time [1848] and why many of them had been there so long" (p. 51).

Radetzky emerges as a proconsul who was shocked by the rapidity of the expulsion of his troops from Milan. His shame intensified his determination to reoccupy the city and to punish the aristocrats whom he blamed for the treason. Always short of troops, he was lenient with Italian deserters and sufficiently impartial to hold on to his Hungarian, Croat, and Grenzer regiments. All too complacent about the warnings sent him from Venice by Count Ferdinand Zichy, he was enough of a propagandist to escape the disgrace visited upon that military commandant. He also won at Custoza and Novara, thanks to the inept Sardinians. Believing that the Italians hated the monarchy, he was unyielding in his opposition to reconciliation after retaking Milan.

At this point Alan Sked develops his chief thesis with a section entitled "Radetzky as communist: The social psychology of the revolutionary crisis." Reviewing the jacquerie of 1846 in Galicia, he decides upon the basis of documents in the archive of the military presidium of the Galician civil and military gubernium for 1845-46 that Metternich and his administrators did not provoke the attack on the rebellious landowners there. He notes, however, that Metternich was quick to impart the appropriate lesson to Radetzky and other commanders in areas where aristocrats might be tempted to conspire. Arguing that for most people even in 1848 "the adjective 'communist' might be applied to any movement which threatened to undermine the bastions of political or social privilege" (p. 164), the author interprets Radetzky's policies of confiscation, sequestration, and extraordinary levies as a class war upon the great property owners. In the end he agreed to the nonenforcement of the toughest of his measures and in Sked's view failed to win over the peasants.

This monograph reveals a thorough combing of archival and secondary material and is honest about what cannot be documented. When it moves beyond 1849, some conclusions are suspect. No one today should accept at face value James Hudson's premature appraisal of the Milanese rising of February 6, 1853. It was abortive because the lower classes did not rally to their "leaders." Only a careful study of what used to be in the Milanese archives might have shown whether "rich and poor alike detested the Austrians" (p. 205) by 1853. The reviewer's survey of the opinions of the provincial delegates in Venetia from 1861 to 1866 shows that

these Austrian appointees regularly pointed to the inherent disloyalty of the middle class and of much of the clergy and nobility. They rarely saw anything but political passivity among the peasants and usually reported uncontested collections of taxes and conscripts. Peasants undoubtedly dislike all such authority. They did not vote for Austria in 1866, whether through apathy or conviction.

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LAJOS KERÉKES. *Von St. Germain bis Genf: Österreich und seine Nachbarn, 1918–1922*. Translated from Hungarian by JOHANNA TILL. Vienna: Hermann Böhlau Nachf. 1979. Pp. 415. DM 92.

Von St. Germain bis Genf is a detailed diplomatic history of Austria and its neighbors from the fall of the Habsburg Monarchy to the Geneva Protocols of October 1922. Lajos Kerekes maintains that by the latter date Austria's diplomatic relations were reasonably well consolidated. The author, who has written numerous earlier studies on Austrian history including *Abenddämmerung einer Demokratie: Mussolini, Gömbös und die Heimwehr*, has used Austrian, German, and Hungarian archives as well as most, although by no means all, of the relevant secondary literature. Important omissions are Alfred Low's *The Anschluss Movement, 1918–1919*, and the *Paris Peace Conference*, C. A. Macartney and A. W. Palmer's *Independent Eastern Europe*, and Dagmar Perman's *The Shaping of the Czechoslovak State*.

Despite its 385 pages of small print, *Von St. Germain bis Genf* reaches few if any startlingly new conclusions. The driving force behind the Austrian Anschluss movement in the early postwar months was Foreign Minister Otto Bauer and the Austrian (but not German) Social Democratic Party. Bauer at first believed the Anschluss ought to be brought about by a fait accompli before the signing of the peace treaties. But by July 1919, difficulty in obtaining disputed borderlands convinced him to wait until after the signing of the Treaty of St. Germain before pushing the Anschluss issue. While the Austrian Socialists were hoping to join hands with their German brethren, many conservatives in the provinces favored the formation of a south German state. Britain and Italy, however, opposed a south German state, fearing it would fall under French domination.

Kerekes adds weight to the long-standing suspicion that in 1921 some influential circles in France supported a Habsburg restoration in Hungary and eventually Austria as a way of halting the Anschluss movement. Only after the unsuccessful attempts by Karl von Habsburg to regain his throne did the French government definitely side with the newly formed Little Entente.

The author also agrees with the recent contentions of Stefan Malfèr in *Wien und Rom* that Austria in effect renounced Sopron by signing the Venice Protocol in October 1921. The later disputed plebiscite, he feels, was essentially fair.

Although the author is a Hungarian, the text is generally free from obvious national bias. There is, however, an occasional bow to Marxist doctrine and jargon. The independence of Czechoslovakia was "determined." The Habsburg nationalities were "oppressed," and Danubian capitalists were "monopolists." Béla Kun's Bolshevik government is hardly mentioned except to say that it fought a "heroic defensive battle" against Hungary's neighbors. In several places Kerekes mentions the anti-Bolshevik motivations of Eduard Beneš and the Entente leaders at Versailles; but here he is only confirming the findings of Arno J. Mayer in *Politics and the Diplomacy of Peacemaking*.

For a book devoted primarily to diplomacy and boundary changes, it is astonishing that there is not a single map. Nor are there any illustrations. With few exceptions, the individuals mentioned in the text remain faceless names. The index (of names only), however, does provide a brief identification of each entry.

Von St. Germain bis Genf is far too long and detailed for the general public; its lack of originality will discourage all but the most dedicated scholars working in the field.

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CHARLES TRINKAUS. *The Poet as Philosopher: Petrarch and the Formation of Renaissance Consciousness*. New Haven: Yale University Press. 1979. Pp. xi, 147. \$14.00.

"In the final analysis he was a poet." Thus Charles Trinkaus characterizes Petrarch the theologian in his *In Our Image and Likeness* ([1970] vol. 1, p. 50). The same assessment informs his new book on Petrarch. In *The Poet as Philosopher* we again meet a Petrarch who was selective, inconsistent, idiosyncratic in his use of his sources and whose choices were dictated by his personal needs, his esthetic temperament, and his attraction to classical and Christian paradigms that he could re-express in compelling images. Trinkaus's book has far more coherence than most collections of essays; each of the five papers extends the author's previous analysis of Petrarch in some way.

The thematic link is the idea of ambivalence. This theme not only underlies Petrarch's moral and emotional approach to philosophy, but it also, for Trinkaus, is his chief bequest to the Renaissance and modern consciousness. It is debatable if ambivalence in this sense is unique to, or exhaustive of,

the humanist movement. The same attitudes, derived from the same sources, are also found in twelfth-century writers. Further, whatever marginal utility there once may have been in equating the Renaissance with the modern mind has long since evanesced. It may also be objected that, despite the book's title, what Trinkaus actually demonstrates is the primacy of rhetoric in Petrarch's handling of philosophy. Still, these cavils aside, this book is an important contribution to our understanding of Petrarch. The author wears his massive erudition with ease and grace, citing previous scholarship sparingly and more to give credit for benefits received than to polemicize. His goal, which he achieves with great success, is to give Petrarch a careful and sensitive reading, sharing the appreciation and insight he has gained in a long and reflective acquaintance with his subject.

The centerpiece of the book and the essay that displays Trinkaus's discernment at its best is "Petrarch and the Tradition of a Double Consciousness." With impressive sureness of touch the author shows how Petrarch's awareness of the divided self derives from both philosophy, especially Roman Stoicism, and Pauline and Augustinian theology, as Petrarch appropriated them. Trinkaus is entirely correct in noting that Petrarch's norm in choosing his sources was their psychological resonance; this also explains the appeal of figures like Cicero and Augustine as psychic alter egos in preference to other authorities who drew on the same sources but whose existential situation had no parallels with Petrarch's.

The motif of double consciousness informs the range of tensions that Petrarch expressed—free will and grace, the active and the contemplative life, the subjective and the objective—and that Trinkaus explores in the other essays. "Petrarch and Classical Philosophy" treats the conflict between Petrarch's objective, historical attitude toward antiquity and his subjective preferences. "Petrarch's Critique of Self and Society" extends the theme of the divided self into the debate between the active and contemplative life and the role of free choice and fortune in the working out of vocational options. "*Theologia Poetica* and *Theologia Rhetorica* in Petrarch's *Invectives*" deals with the tension between objectivity and subjectivity in theology, as Petrarch confronted the need to balance his own religious interests with the didactic imperatives of prophetic eloquence.

The final essay, "Estrangement and Personal Autonomy in Petrarch's *Remedies* and Boccaccio's *Decameron*" treats the theme of autarchy as a remedy for alienation. The comparison with Boccaccio is instructive for two reasons. First, it enables the author to shed light on the degree to which the Petrarchan problematic is a function of Petrarch's own personality and situation and the degree to which it was

absorbed and transmitted by a disciple known for his pre-eminently tranquil and sunny disposition. Second, and this is perhaps an unintentional lesson on the author's part, it raises once again the question of whether it is the poetic calling itself that is the critical factor in Petrarch's perception and use of philosophy. For Boccaccio, and other authors of the time, could handle ambiguity, paradox, and loose ends without the mental discomfort of Petrarch.

If this book leaves some questions open, that is to be expected in a volume of such small size that tackles issues of such major importance in Renaissance thought. Both for the continuing inquiry that these essays will stimulate and for the depth and freshness of his portrayal of Petrarch, Trinkaus leaves the reader very much in his debt.

MARCIA L. COLISH
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ENRICO STUMPO. *Finanza e stato moderno nel Piemonte del Seicento*. (Studi di Storia Moderna e Contemporanea, number 6.) Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1979. Pp. xxi, 470. L. 10,000.

The history of public finance in Italy has a long and distinguished tradition that links older institutional histories with the new economic history. Its focus on budgetary income and expense highlights not only where revenue came from and went but also the mechanics of government—bureaucratization, rationalization, and the ties between public and private, investment and credit. Enrico Stumpo's examination of seventeenth-century finances in Piedmont follows the standard model: chapter 1, budgets as sources, ordinary income, and local finances; chapter 2, extraordinary income and war finances; chapter 3, state operations and their cost; chapter 4, the problem of extraordinary finance (the venality of offices and the "cursus honorum" of the bourgeoisie); chapter 5, economy, society, and state in the seventeenth-century crisis; and chapter 6, public debt and the success of financial policy. The conceptual framework is conservative, but the potential dividends are great.

Building upon the magisterial work of Luigi Einaudi and Giuseppe Prato before, as well as that of Attilio Garino Canina immediately after, World War I, Stumpo adds some new archival documentation for the seventeenth century. Essentially, however, he is reworking all the old data in terms of the contemporary bibliography on the seventeenth century and more modern social science methodology. Stumpo would like to design a unique Piedmontese *histoire totale* from the budgetary raw material but finds himself, more often than not, unable to an-

swer the questions his analysis raises or reaffirming the accepted wisdom on early modern Europe. The absence of detailed research on prices, wages, and the cost of living in Piedmont by other scholars, for example, prevents firm conclusions on the effect of money devaluation. On the other hand, Stumpo's description of the character and aspirations of the bourgeoisie corresponds neatly to the pattern of development found in France. Do Italian public finances, subspecies Savoyard, have something distinctive to attract our attention?

Stumpo's conclusion addresses this concern. He applies to Piedmont an aphorism taken from Fernand Braudel: "the modern state is the greatest entrepreneur of the century" (p. 356). Piedmont, a small agrarian state in the orbit of France, differed radically from its Italian neighbors, Milan, Genoa, and Venice, in its rapport between city and countryside. Because it remained feudal and did not urbanize in the fourteenth and fifteenth centuries, this "retardation" was compensated for in the sixteenth and seventeenth centuries by a transformation of the feudal state into a "modern state." Although it shared a distinctive fiscal structure (a *camara dei conti*) with two other Italian states of "feudal formation" (the Papal States and the Kingdom of Naples), Savoy stood out as the "unique modern state of the absolute type in seventeenth- and eighteenth-century Italy." Although this analysis is undoubtedly true in gross terms, Stumpo's method of deductive reasoning does little more than confirm the authority of Braudel, Cipolla, and others. History argued from general principles to the specific case becomes much more of a mechanical identifying, measuring, and comparing of typologies than a working out of the process of change.

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XENIO TOSCANI. *Il clero lombardo dall'Ancien Regime alla Restaurazione*. (Religione e Società, number 8.) Bologna: Società Editrice il Mulino. 1979. Pp. 437. L. 12,000.

Inspired by the example of Jean Delumeau and others, a loosely organized study of the socioreligious conditions in postmedieval Europe has been going on for some years. Its purpose is to test whether a so-called de-Christianization had actually taken place; its methodology is largely quantitative, but its real goal is to produce a study in the collective perception of values, something akin to what the *Annales* school comprehends by *mentalité*. One of the principal constituents of this investigation is the historical study of the clergy and the problem of clerical vocations. The interest in such

studies first arose around 1870 and coincided with the crises in the Catholic churches of Italy and France. It later spread to other lands, including Protestant areas, and reached its climax with Fernand Boulard's *Essor ou déclin du clergé français?* in 1950. Only within the last few years has this genre of studies fallen into the larger contours of the problem of de-Christianization, in the belief that priestly vocations could be a valid criterion of the religious vitality of a diocese.

The present work fits within this general pattern. Some studies had previously been done for various Italian regions, such as Piedmont, Venetia, and Campania, but none until now for Lombardy. Lombardy is perhaps best known in recent church history for the reforming work of Carlo Borromeo. Since his day, however, the territory had been divided between the Austrian Empire and the Republic of Venice. Toscani attempts to establish, for the period between 1750 and 1830, a data base for ordinations, taking into account the number of those who entered and left the seminaries in each diocese, the social and geographic provenance of those ordained, and the ratio of ordinations in relation to the total population. The results are placed against the type of landholding, the industrial development, and the demographic trends in the area. He does this for the seminaries in each of the nine dioceses. It forms the heart of his work. The merit of this part of the book is that, unlike so many other scholars using quantitative tools, Toscani does not intimidate or drown his readers with a surfeit of highly sophisticated statistical tables and diagrams.

The author's conclusions are most interesting. He finds a fundamental dichotomy between the conditions in the three dioceses of "Venetian" Lombardy and those under Austrian rule during most of the second half of the eighteenth century. Those in Venetia showed an altogether greater stability: the proportion of priests remained higher and the decline in ordinations was smaller than in the Austrian part. Toscani attributes this difference to the rigorous ecclesiastical reforms of Maria Theresa and her son Joseph II. From 1767 there was a regular Austrian legislative policy to reduce the number and wealth of the clergy, which they justified by the Canons of Trent that recommended that bishops only ordain as many priests as were needed in a diocese for pastoral care. The high point of this policy came with the establishment of a centralized *Seminario Generale* for Austrian Lombardy in Pavia. Although the Austrians reversed themselves at the death of Joseph II, it was only with the French occupation in 1797 that both parts of Lombardy came to share a common, if not very prosperous, religious fate. Matters improved following Napoleon's concordat in 1803. With the return of the Austrians in 1814 the picture changed again. Finding a scar-

city of priests, they encouraged young men to take up clerical careers and even introduced clergy from impoverished dioceses outside Lombardy, who were to form a kind of clerical proletariat. Yet, perhaps, Toscani's most surprising finding is that cities provided more priests than the countryside, an apparently general European phenomenon, that confutes the widespread belief that, at the end of the eighteenth century, cities were less favorable to religious life than rural areas.

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ILARIA PORCIANI, *L' "Archivio storico italiano": Organizzazione della ricerca ed egemonia moderata nel Risorgimento*. (Biblioteca di Storia Toscana Moderna e Contemporanea, Studi e Documenti, number 20.) Florence: Leo S. Olschki, Editore. 1979. Pp. vi, 302.

In the past century, the process of welding together disparate elements in an Italy divided by political boundaries, trade barriers, and age-old rivalries enrolled the efforts and dedication of many. Not the least was the contribution made by the *Archivio storico italiano* in developing and furthering intellectual collegiality among Italian historians. One of the many journals launched by the Swiss editor, Gian Pietro Viesseux, working in Florence, the *ASI* began publication in 1842. Its first series, totaling sixteen volumes (some in two parts), was published from 1842 to 1851 and concentrated on making available to scholars documents long out of print or never before published, an innocuous and apparently non-political scholarly endeavor. An appendix, which became longer and longer, however, reported historical activities in other Italian states, reviewed books on Italian history, and occasionally included articles. Drawing financial support from a group of enlightened Florentine liberals, led by Gino Capponi, the *ASI* survived government suspicions and post-1850 repressions by emphasizing its purely scholarly interests. It encouraged research, increasingly reviewed works on Italian history, and gained the collaboration of historians from Turin to Palermo and expatriate scholars, such as Michele Amari, then living in Paris.

In 1855, Viesseux initiated the *ASI*'s second series, making some changes in its format. He informed his readers that, although the journal would continue to reproduce rare documents on the history of Italy, it would give equal weight to publishing articles based on original research and would include extensive bibliographical reviews on current scholarship on Italian history. With unification and the growing Piedmontese preponderance in all of Italy's activities, the importance of the Florence-based *ASI*

declined. Moreover, after 1861 intellectual initiative "passed to the state and took the form of institutionalized research and teaching" (p. 230). The *ASI* became more Tuscan oriented and less national in its coverage. But in the two decades so crucial to the unification movement it had made an important contribution by providing a rallying focus for historians throughout Italy, encouraging discussion among them, and helping to lay the basis for a national historical awareness.

The importance of these pioneer endeavors by the *ASI*—still active today—is well brought out in this excellent study. Begun in 1973 as a doctoral dissertation and completed in 1977 under Ernesto Ragionieri at the University of Florence, the book rests on extensive research in Florence's many libraries and archives and elsewhere. It represents an important contribution to the history of Italian historiography and of the intellectual matrix of the Risorgimento. Impeccably documented and exceptionally well written, it is a pleasure to read and should be consulted by anyone interested in the Risorgimento.

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GIANFAUSTO ROSOLI, editor. *Un secolo di emigrazione Italiana, 1876-1976*. Rome: Centro Studi Emigrazione. 1979. Pp. 386. \$14.00.

The Italian emigration of the last hundred years is one of the important phenomena of modern times. Between 1876 and 1976, some 25,800,000 Italians left their nation, with 50 percent of the migration having already occurred before the beginning of World War I. In 1913, expatriates numbered 872,598 (the zenith); as late as 1961, the number reached 387,123. Many Italians returned home, but for most of the emigrants expatriation was permanent.

So vast a migration was born of a complex constellation of factors, both social and economic, and was not unrelated to the inadequacies of an Italian elitist government that failed, from the outset, to understand the nature of the migration (for example, the economic "dualism" that has been characteristic of Italy since its political unification and the "southern question" that sought its own solution in migration). If Italian migration critically affected the fortunes of the mother nation, it vitally influenced the destinies of the receiving countries. It may be cogently argued that the histories of many nations to which Italians emigrated were (and continue to be) significantly influenced by the Italian immigrant presence (Argentina is the best example, but one could not exclude the United States, Brazil, Canada, and, in Europe, France and Germany).

The influence is worldwide; the Italian presence has been felt in Australia, Peru, Uruguay, Algeria, Egypt, the Union of South Africa, Libya, Eritrea, Ethiopia, Somaliland, and the Belgian Congo. How vast and internationally multifarious the Italian emigration was can be best assessed in the over 300 fascicles of some 36,000 pages that comprise the *Bollettino dell'Emigrazione* (1902–27), which officially chronicled its history and for which a thematic index has at last been published by Rowman and Littlefield.

Against this background, the publication of the Centro Studi Emigrazione's *Un Secolo di Emigrazione Italiana* is an auspicious event. Each of its essays is written by a specialist, and Gianfausto Rosoli has masterfully edited the materials. He successfully produces the best overview of Italian emigration now available, comparable only to the classic work of Robert F. Foerster, *The Italian Emigration of Our Times* (1919), to which Rosoli's compendium may be judged an indispensable sequel.

Rosoli's colleagues have produced monograph-quality essays, each significant. Luigi Favero and Graziano Tassello provide an overview of the history of Italian emigration (enriched by invaluable tables and graphs); Francesco Balletta studies the twin cycles of economics and immigrant remittances; Eugenia Malfatti clarifies the complexities of the "southern question" and emigration; Francesco Cerase harnesses the elusive dynamics of a "precarious economy" that explain migration; Antonio Golini interrelates the processes of internal migrations, population distribution, and urbanization in Italy; A. M. Birindelli, G. Gesano, and E. Sonnino make intelligible depopulation in the contexts of migratory and demographic change; G. B. Sacchetti trenchantly reviews the disastrous consequences of inadequate migration policies; and Rosoli and M. R. Ostuni conclude the volume with an invaluable annotated bibliography of statistical sources on Italian migration.

Rosoli's compendium will stimulate new attentions to the worldwide Italian presence. In the United States, in a period of heightened interest in ethnic historiography, it provides a critical informational centerpiece for one of America's largest ethnic groups.

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BRENDA L. MARDER. *Stewards of the Land: The American Farm School and Modern Greece*. (East European Monographs, number 59.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1979. Pp. xi, 234. \$14.50.

Brenda L. Marder's study is an interesting account of the American Farm School, situated near Thes-

saloniki, from 1902 to 1949. The founder of the school, John Henry House, an indefatigable worker and an indomitable spirit, arrived in the Balkans in 1872. Following his early experiences as a missionary, he developed an educational philosophy predicated on a work ethic conducive to manual labor and social service—an education "which should train the children along things of the heart, the head, and the hands, that is the whole man" (p. 22). It was this philosophy he put into practice in the American Farm School, founded thirty-two years after his arrival in the Balkans. Under his guidance and that of his successors, the school grew into a respectable institution that worked effectively within the agricultural educational structure of the Greek state following the school's hellenization after the Balkan Wars.

Marder's account is informative and well written, but it suffers from serious weaknesses. "The Farm School," she writes, "is much the tale of four gigantic personalities: the founder, John Henry House, his wife, Susan Adeline House, their son Charles Lucius House (director, 1929–1955) and his wife, Ann Kellogg House" (p. ix). This "heroic" approach in the study of an educational institution is a major flaw. We have before us sympathetic biographical sketches of those who, through their courage, determination, dedication, and humanness, managed to sustain the school through an age of adversity, confronted with difficulties that would have crushed ordinary men. Impressed by the qualities of her protagonists, the author has succumbed under the weight of her own perception of her subject matter. We are, indeed, confronted with human beings who loom larger than life. The institution itself, whose growth and social interaction should have constituted the essence of this study, is, in effect, pushed into the background.

The success or failure of a biography of an educational institution depends on the author's capacity to capture the dialectics of the institution's social essence, that is, its interaction with its social milieu. Since Greece was an agrarian society with its economic well-being depending upon the organization and exploitation of its agricultural resources, the contribution of an agricultural institution to that society should constitute the essence of the study. Unfortunately, this cannot be said for this work. Occasional references to its successes do not help us understand the school's function in the process of Greece's agricultural modernization. We do not even have a complete statistical account of the school's student population throughout the period under study, nor do we have a satisfactory picture of its operations and curriculum. In short, we are not able to assess the contributions of the school, however valuable they might have been. It must be mentioned finally that in her effort to integrate the

school's development into the broader context of Balkan and Greek history, Marder accepts uncritically positions that are either highly controversial or unacceptable. Be that as it may, this story deserves to be told, but it could have been told more effectively.

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CONSTANTIN N. VELICHI. *La Roumanie et le mouvement révolutionnaire bulgare de libération nationale (1850-1878)*. (Bibliotheca Historica Romaniae, number 60.) Bucharest: Editura Academiei Republicii Socialiste România. 1979. Pp. 231. 11 L.

The history of Rumanian-Bulgarian relations has had a long and sometimes heated historiography; after a friendly nineteenth-century relationship, the historians joined the politicians in attacking each other, using the past as a tool in a political and diplomatic struggle based mainly on territorial claims. Since World War II, with no more real problems to fight over, the Rumanians and the Bulgarians have started again to look more objectively to the many historical problems the two countries have had in common; a joint historical commission is trying to establish projects to be worked on by joint teams; the Academies of Sciences of the two countries have published the first volume of a series dedicated to the long history of Rumanian-Bulgarian relations. Constantin N. Velichi's new book should be seen in this broader context as an attempt to interpret these relations in a scientific and objective way.

Velichi is probably the best scholar on the subject; he has written extensively in Rumanian as well as in Bulgarian on a variety of subjects such as the Bulgarian emigration north of the Danube in 1806-12 and 1828-34, on the Brăila uprising of 1841, on the activities of the Bulgarian diaspora living in Rumania, and on its role within the movement of national revival.

His latest book covers the last thirty years of Ottoman domination over the two peoples, one constantly enlarging its autonomy up to the recovery in 1877 of its total independence, the other struggling to create a political movement in exile, which led in 1878 to the rebirth of an autonomous Bulgarian principality.

The book follows in general the course of the political events; the reaction to the Crimean War, Rakovski's activity in the Principalities before and after their unification, the ups and downs of the different societies and revolutionary committees set up in order to give a political structure to the national movement, and the history of the many *tchétas* crossing the Danube with the hope of provoking a general anti-Ottoman uprising. Great at-

tention is also paid to Karalevov's and Botev's activity, as well as to the participation of the Bulgarian volunteers in the war of 1877-78.

The information is overwhelming, the interpretation correct. A regret has nevertheless to be expressed; the book focuses too much on political history, too little on cultural relations, and not at all on the economic and social problems of the Bulgarian community living in Rumania. It would be to the great benefit of the history of Rumanian-Bulgarian relations if Velichi were to return someday to these topics and complete a study very few others would be able to deal with so thoroughly.

VLAD GEORGESCU
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ELKA BORNEMANN. *Der Frieden von Bukarest 1918*. (Europäische Hochschulschriften, series 3; Geschichte und ihre Hilfswissenschaften, number 64.) Bern: Peter Lang. 1978. Pp. 241.

A proper understanding of Rumania's separate peace with the Central Powers (March-May 1918) in its own right has been impeded by relegating it to the status of an adjunct of the Treaty of Brest-Litovsk, an example of Germany's *Griff nach der Weltmacht* (Fritz Fischer), or a chapter in the struggle in Germany between *The Sword and the Scepter* (Gerhard Ritter). We welcome therefore this Bonn dissertation that gives us the first detailed study of the treaty's provisions and the negotiations behind them.

This study is not without limitations, the most important of which is its decidedly German perspective, which slights the Austro-Hungarian point of view and virtually ignores the Rumanian. It could be more accurately entitled "Germany and the Peace of Bucharest." This problem stems from a reliance on German-language sources, necessitated, in part, by Elka Bornemann's lack of success in gaining access to important Rumanian archival materials. Less excusable is the author's failure to utilize other, readily available Rumanian primary and secondary sources, for example, Alexandru Marghiloman's *Note Politice, 1897-1924* (1927).

But this shortcoming can easily be forgiven in light of Bornemann's thorough exploitation of archives in Bonn, Koblenz, Freiburg, Munich, Potsdam, Merseburg, and Vienna to give us a detailed exposition of the formulation of the German demands and the debate both at the conference and within German circles that led to the final settlement. The latter is a complex and interesting story of interplay and often bitter conflict involving the foreign office (Kühlmann), the economic authorities (Helfferich and Koerber), the high command (Ludendorff), the German army in Rumania (Macken-

sen), the occupation administration (Hentsch), special interests (German bankers and industrialists), not to mention the input of Austria-Hungary, Bulgaria, and Turkey, Germany's allies.

What emerged was a settlement dominated by its economic clauses that obligated Rumania to mortgage its rich natural resources and tie its economy to Germany for the future. While not denying the harshness of these terms, Bornemann points out some facts often overlooked by the critics of German policy. First, the German demands were closely tied to the acute economic crisis caused by the Allied blockade and its anticipated postwar counterpart, a trade war against Germany, led by England. Germany's was not a striving after "a significant increase of power," but "a striving after security," that is, "a guarantee for an acceptable continuation of the economic and social structure of the German Empire."

Secondly, Bornemann argues that the Peace of Bucharest was a "negotiated peace" and gives evidence to show that the Rumanians were not passive recipients of dictation but that they fought hard, with considerable skill and success, to modify some of the more exorbitant demands of the Central Powers. The German record at Bucharest must be considered, she argues, in the context of the Entente's promise of immense annexations to Rumania in 1916 and of the terms dictated to Germany at Versailles in 1919. On a minor but often forgotten point, Bornemann reminds us that Germany worked for the insertion into the treaty of a strong guarantee of Jewish rights, which traditionally had been abridged in Rumania.

Bornemann's *relatively* favorable conclusions regarding the Peace of Bucharest will not find favor among Rumanian, East German, or Western historians who follow Fritz Fischer's interpretation of German war aims. On the other hand, her solid research and exposition cannot be ignored and will be the starting point for any future study and discussion of the subject.

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DOMOKOS KOSÁRY. *Napoleon et la Hongrie*. (Studia Historica Academiae Scientiarum Hungaricae, number 130.) Budapest: Akadémiai Kiadó. 1979. Pp. 122. \$10.00.

A large part of *Napoleon et la Hongrie* concentrates on those Hungarian "echoes" of Napoleon's designs, which Domokos Kosáry expounds with his customary, if controversial, insight. Kosáry's innovative interpretations have provoked a string of challenges and rebuttals, stirring lively debate. Here, Kosáry replies to these challenges, reaffirming the correct-

ness of his representation. In short, then, this little book has had reverberations in Hungary of a significance belied by its size.

Kosáry takes issue with the views on the Hungarian political scene at the end of the eighteenth and the beginning of the nineteenth century as construed by the controversial but interesting Stalinist historian, József Révai, whose interpretations survived the downfall of Stalinism in Hungary. As early as 1938 Révai had asserted that three major political trends characterized the period. The first was the Jacobin line of the Ignác Martinovics conspirators. The second was that of Gergely Berzevichy, who was ready to set aside strident nationalist views, cooperate with the dynasty, and pioneer economic advancement in order to achieve the transformation of Hungary's feudal society into a bourgeois one. The third was the group around Ferenc Kazinczy, who envisaged the rebirth of Hungary through a linguistic and literary renaissance. The third trend was the successful one. "The Hungarian response to the French Revolution," Révai claimed, "was the renaissance of language and the new literature."

Kosáry challenges Révai's assessment, proposing instead that only two major trends dominated the Hungarian political scene at the turn of the century. The predominant tendency, which had the support of the vast majority of the nobility, the most powerful political element in Hungary at that time, was the feudal nationalism of this estate. The secondary line, supported by a minority of the politically enfranchised, was that of the enlightened reformers who sought the bourgeois transformation of society. Theirs was by nature an antifeudal movement (pp. 90-96).

This was the sociopolitical background of Napoleon's designs for Hungary, as Kosáry sees it. The French Revolution and the Napoleonic Wars divided Hungary's body politic. There was a strong coalition of the Habsburg dynasty and the Hungarian estates aimed at preserving the monarchy and feudal privileges against the threat of the French Revolution. There also existed among the Hungarian estates an opposition to the Habsburgs that sought to protect feudal privilege, but against the dynasty rather than in cooperation with it. Only a small, unorganized opposition saw in Napoleon and the French an ally against both absolutism and feudalism and for the progressive bourgeois transformation of Hungary's feudal system.

Kosáry's book contains such a wealth of material, both archival and printed, and draws on such a breadth of literature, that it would be hard even to summarize. His little book constitutes a significant essay, informative and rich in ideas and controversy. Written with a masterly hand, it proves that highly scholarly work need not be dull. It offers

excellent reading as much to the expert as the literate general reader.

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THOMAS L. SAKMYSTER. *Hungary, the Great Powers, and the Danubian Crisis, 1936-1939*. Athens: University of Georgia Press. 1980. Pp. xi, 284. \$20.00.

Was Hungary the "beast of prey" evoked by Churchill that exploited the Danubian crisis to complete the destruction of Czechoslovakia? Thomas L. Sakmyster's study confronts this judgment—one shared by many historians—and exposes it as a simplification and a distortion of the complex politics of interwar Central Europe.

The territorial and ethnic truncation of Hungary, foreshadowed by Entente diplomacy in the course of World War I and confirmed in the Treaty of Trianon of 1920, was a punishment far out of proportion to Hungary's role in the war. It also made a mockery of the Wilsonian principles of self-determination by incorporating into the successor states, Czechoslovakia, Rumania, and Yugoslavia, over three million ethnic Hungarians, some living in solid blocks contiguous to the new frontiers. The containment of German expansion and the long-suppressed national aspirations of the subject peoples of the Austro-Hungarian Empire were the objective motives in this redrawing of the map, but the political outcome was the division of Europe into status quo and revisionist camps. The festering sense of injustice in the latter camp undermined any hope of enduring stability.

In his introductory chapters, the author provides a sensitive and judicious survey of Hungary's fortunes between 1918 and 1936. The convulsions that shook Hungary at the war's end—the liberal-democratic interlude of Michael Károlyi, the short-lived Soviet republic of Béla Kun, the conservative counter-revolution, and the Trianon dictate—led to a political system possessed of clear and rigid values. In foreign policy, revision of Trianon became the overriding and consensual objective. The conservative, "Christian-national" ideology that predominated in interwar Hungary rested on the pillars of revisionism and anti-Bolshevism and also proved inimical to right-wing radicalism. The author skillfully portrays the principal Hungarian statesmen in their attempts to advance the cause of revision on behalf of a country that was militarily and economically feeble and diplomatically isolated. In particular, the efforts of the foreign minister, Kálmán Kánya, to pursue a "free hand" policy and employ traditional diplomacy in opposition to the adventurism of more fanatical nationalists are

related and analyzed with a wealth of convincing documentation.

With the Anschluss, the German empire reached Hungary's doorstep, but Kánya resisted Hitler's urgings that Hungary participate in the forceful dismemberment of Czechoslovakia and persisted in seeking alternative, notably British, support for peaceful revision. Ultimately, of course, the policy failed, for the Munich agreement was a concession to German power and ignored the historically better-justified Hungarian case for partial revision. The subsequent return of some Hungarian-inhabited areas of Slovakia through Italo-German arbitration was a Pyrrhic victory, for it strengthened Hungary's alignment with the Axis and disillusionment with British prevarication. The relentless momentum of revisionism had driven Hungary's leaders to make a pact with a devil that most of them fundamentally despised. As Thomas Masaryk himself admitted, if he were a Hungarian he too would be a moderate revisionist. The diplomacy of moderate revisionism, pursued fruitlessly by Kánya and others, receives a lucid and scholarly exposition in Sakmyster's superb study.

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ALVIN MARCUS FOUNTAIN II. *Roman Dmowski: Party, Tactics, Ideology, 1895-1907*. (East European Monographs, number 60.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1980. Pp. xiii, 240. \$16.00.

Few figures in Polish history have been more influential and controversial than the nationalist political theorist and leader, Roman Dmowski. His life was marked by ironic paradox. Dmowski was a political failure during his lifetime, but his ideas were embraced and enacted by his bitterest enemies both during his lifetime and continuing to the present. He was identified in most quarters as a dedicated Russophile, but, actually, he despised the Russians, although he saw them as the only feasible check against the Germanic threat that he perceived to be far more dangerous to Poland. Understandably, nearly everything written on Dmowski and his thought is violently polemical, openly for or against him; this dearth of objective material is especially true of English-language works.

Alvin Marcus Fountain has made an impressive start toward filling this gap in scholarship. Drawing on a wealth of primary sources from Polish state as well as private archives, he constructs a three-dimensional portrait of the man, his ideas, and his movement, during the period when all were in their formative stage of development as a force in Polish political life. In the process of revealing how

Dmowski first became actively involved in politics, how his political thought evolved and matured into a coherent doctrine of Polish nationalism, and how he built an organization that could translate theory into action, the author also paints a comprehensive backdrop of the political scene in Poland at the turn of the century. A major plus is the appended "Biographical and Party Register," which provides a brief but informative sketch of each leading political figure and grouping of the time. This feature, together with a highly readable narrative style, solid annotation, and extensive bibliography, renders the book invaluable both as an introduction to this aspect of Polish history and as a reference work for the expert desiring more specialized knowledge.

Despite its obvious merits, the book does have several shortcomings that leave the informed reader somewhat uneasy. One of the most salient components of Dmowski's political ideology was a fiercely chauvinistic and ethnocentrically exclusivist Polish nationalism that translated into rabid anti-Ukrainian and virulent antisemitic doctrines. While the author admits their existence, he never really explains when and why these negative forces became fundamental elements of nationalist thought. Moreover, the artfully balanced, scholarly, objective tone that Fountain maintains throughout the narrative suddenly yields to a highly subjective, positive appraisal in the conclusion. He may be correct in asserting that Dmowski was not a "proto-Fascist," as the term is commonly used, but rather a prime example of a classical nineteenth-century leader of the European right, who altered its precepts slightly to suit the needs of the new century. Nonetheless, the fervor with which Fountain presses these arguments in favor of Dmowski as "a political leader and diplomat of accomplishment and skill, whose efforts are chiefly to thank for Poland's emergence as a viable state" after World War I (p. 163) has the unfortunate effect of converting a serious academic treatment into precisely the type of polemical apologia that the author avowedly set out to avoid. These flaws notwithstanding, however, Fountain has produced a book certain to spark new interest in Roman Dmowski, while serving as an excellent case study of the formation of an organized, articulate nationalist movement in East Central Europe.

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JANUSZ PAJEWSKI. *Odbudowa państwa polskiego, 1914-1918* [Restoration of the Polish State, 1914-18]. Warsaw: Państwowe Wydawnictwo Naukowe. 1978. Pp. 384. 120 Zł.

Prior to the Second World War, Janusz Pajewski distinguished himself in Poland by writing and

publishing specialized studies on Polish-Turkish relations as they existed in the early sixteenth century under the Jagiellonian dynasty. By 1946, when he left his teaching position at the University of Warsaw to take up a position at the University of Poznań, Pajewski had made a dramatic shift in his interests to more recent history. The focus of his research became the study of German imperialism, the genesis of World War I, and the restoration of Poland during the course of that struggle. These topics are not new fields of study, and there have been scores of books about them published in Poland. Perhaps because he came to this period of study late in his career, after devoting his earlier years to what would seem to be an unrelated area of study, his numerous publications have received universal praise for their scholarship and originality. Even the University of Strasbourg has seen fit to award him a doctorate "honoris causa" in 1976 for his contributions.

For over 120 years after the partitions of Poland, Polish history could not be studied or written in the conventional manner of national histories. Prior to World War I and during the course of that struggle, Polish "society" in want of a Polish state carried on an inheritance that had never completely disappeared. Pursuing its traditions under three different political powers, as well as under different economic and social conditions, the degree of polonization in the three societies was far from homogenous. So disparate and so myriad were the views that if it had not been for the one underlying belief of all Poles that Poland should be resurrected, these aspirations would have been self-defeating.

In his opening chapter Pajewski scrupulously guides us through the Polish Question as it existed on the eve of the First World War, discussing the issues not only as the partitioning powers saw them but also through the eyes of the various camps of Polish political parties—none of whom believed in a restoration by revolutionary means. With the onset of the war, Józef Piłsudski and Roman Dmowski, in Austria-Hungary and Russia respectively, became the most ardent spokesmen for polonization. What distinguished Piłsudski from the others was his unswerving belief that Poland's rebirth would have to come by its own efforts and struggle. It is this theme of faith in Poland's own abilities that Pajewski focuses on.

Pajewski is correct when he allows that of all the dilatory proclamations issued during the course of the war with regard to Poland, Austria's seemed to hold the most promise. But always it was too little too late, and invariably the voices of Berlin and Hungary were in opposition to any Austro-Polish scheme. For Berlin, the Poles were to be used and not humored. Russia in turn countered these proposals with eloquent appeals that held little real

promise. Poland's fate became a pawn between these great powers until the Two Emperors' Manifesto was declared on November 5, 1916. With the manifesto, Poland became a question of international importance, and, to further underline this fact, Woodrow Wilson declared in January 1917 that it was the feeling of all that a "united, independent and autonomous Poland" should exist.

In this excellent monograph Pajewski continues to concentrate on the efforts Poles were making on their own behalf. The vicissitudes of the war, the February and October Revolutions in Russia, the Treaty of Brest-Litovsk, and the eventual collapse of the Central Powers are all interpreted from this perspective. The Poland that was restored was a combination of Piłsudski's Jagiellonian concept and the Poland of the Piasts advocated by Dmowski.

Pajewski has accomplished a tour de force. This is a closely reasoned chronological historical narrative, and it is told in a straightforward manner. Almost nothing outside the main theme intrudes. Cosmopolitan zealots like Rosa Luxemburg and Felix Dzerzhinsky or fringe elements led by Roman Skirmunt or Wilhelm Friedman are not even mentioned.

Oh yes, one more thing. It is to be regretted that, contrary to the usual practice in publications of this sort coming out of Poland, there is no English or French summary in this book.

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CLAES PETERSON. *Peter the Great's Administrative and Judicial Reforms: Swedish Antecedents and the Process of Reception*. Translated by MICHAEL F. METCALF. (Skifter Utgivna av Institutet för Rättshistorisk Forskning Grundat av Gustav och Carin Olin, series 1; Rättshistoriskt Bibliotek, number 29.) Stockholm: A.-B. Nordiska Bokhandeln. 1979. Pp. x, 448.

This book is an impressive and important work of scholarship. Its scope is markedly wider than the title indicates: within its own limits it is by far the best study in any Western language of the often chaotic administrative history of the reign of Peter I. Claes Peterson is concerned primarily to determine the extent to which the tsar's efforts at administrative reform—above all the creation of the new administrative colleges in 1718–20 and the abortive remodeling of provincial government in 1719—were an imitation of Swedish models. The core of the book thus consists of a very detailed study of the establishment of the colleges and of the structure and personnel of those three mainly concerned with financial affairs—the *kamer-kollegiia*, the *shats-kontor-kollegiia*, and the *revizion-kollegiia*. This is followed by an equally full discussion of the simul-

taneous effort to overhaul local administration. The other colleges receive much less attention, partly because Peterson found it impossible (notably in the case of the war and admiralty colleges) to gain access to adequate primary materials.

Throughout the book comparisons between these Russian innovations and Swedish precedents are made in great detail; one result of this is that the book tells its readers a great deal about Swedish as well as Russian administrative problems and methods. The case for Russian imitation of Swedish models, often faithful to the point of slavishness, is convincingly made. Frequently Peter's enactments, in structure and even wording, were clearly based on earlier Swedish ones. It will be very hard in the future for Russian historians, as they have sometimes done in the past, to deny the importance of this imitative element in Peter's work or even to argue that these Swedish models were subjected to very much "creative reworking." Peterson's case is made all the more convincing by his unwillingness to press it further than the evidence will go. The means by which Peter obtained information about Swedish institutions and practices is given considerable attention; here the importance of the remarkable Heinrich Fick, who for this purpose spent most of 1716 in Stockholm, is clearly brought out.

The author takes a wide view of his subject. The first part of the book consists of an excellent summary of the entire administrative history of Peter's reign that will be of great use to everyone seriously interested in the subject, while in the chapter on the *iustitz-kollegiia*, for example, there are some extremely interesting remarks on legal procedures, a subject on which little has been written in any Western language. The immense obstacles in the way of real administrative improvement in Russia, notably the crucial shortage of suitably trained men and the difficulty of making the organs of local administration cooperate effectively with central institutions, are particularly well illustrated. Occasionally the density of detail makes for less than easy reading, but Peterson has made an outstanding contribution to our knowledge of Petrine Russia.

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J. MICHAEL HITTLE. *The Service City: State and Townsman in Russia, 1600–1800*. Cambridge: Harvard University Press. 1979. Pp. viii, 297. \$20.00.

It has long been Russia's fate to have its society and institutions perceived, evaluated, and, in most cases, denigrated on the basis of models and expectations derived from Western Europe. What a pity, runs the refrain, that the Russian Church was not more like the Catholic or Anglican or Lutheran Church,

that its landed nobility did not think and act like the nobles of France or England or Germany, that its civil servants failed to meet the standards set by Prussians, that its middle class or bourgeoisie could not play the role that history had written for those ever-rising townsmen of the West. Tsars and their officials deplored the fact that in Russia those groups were so much less useful than their Western analogues, and the Westernizer and liberal opponents of the tsarist regime lamented the inability of those same groups to limit autocracy by securing rights, privileges, and autonomy for themselves. More recently, Soviet historians have been struggling for decades to reconcile Russian history with patterns of development that Marx extracted from the experience of Western Europe.

To his great credit J. Michael Hittle does not write about Russia's towns and townsmen with the tone of a frustrated parent comparing a disappointing younger child with a distinguished older sibling. Although he is fully aware of the importance of Western models in the history and the historiography of Russia's towns and townsmen and deals with it in appropriate sections of his book, he presents the development of urban institutions in terms of a specifically Russian context. Many Westerners prefer to overlook the remarkable success of the tsarist government in organizing an enormous territory with few natural resources and virtually no natural boundaries into a state that could defend itself from foreign invasion and become an international power. Hittle, on the other hand, points to "the difficulties of building a nation-state in a vast and relatively poor country" as a major force shaping the character of Russia's towns and their inhabitants. The crucial institution, in his judgment, was the *posad*, the urban commune, whose members bore a collective burden (*tiaglo*) of state taxes and services, which they apportioned among themselves in much the same way that the peasant members of rural communes would do after the emancipation of 1861. Equally attentive to the internal relationships among the members of the *posad* and the external relations of the *posad* with the state, this book traces the evolution of the urban commune from its rise amid the domestic crises of the late sixteenth and early seventeenth centuries to its replacement with Western-inspired institutions in the last third of the eighteenth century.

With no access to archival materials, the author uses the standard published sources and draws heavily upon the monographic studies of pre-revolutionary and Soviet historians. The latter have undertaken a number of detailed investigations of towns as social and economic institutions, and their research is relatively unfamiliar to nonspecialists in the West. Hittle makes effective use of their data and comments judiciously on their hypotheses and

conclusions. His prose style is a model of expository writing.

Anyone interested in Russian history or the general history of urban development can read this book with profit.

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A. S. ORLOV. *Volneniia na Urale v seredine XVIII veka: K voprosu o formirovanii proletariata v Rossii* [Disturbances in the Urals in the Middle of the Eighteenth Century: On the Question of the Formation of a Proletariat in Russia]. Moscow: Moscow University Press. 1979. Pp. 262. 2 r. 80 k.

Despite a growing number of monographs on the formation of the Russian proletariat, our knowledge of many important aspects of the subject remains in a rudimentary state, and there is no comprehensive treatment in Russian or any other language to match E. P. Thompson's *Making of the English Working Class*. Such a work would devote considerable space to the mines and foundries of the Ural Mountains where, as early as the eighteenth century, a prototype of the modern Russian proletariat was taking shape. After an initial burst of construction under Peter the Great, the 1750s and 1760s saw the rapid expansion of the Urals metallurgical industry, accompanied by a mushroom growth of the labor force—a motley assortment of peasants and town-folk, convicts and vagrants, tribesmen and soldiers, religious dissenters and other combustible elements, with diverse and often contradictory aspirations.

As the demand for labor increased, whole villages of state peasants were "assigned" to the Urals enterprises as seasonal workers. Crowded into ramshackle dwellings, subjected to fines and beatings, their pay often in arrears, they were saddled with the heavy tasks of mining ore, felling trees, and hauling wood and charcoal, at which they toiled under appalling conditions for little reward. By 1762, when Catherine II ascended the throne, they had been reduced to "utter squalor and ruin," in the words of one of their numerous petitions to the authorities, twenty-nine of which are reproduced in an appendix to this book. The petitions, however, brought little relief, and a wave of violent, sporadic, and uncoordinated disturbances swept through the Urals, severely impairing production. In December 1762 the empress dispatched General A. A. Viazemsky to the area, with full power to quell the disorders. The efficiency with which he carried out this task led to his appointment as Procurator-General of the Senate, a post that he held until his death in 1793. His successor in the Urals, named in December 1763, was General A. I. Bibikov, who was to become a leading figure in the suppression of the Pugachev rebellion in the 1770s.

In his careful account of the Urals disturbances, A. S. Orlov examines the composition of the labor force, the demands of the insurgents, the nature and impact of their activities, and the methods of government repression. He underscores the role of village priests in assisting the rebels (one government list of "instigators and troublemakers" included the names of eight local clergymen), and he analyzes the petitions to the Viazemsky commission, which was formed to investigate the causes of the disorders. He also provides a useful survey of the literature in Russian on the Urals workers, although he completely ignores the fruits of Western scholarship, notably Roger Portal's *L'Oural au XVIII^e siècle* (1950) and Michael Confino's "Maîtres de forges et ouvriers dans les usines métallurgiques de l'Oural aux XVIII^e-XIX^e siècles," *Cahiers du Monde Russe et Soviétique* (1959). Orlov's book is itself a valuable contribution to the history of the Urals workers in the eighteenth century. It is noteworthy, however, for the detail that it adds to a generally familiar picture, rather than for any underlying interpretation of the formation of the Russian industrial proletariat.

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ROBERT EUGENE JOHNSON. *Peasant and Proletarian: The Working Class of Moscow in the Late Nineteenth Century*. New Brunswick, N.J.: Rutgers University Press. 1979. Pp. xii, 225. \$17.50.

Some social scientists, including most Soviet historians, have represented the factory workers of pre-revolutionary Russia as true proletarians who had cut their ties to the village and to the land. Others have held that, culturally and economically, these workers were peasants for whom factory labor was a transient episode. Still others have portrayed them as passing between the poles of the proletarian-peasant dichotomy. Robert Eugene Johnson insists that the dichotomy itself is specious and that, since the underlying question is *mal posée*, evidence heretofore adduced to resolve it is open to other interpretation. He finds a "symbiosis" of the rural and the industrial in the factories of Moscow and Moscow province, and he concludes that the factory workers of the 1880s and 1890s were neither proletarians nor peasants but rather a distinctive and stable compound sanctioned by long tradition.

Migrants from the countryside constituted an absolute majority of the population of Moscow in the late nineteenth century. Johnson shows that most of these migrants eventually left the city, presumably to return to their villages. Female migrants tended to follow urban marriage patterns, but male factory

workers, like peasants, married young and maintained "bifurcated" households so long as they worked in factories. Although workers were estranged and segregated from the life of the city, their alienation was tempered by the support they found in *zemliachestva* (the Russian equivalent of *landsmansschaften*). This support, the lines of communication that followed from it, and the open line of retreat to the village helped to sustain the workers in strikes and other acts of protest. Johnson finds that workers were more militant in industries that were closely linked to the village, for "the fusion of urban and rural discontents produced an especially explosive mix" (p. 158).

Johnson's findings are consonant with those of historians of Western Europe who are also questioning the conventional image of the first generations of factory workers. His findings will not astonish specialists on Russian history who often point out that early in this century many of the best-established workers in industry continued to hold allotments in their native villages. Johnson has nonetheless produced a book of great importance, which challenges the assumptions that permeate the literature on Russian labor history.

The value of Johnson's book is somewhat diminished by its brevity. In some instances, as with payroll sheets, Johnson reports that he could not get access to the archival sources he needed to substantiate his argument. (Although he made use of three archives in Moscow, he relied primarily on published sources.) In other instances, however, such as differential mortality rates and naive monarchism, Johnson simply did not adequately develop and document his point.

Peasant and Proletarian will generate controversy and further research. Since the larger factories of Moscow province were in outlying areas and since most workers in the city of Moscow were not employed in factories, Johnson's inferences about factory workers from the abundant demographic data for Moscow are correspondingly vulnerable. Many historians will challenge Johnson's assumption that the city and province of Moscow represented a typical industrial region in prerevolutionary Russia. Other historians will look more closely than Johnson did at the workers' villages of origin, where they will find evidence that supports Johnson's views. And we can be sure that Soviet specialists on labor history will not simply throw in their hands. The ensuing discussion will constitute a well-deserved tribute to this forceful, lucid, and original work.

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V. S. DIAKIN. *Samoderzhavie, burzhuaizii, i dvorianstvo v 1907-1911 gg.* [The Autocracy, the Bourgeoisie, and

the Gentry, 1907–11]. Leningrad: Nauka. 1978. Pp. 244. 1 r. 80 k.

V. S. Diakin has made many contributions to our understanding of Russia's prerevolutionary crisis of the elites. His latest work details the tensions among the autocracy, the bourgeoisie, and the landed nobility.

Diakin has studied the period 1907–11 with far more thoroughness and presents it with far more nuance than any of his predecessors. He has read an exceptionally broad range of the daily press of the capitals, which he uses as a primary source of rumor and opinion. His use of the available archival material is similarly extensive.

Diakin develops with great clarity the distinction between "Bonapartist" conservative attempts to broaden the autocracy's social base and "legitimist" resistance to any changes in the state structure that existed prior to the reforms enacted after the Revolution of 1905. P. A. Stolypin's program is contrasted with the attempts of a wide variety of reactionary figures to undo what had been done in 1905. These extreme right-wing personalities and groups are portrayed in far greater detail than ever before, and the differences among them are at last made clear. The result is the most sympathetic account of Stolypin's career ever offered by a Soviet author. In comparison with his opponents on the right, the premier comes across as a man of vision. At the center of Stolypin's program was a series of reforms of local government that would have ended traditional noble control of the countryside. These proposals were opposed even by liberal segments of gentry.

Diakin offers an acute description of the rise and fall of the various Duma parties. He pictures the decline of the Kadets, who saw themselves as professional men standing above class interest, and the collapse of the Octobrists, who sought to bridge the gap between landed and industrial property. The fate of these two parties is contrasted with the growth of the *Progressists*, who were supported by Moscow industrialists, and the Nationalists, who represented the Russian landlords of the western borderlands. Both of these groups staunchly defended the interests of the sector of the economy from which their constituents drew their wealth, and each party deeply mistrusted the other. After 1911 both parties lost what little faith they had in the autocracy. This was a recipe for disunity among Russia's elite propertied classes at a moment of acute peril.

Diakin's discussion of the critical Western Zemstvo Crisis of March 1911 is especially compelling. The exchanges between the tsar and Stolypin are presented in far greater detail than ever before. More important, Diakin is the first scholar to sug-

gest that Stolypin was looking beyond the immediate crisis to a moment when his base of support in both houses of Russia's quasi-parliament would be more secure. Nicholas offered Stolypin the opportunity to name thirty members of the State Council at the next time such nominations were to be made. As Stolypin had just been severely challenged in the upper house, he was especially willing to go along with this part of the plan. Had Diakin mentioned that Stolypin had similar hopes for a Duma majority based on the Nationalist party, his argument would have been even more compelling.

Despite these strengths, the book has certain problems. Following the example of A. Ia. Avrekh and E. D. Chermenskii, Diakin confines himself to national politics. The constituencies of the parties are not analyzed. Rather, class labels are affixed to groups to justify rather than explain their behavior. A discussion of the elections to the Third Duma in 1907 could provide clues to the nature of these constituencies, but Diakin ignores the electoral process. One could also argue that his emphasis on Stolypin's local reforms distorts his interpretation of key episodes during the Western Zemstvo Crisis. To argue that conservatives in the State Council opposed the Western Zemstvo Bill primarily because of its lowering of property requirements for participation is a novel approach, but such a view ignores some rather strong evidence that national curiae (to exclude Polish participation) were, for a variety of reasons, the element in the bill that provoked the most significant opposition.

Nevertheless, this is a work of great importance. It is the best book in any language on the Third Duma. No one with an interest in the causes of the Russian Revolution should ignore it.

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GEORGE F. KENNAN. *The Decline of Bismarck's European Order: Franco-Russian Relations, 1875–1890*. Princeton: Princeton University Press. 1979. Pp. xii, 466. \$25.00.

"The Russian diplomatic service," Fritz von Holstein wrote in 1884, "moves about as independently as the maggots in the cheese." This would be an appropriate motto for George F. Kennan's book, for in very large part it is the story of the way in which different entities in the Russian structure of power, following objectives that conflicted with those of the officials formally charged with the conduct of foreign relations, were successful in undermining the diplomatic tie with Germany that had been maintained since the 1860s and in laying the basis for a fundamental change in European power relation-

ships. With his eyes fixed on 1914, Kennan appears to regard this change as a tragic one, although it might be argued that what happened in 1914 was less influenced by the Russo-German break in the 1890s than by the economic and political events of the decade immediately preceding Sarajevo. Even so, the end of Bismarck's system introduced a new liability into European affairs that frightened the European chancelleries and led them into dangerous courses of action. That, in itself, justifies Kennan's decision to examine anew the background of the Franco-Russian alliance.

It is easier to perceive why France was interested in such a combination than to understand Russian motives. The Bismarck system was designed, after all, to keep France isolated, and after the war scare of 1875, with which Kennan begins his account, French statesmen feared a sudden German attack and were anxious to find friends who might be of assistance when it came. But, despite the force of revanchism and the zeal of patrioteers and soldiers, the attitude of official France was always circumspect. French governments were not anxious to press for a Russian connection until they were sure that it would not be a liability, and throughout the period covered here their doubts were unresolved. The initiatives came from the Russians.

It was irrational that they did so, for the Russians had no very good reason for turning away from their German alliance. They had little with which to reproach Bismarck, who had extricated them from troubles of their own making at the Congress of Berlin, who had done his best to support their legitimate interests in the Balkans, and who had restrained the Austrians and his own soldiers when they wanted war with Russia in 1887. The solid advantages of membership in the Bismarck system were clear to Alexander III's foreign minister N. K. Giers. But his was a lonely voice among the welter of ministerial rivals, orthodox prelates, Panslav ideologues, nationalist newspaper editors, agents of the military, and individual go-betweens of obscure motive, like the sometime scientist Elie de Cyon. These forces in the end persuaded the tsar that his frustrations in Bulgaria were due not to the heavy-handedness of Russian policy but solely to Austrian intrigues that were supported by Bismarck. By 1888, it was only his respect for Emperor William I that sustained Alexander's loyalty to the German alliance, and the emperor was not to survive the year.

The hidden assumption of Giers's rivals was that a French alliance would make possible a war that would restore to Russia what they believed it had been unjustly deprived of in 1878 and during the Bulgarian crisis of the 1880s. Kennan makes clear how misguided and self-destructive this adventurism was, given Russia's internal conditions. He lays

the blame on the spirit of nationalism that overtook so much of the educated classes in Russia in the wake of the Crimean War and found its shrillest embodiment in Panslavism and the activities of the Moscow newspaper editor M. N. Katkov. Russia was not the only victim of that crisis of identity, caused by overly rapid social and economic change, which found its outlet in jingoism; but in the Russian ruling class there were fewer effective brakes upon its expression than elsewhere. The ultimate power of decision in the country, Alexander III, was always terrified of appearing to oppose nationalist sentiment, and after 1888 he stopped trying.

Kennan describes his study as a kind of micro-history, an attempt to examine the texture of the historical process and to uncover the motives and concepts by which men were driven. There will be readers who feel that this has led to a rather too exhaustive treatment of a story that, in its basic outlines, is reasonably well known. But this is a small thing compared with the richness of Kennan's account of what went on in the coulisses of French diplomacy and his skill in charting the movements of the maggots in the Russian cheese. Moreover, his personal portraits (Alexander, Giers, Katkov) are incisive and convincing and fully up to the literary standard that Kennan has set in his previous work.

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A. S. SMOL'NIKOV. *Bol'shevizatsiia XII armii severnogo fronta v 1917 g.* [The Bolshevization of the Northern Front's Twelfth Army in 1917]. Moscow: Moscow University Press. 1979. Pp. 199. 1 r. 60 k.

This book is worth the paper it is printed on only because it treats a very important subject. The Twelfth Army stood between the Germans and Petrograd, and its military performance in 1917 had important political repercussions. Because of its proximity to Petrograd alone it was politically significant. The soldiers of the Twelfth Army came under strong Bolshevik influence early in the year, and in the Twelfth Army voting for the Constituent Assembly Bolsheviks outpolled SRs by two to one. The Twelfth Army was home to the 40,000-strong Latvian Rifles, who were firmly pro-Bolshevik by May and were destined to play a key role in the early stages of the Civil War. When the October Revolution was being planned, the Latvians and other revolutionary units were assigned the task of blocking the movement of anti-Bolshevik units from the front to Petrograd (in the event, there were no such units to block). All of this can be gleaned from A. S. Smol'nikov's monograph if one does not happen to know it already.

What one does not learn is *why* the Twelfth Army

should have been so revolutionary and hence so supportive of the Bolsheviks. Smol'nikov's assumption is that the Twelfth Army turned to the Bolsheviks because Bolsheviks told them the truth about the war and the revolution. He further assumes that, by listing individual Bolsheviks, Bolshevik-influenced units, and Bolshevik conferences and resolutions, he has exhausted the issue. Context is a concept foreign to him. Smol'nikov's primitive approach has the effect, among other things, of understating the significance of the activity of pro-Bolshevik units. He does not, for instance, explore the repercussions of the fact that by mid-1917 the Latvian regiments were a law unto themselves and were so cohesive militarily that the Twelfth Army command feared to do anything about them.

Of the many glaring omissions, one is particularly worth mentioning: Smol'nikov does not deal with the difficulties Bolshevik military organizers had in applying the party's antiwar position. For example, the Bolsheviks held (this is not mentioned by Smol'nikov) that, given orders for an offensive, Bolshevik units even while denouncing the orders were to take part if other units did so: the politically more backward soldiers could not be left in the lurch. Did this make sense to units that disobeyed orders to attack? Were Bolshevik activists on the spot forced to go along with the spontaneous soldier refusal—once soldiers had comprehended the antiwar position—to fight under any circumstances?

For a better understanding of events (and Bolshevik activity) in the Twelfth Army one can turn to the reasonably competent monographs on the northern front by Kapustin (1957) and Shurygin (1958) and by Kaimans (1961, or the 1958 Latvian original) on the Latvian Rifles.

JOHN BUSHNELL
Northwestern University

ALLAN K. WILDMAN. *The End of the Russian Imperial Army: The Old Army and the Soldiers' Revolt (March–April 1917)*. Princeton: Princeton University Press, 1980. Pp. xxvi, 402. \$25.00.

The Russian Revolution of February–March 1917 began with industrial strife but ended in military mutiny. The decisive role played by the Petrograd garrison has generally been acknowledged *en passant* by historians, but Allan K. Wildman is the first to examine it thoroughly. His highly professional monograph covers a broader canvas than the subtitle might suggest. He traces the roots of the imperial army's collapse to the late nineteenth century, when it went through an incomplete and unsettling process of modernization. The revolts that broke out during and after the Russo-Japanese War gave an earnest of what was to come once the empire

had entered its "great ordeal" in World War I. Blundering leadership, heavy casualties, and the psychological impact of defeat fatally weakened the army's tradition-bound authority structure. Consequently, it all but disintegrated when the monarchy fell.

The officers' failure to inform their men promptly of what was going on in the capital further undermined their credibility. The fighting and political maneuvering in Petrograd during the "February days" is skillfully analyzed here, as is the impact that the mutiny had on various sectors of the front. Wildman argues that Order Number One, which many conservative officers blamed for the collapse of discipline, initially enabled commanders to keep their rebellious troops under control through the newly established committee hierarchy; it was only in April that the precarious equilibrium was upset as the Petrograd Soviet won its duel with the Provisional Government. The strength of "revolutionary defensism" at this juncture is underestimated here. Wildman concedes that "attitudes toward the war were still ambiguous and undefined" (p. 252), but only after repeatedly assuring us that the soldiers wanted peace. Actually very few as yet favored a "defeatist" policy. Had this been the case, there would have been many more desertions—and Wildman shows convincingly that in March the rate was still tolerable, far less than has sometimes been claimed. More might have been made of instances of xenophobic nationalism among the troops, exemplified by pogroms of officers with German names, which indicated how easily these sorely tried but gullible men could be misled. Soon afterwards their violent antipathies would be manipulated for their own ends by demagogues of the extreme left—a danger to which Wildman seems a little insensitive. We are treated to a certain amount of revolutionary rhetoric in these final chapters, and the narrative slows to a very leisurely pace; there is, however, an excellent section on the short-lived "fraternization offensive" launched by the Germans in the spring.

This solid study makes a notable contribution to the growing body of Western literature on the social aspects of the Russian Revolution. Excellent critical use is made of all published sources as well as of manuscripts in Western and Soviet archives (including some regimental records). This type of material is more plentiful for the latter months of 1917, and the author's promised sequel volume will be awaited with keen appreciation.

JOHN KEEP
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FRANCIS CONTE. *Un révolutionnaire-diplomate: Christian Rakovski. L'Union soviétique et l'Europe (1922–1941)*.

Preface by ANNIE KRIEGL. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques, Civilisations et Sociétés, number 57.) New York: Mouton; distributed by Walter de Gruyter, New York. 1978. Pp. 355. 160 F.

Christian Rakovsky is surely one of the most interesting and sympathetic figures in the history of early twentieth-century socialism. He participated actively in the socialist movements of Russia, Poland, Germany, France, and Switzerland, as well as those of the Balkan states. Converted from Menshevism to Bolshevism under Trotsky's influence, he undertook important diplomatic assignments in 1918 and served as the head of the Ukrainian Soviet Republic until 1923, when he clashed with Stalin over the status of the Soviet Union's constituent republics. Thereafter, Rakovsky was occupied with high-level diplomatic work, serving as Deputy Commissar of Foreign Affairs, as Soviet delegate at Genoa, and then successively as ambassador to London and Paris. He was also a mainstay of the anti-Stalinist opposition, refusing to recant even when so many others did in 1929. He "rallied to the general line" only in 1934 out of conviction that the Nazi threat required Communist unity. His reward for this sacrifice of political principle was a defendant's seat at the 1938 Moscow show trial and ultimately death in a concentration camp.

Francis Conte intertwines Rakovsky's biography during his years as a diplomat with an analysis of Soviet policy toward the major European powers in the 1920s. In a series of well-developed chapters, Conte describes Rakovsky's secret missions to Berlin and Prague in early 1922, his participation in the Genoa and Lausanne conferences, and his term as ambassador in London (which encompassed Britain's *de jure* recognition of the Soviet regime, the Zinoviev letter scandal, and the problems raised for Moscow by the Locarno accords). The most rewarding chapter deals with Rakovsky's ambassadorship in Paris where he tried, but ultimately failed, to negotiate a comprehensive settlement on debts, trade, and political relations. The picture that emerges of Rakovsky as diplomat is one of a charming man with "une amabilité spontanée et une grande facilité d'élocution" (p. 18), who nonetheless was unable to overcome the international climate of hostility toward the USSR. The concluding sections of the book treat Rakovsky's role in the party opposition, his banishment to Astrakhan, his eventual, reluctant recantation, and finally his trial. It is little wonder that the cosmopolitan Rakovsky could not survive in the closed world of Stalinist Russia. He was condemned in the purges not so much for what he had done as for what he was—a European and a passionately internationalist socialist. As he told Kaganovich in 1928, "Je com-

mence à me faire vieux; pourquoi gâcher ma biographie . . . ?" (p. 289).

Specialists may quibble with some of Conte's interpretations, but all will welcome this contribution to the study of Soviet foreign policy. *Un révolutionnaire-diplomate* is a valuable supplement to the work of Gabriel Gorodetsky and Daniel Calhoun on Anglo-Russian affairs and an even more significant addition to the sparse literature on Franco-Soviet relations in this period. The book is based on French and British archival sources as well as a wide range of published documents, memoirs, and secondary works. It includes a useful bibliography, several very nice photographs of its subject, and a list of Rakovsky's many writings.

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NICHOLAS LAMPERT: *The Technical Intelligentsia and the Soviet State*. (Studies in Soviet History and Society.) New York: Holmes and Meier. 1979. Pp. x, 191. \$28.00.

Nicholas Lampert's *The Technical Intelligentsia and the Soviet State* is a useful addition to the growing number of studies in Soviet social history. Lampert, a historical sociologist associated with the University of Birmingham's Centre for Russian and East European Studies, sets out to examine the complex relationships between the technical intelligentsia and the Soviet state in the late 1920s and early 1930s. The period under investigation was turbulent and is crucial for an understanding of the emergence of the Soviet Union as an industrial society. The rapid and intensive expansion of political and administrative power that powered Stalin's "third revolution" created structures and consequences for Soviet industrial development that have endured to the present.

In contrast to the larger and more comprehensive social history of Keith E. Bailes's *Technology and Society under Lenin and Stalin: Origins of the Soviet Technical Intelligentsia, 1917-1941* (1978), the central focus of Lampert's work is the managerial and technical personnel (ITR) of the industrial enterprise. With the exception of the now classic studies of David Granick and Jeremy Azrael, this aspect of Soviet history has been seriously neglected. Lampert's book is valuable for the additional light it throws on the subject.

The organizing conception of this relatively concentrated volume lies in the interplay of the complex and often contradictory pressures on the technical intelligentsia created by competing demands for professional autonomy and rigorous political controls imposed by the Stalinist mode of industrial

management. Lampert examines the impact of these conflicting pressures on the functional roles, social status, and material conditions of the enterprise intelligentsia in two successive phases between 1928 and 1935.

The launching of a severe "class struggle" against the still prerevolutionary intelligentsia between 1928 and 1931 effectively curbed their limited professional autonomy and generated serious political and psychological insecurities. After 1931, the intense politicization of the earlier period eased. The essential configuration of the relationship between the technical intelligentsia and the Soviet state, however, had been permanently altered.

Stalin's policy of the political mobilization of resources through a command system of central planning became institutionalized. The authority of the managers in the form of one-man management (*edinonachalie*) increased as their legitimacy and incomes improved. Fundamental social differences between mental and physical labor, however, became fixed, effectively ending the underlying vision of the revolution. As industrialization progressed, the technical intelligentsia in the enterprise developed as an intermediate layer in a sharply stratified social structure.

In conformity with the author's objectives, the contribution of this volume is limited. The period under investigation was turbulent and the relationships between the enterprise intelligentsia and the Soviet state exceptionally complex. Lampert has a thorough and subtle grasp of the period. One hopes he will bring his talent for clarification and generalization to a more detailed and differentiated examination of this important subject.

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NEAR EAST

HASAN KAWUN KAKAR. *Government and Society in Afghanistan: The Reign of Amir 'Abd al-Rahman Khan*. (Modern Middle East Series, number 5.) Austin: University of Texas Press. 1979. Pp. xxiv, 328. \$19.95.

This work is an authoritative study of Afghanistan during the reign of Amir Abd al-Rahman (1880-1901). Hasan Kawun Kakar describes the nature and operation of the central and local governments, the system of taxation, the organization of the army, and the social and economic structure of Afghanistan.

The first two chapters give a description of the central government with data on the Amir as a person and statesman, the household, court, and council, as well as outlines on the functions of ministers, secretaries, and other "influential elements." Some-

what out of context in this part is information on prisons and punishments and the problem of refugees. The next chapter deals with local government, beginning with a brief description of the historical background, and proceeding to detail the functions of governors, judges, *muhtasibs* (market inspectors also responsible for the protection of public morality), and the organization of the police. The author details the varying approaches in the administration of towns, rural areas, and the frontier districts.

In chapter 4, we see how the "Iron Amir" went about raising funds for "the protection of the frontiers of the country and the honor of the religion and the nation." As the "deputy of the Prophet" Abd al-Rahman considered it his duty to collect taxes, and tardiness on the part of his subjects was therefore "tantamount to abandonment of the commands of God."

Another chapter is devoted to the organization of the army, including recruitment practices, conditions of service, and the weapons of the regular and irregular forces. Afghanistan's social structure is discussed in two chapters, beginning with rural landlord and peasant relations and describing the peculiar class structure of Afghan agricultural society. The problems of nomadism, religion, and minority groups are sketched, followed by two chapters detailing the economic system of Afghanistan in the late nineteenth century.

Kakar's work is encyclopedic in nature and covers a wide range of data; in this sense it reflects such sources as the *Siraj al-Tawarikh*. It is a laudable effort, but this reviewer would gladly have sacrificed comprehensiveness for in-depth coverage. For example, this writer would have preferred to learn more about such topics as the qazis and courts, towns and police, the army and local government, and laws that are each covered in only two or three pages. On the other hand, trade and commerce and the financial system are covered in greater scope. An appendix lists Afghan weights and prices of agricultural products and other commodities during the two decades of Abd al-Rahman's reign, followed by a note on the sources utilized for this study. A glossary lists terms in various Middle Eastern languages employed in Afghanistan at the time. The bibliography is followed by a comprehensive index.

Kakar's work is a welcome addition to the field of Afghanistan and Middle East studies and should be required reading for anyone interested in Islamic society and civilization.

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G. S. GEORGHALLIDES. *A Political and Administrative History of Cyprus, 1918-1926, with a Survey of the Founda-*

tions of British Rule. (Texts and Studies of the History of Cyprus, number 6.) Nicosia: Cyprus Research Centre. 1979. Pp. vii, 471. £13.50.

A much criticized, unilateral British initiative at the time of the Congress of Berlin of 1878 was its separate Convention of Defensive Alliance by which it obtained use of the island of Cyprus from the Turkish sultan. Disraeli, the imperialistic premier, called it "the key" to the East; Lord Northcote said it was a vital "place of arms, where our ships can lie." From Famagusta, Britain could land troops at Alexandretta to fight the Russians and also develop trade links with the Persian Gulf. But in 1882, the British were in Egypt, a much better "place of arms." Cyprus, nominally Turkish, remained in British hands—in case the French, not the Russians, took it—for eighty years more. It was unthinkable to give a Christian population back to Abdulhamid; yet strategically Cyprus was soon deemed useless. Why not give it to Greece? Or make it an imperial showcase?

In an excellently researched and clearly argued volume, G. S. Georgiallides of the Cyprus Research Centre traces, with admirable impartiality, the failure of the world's greatest power to provide the (at first) welcoming Cypriots with a resoundingly good administration and other great powers with a lesson in fruitful colonial government. In part 1 (four chapters), the author is at his most interesting in revealing how, from 1878 to 1915, leading British statesmen and departments of government (Foreign Office, Colonial Office, Treasury, Admiralty) turned somersaults in their dealings with successive, and "always polite," Cypriot delegations to London. If Cyprus was imperially important, the delegations asked, why not develop it properly, with roads, mines, low taxation, and, above all, abolition of the "tribute" due to the Turkish government? If Cyprus was not important, please could it join Greece as the Ionian islands did in 1863?

With a few exceptions, British rulers come out badly, less for deceit and ill manners than for hanging on without being able to tell the Cypriots, or each other, why. Kimberley in 1880, Winston Churchill in 1907, and Lloyd George in 1915 encouraged hopes of *enosis*. The historians advising the Foreign Office—Toynbee, H. A. L. Fisher, Temperley, and Burrows—were all for it. One can guess who resisted and prevailed: Curzon, Milner, Walter Long. In 1915, Greece was offered the island if it would join the Allies and aid Serbia. The neutralist King Constantine refused. The offer was not made again, but it vindicated the *enosis* agitation, before and after.

In part 2 (five chapters), Georgiallides devotes over three hundred pages to the island's administration after World War I, a story of small-minded,

second-rate administrators (Cyprus was denied the best until the arrival in 1926 of Ronald Storrs) who strengthened the *enosis* they condemned as disloyalty with their own poor performances. A survey conducted in 1927 shows a negligible improvement over the previous Turkish performance, except in the impartiality of the island's courts and the disappearance of feudal violence. The old salt tax was still there; 19 percent of the annual budget was spent on police (most of them Turks complete with *fēz*) and prisons, only 3 percent on agricultural aid (p. 404). Infant mortality was 163 per 1,000 (p. 400), and average peasant income totaled £11 per annum. Above all, the "tribute" was still exacted long after the Turks had given up all claims on Cyprus and in practice misapplied by the British Treasury to other purposes. The appendixes on island revenues, imports and exports, and forced sales by bankrupted peasants (pp. 425–29) make shameful reading. How clearly a governor like Malcolm Stevenson prepared the way for Grivas and Makarios!

This important book, quite unequaled as a study of British Cyprus, is beautifully printed but needs a map and a bibliography.

ALLAN CUNNINGHAM
Simon Fraser University

AFRICA

SA'AD ABUBAKAR. *The Lāmibe of Fombina: A Political History of Adamawa, 1809–1901.* (Ahmadu Bello University History Series.) New York: Oxford University Press and Ahmadu Bello University Press. 1977. Pp. x, 190. \$29.95.

Since Nigeria's independence, that nation's competent young historians have developed an enviable tradition of regional political history. Their work, however, has focused predominately on the country's southern portion, and few indeed have been the contributions of historians northern by birth and by training. The appearance of *Lāmibe of Fombina* is thus doubly welcome, as the second book in the new "Ahmadu Bello University History Series" on Northern Nigeria and as the debut of Sa'ad Abubakar, one of the very few indigenous northern historians. The book is a detailed study of Fombina, a territory extending from below Lake Chad to the margins of the rain forest. Here was established one of the large emirates of the Sokoto Caliphate, following the great jihad early in the nineteenth century. The new emirate was more Fulbe (Fulani) than most of the others, with Fulfulde widely spoken there. The book's detailed reconstruction of the emirate's origins, development, and decline draws

extensively on oral tradition, much of which was collected by Abubakar.

As he tells the story, the emirate's founder, Modibbo Adama, began the jihad against the pre-existing rulers ca. 1809. The jihad resulted not from any infringement of Fulbe freedom to practice Islam but instead from underlying social, economic, and political conditions. Fulbe strength and leadership were augmented by an influx of kindred refugees from Borno, where the jihad had failed. They brought ideas of strong central government of the sort they had experienced in that area, and they also exacerbated competition for the limited grazing area of the region. By the 1830s, the jihad was virtually over. A large centralized system now replaced the multiplicity of smaller units that had existed before. The book's emphasis here turns to the consolidation and expansion of the emirate, a process that lasted almost to century's end, when the emirate was partitioned by the German and British colonial authorities.

The reader who is not familiar with the area (or for that matter the reader who is) will be impressed with the depth of detail that Abubakar presents. The texture is thick. There are "war cabinets" and generals, archers, infantry and cavalry, internecine challenges to leadership, disputed successions, and rebellions. There are blockades of trade, collaborators among the conquered non-Muslim peoples, and matrimonial connections as a road to political power.

Certain aspects of Abubakar's book are open to criticism. More attention to the economic foundations of the emirate would have enhanced understanding. There are indeed good sections on tribute and taxes, but the treatment of slavery leaves something to be desired. Slave settlements were important economically (for producing agricultural commodities) and militarily (for the slave settlements placed in contested border territories). But the main treatment of slavery puts its stress on comparing favorably the slaves' condition in Fombina to their exploitation in the Americas. That a slave's life was perhaps more tolerable than on the sugar plantations and cotton fields of the New World certainly deserves comment, but the comment should not amount to apologia.

One fault of the editors: the end paper map is not very helpful, with much empty space and little of use identified. (The other two maps are much better.) Surprisingly, the meaning of "Lāmibe" in the title is not immediately defined. Only the eventual discovery of an excellent glossary allows the non-specialist, non-Fulbe reader to find it means "emirs." But these problems are offset by otherwise good editing, adequate style, a good index, and, as just mentioned, the invaluable glossary.

The impression is one of careful research and a

promising academic future for Abubakar. But as so often seems to happen in modern Nigeria, he is currently lost to academic research in his post as commissioner for education in Gongola State. One hopes for the sake of Nigerian historiography that the loss is only temporary.

J. S. HOGENDORN
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HENRIKA KUKLICK. *The Imperial Bureaucrat: The Colonial Administrative Service in the Gold Coast, 1920-1939*. (Hoover Colonial Studies; Hoover Institution Publication, number 217.) Stanford, Calif.: Hoover Institution Press. 1979. Pp. xvi, 225.

BJÖRN M. EDSMAN. *Lawyers in Gold Coast Politics, c. 1900-1945: From Mensah Sarbah to J. B. Danquah*. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 111.) Uppsala: University of Uppsala; distributed by Almqvist and Wiksell International, Stockholm. 1979. Pp. 263. 87.00 KR.

Henrika Kuklick's *The Imperial Bureaucrat* will be of interest to any scholar who has ever read colonial memoranda in the National Archives of Ghana without knowing anything about the people who wrote them. Based on sources such as official "diaries" and recruitment records of the British colonial service, this work is a quantitative content analysis, detailing the origins, careers, and administrative characteristics of ordinary colonial officials. The reader learns something of how the official British policy of "indirect rule" actually worked in practice, becoming individualized personal rule by junior officials, each one having control over "his" tribe.

Kuklick's perspective is that of organization theory and adult socialization or role theory, which, although it will be of interest to sociologists in those specialties, will somewhat disappoint Africanists. Although Kuklick is generally well read on Ghana, she either is unfamiliar with or rejects the more recent dependency-Marxist literature on her topic, and, indeed, she abstracts her subjects almost completely from their African surroundings. Although she acknowledges that colonial rule was coercive, she stresses rather superficial administrative questions rather than political issues. Moreover, her ethnography is highly quantitative. She provides, in a long methodology section, very detailed information (such as the amount of time spent on paperwork by diary writers) at the expense of a more qualitative, textured picture that might have provided a more intuitive sense of the nature not only of the colonial civil servants but also of their relationships with their African subjects. Kuklick seems unaware of the possibilities for bias in the essen-

tially self-generated data that she uses; not only were colonial diaries "cooked" (as she acknowledges) because they were to be read by superior officers, but they also present only one side of a two-sided story, that of the interaction between British administrators and the Africans whom they administered and who often opposed them.

Björn M. Edsman's *Lawyers in Gold Coast Politics* provides a good counterpoint to *The Imperial Bureaucrat*, focusing on the opposition generated by a small number (never, it seems, more than a hundred) of Western-educated Gold Coast lawyers to colonial administration. This meticulously researched dissertation is one of the few studies that successfully wends its way through the intricacies of local Gold Coast politics (especially in two fascinating sections on chieftainship disputes in Accra, the capital, and in the town of Asamankese).

Edsman makes two major arguments. First, in direct contradiction to the nationalist school of Gold Coast history, he does not agree that the intense opposition from chiefs and westernized elites during the colonial period constituted a linear, progressive nationalism, culminating in Nkrumah's takeover in 1957. Rather, he believes that the chiefs' relationships with the British, especially after about 1920, were guided by pragmatic adaptation to social change, while the Westernized elites, far from opposing colonialism, were trying to obtain a recognized place in the colonial judiciary and legislature. The second argument is that, although Gold Coast lawyers consistently maintained that they were trying to defend traditional law and custom, they were actually attempting to manipulate tradition and its symbolic representatives, the chiefs, in their own interests and became distraught when the chiefs began to garner their own political power. While proving this point Edsman also explains many of the changes that took place in indirect rule. Unlike Kuklick, who attributes changing practice to changing ideology, Edsman argues that much colonial government practice was a response to indigenous actions: for example, more "direct" rule was reintroduced in the 1930s consequent upon perceived irresponsibility on the part of chiefs in running their courts and their finances.

Edsman's book is an excellent source for Ghana specialists; it would be difficult, however, for other specialists, even other Africanists, to follow. It lacks a chapter explaining the social origins, especially the class positions, of the lawyers it studies, although there are many hints throughout the book that their "disinterested" constitutional actions might have been influenced by the high court fees they obtained or by their roles as landowners and businessmen. Explanations of constitutional issues are sometimes too detailed even for a reader familiar with Edsman's archival sources to follow, and one sometimes has the impression that one is learn-

ing more about personal disputes than about political issues. The book deserves an index, which is lacking (as also are the page numbers on the table of contents).

Although both *The Imperial Bureaucrat* and *Lawyers in the Gold Coast* are useful books, the latter is richer, more complex, and at times genuinely exciting in its explanations of the complicated internal politicking of the colonial Gold Coast. It is an exemplary archival study.

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ASIA AND THE EAST

JEROME CH'EN. *China and the West: Society and Culture, 1815-1937*. Bloomington: Indiana University Press. 1979. Pp. 488. \$22.50.

In this book Jerome Ch'en ambitiously aims to study China's contacts with the West in all its ramifications—religious, diplomatic, political, economic, cultural, social, and legal. As if the task is not hard enough, he chooses to cater both to the general reader and to the specialist, and, to satisfy the latter, he includes many details that, because of the printing cost, are not supported by footnotes. The end product is a highly unbalanced work filled with sweeping generalizations as well as interesting but unverifiable information that has no direct bearing on the main themes.

As an illustration, the author, speaking of the era from 1860 to 1937, writes on page 451: "The government had given three reasons for the preservation of Confucianism. . . . These were the centralization of power . . . the consolidation of civil service morale . . . and the discipline of the people. . . . The fact was that, except for the attempts to centralize power, all the other . . . goals remained only rhetoric throughout the whole period up to 1937. . . . The cadre of the KMT became . . . dysfunctional. . . . With the party corrupt . . . the country stood weak." The passage is as sweeping as it is ambiguous. Was there only one government between 1860 and 1937? When and where did the government give these three reasons? The KMT might be corrupt, but the author's summary condemnation is hardly scholarly. Moreover, it clearly implies that corruption was the only or at least the main reason for China's weakness, a position that I think is rather simplistic.

Perhaps a word should be said of certain theses that, although never specifically formulated and defended, nevertheless run through the book. One is Ch'en's partiality toward the missionaries, to the degree that he accepts their words uncritically. Thus, Chang Chih-tung is said to have invited the

missionaries to edit textbooks, and the dowager empress is described as having resorted to Western medicine; when local authorities converted some temples into schools, Ch'en sees it as the outcome of a growing public disbelief in idols, which in turn is attributed to missionary efforts. In actual fact, the conversion of temples by government orders was not extraordinary in China, and it need not have had anything to do with the very small number of converts or the missionaries.

A point that Ch'en does not stress is the abundance of derogatory information about the missionaries to be found in contemporary Western sources. Apparently, the missionaries not only participated actively in the opium trade and abused their role as interpreters in diplomatic negotiations, but they also gathered intelligence for the invading Anglo-French forces in 1858-60 and participated in the looting of Chinese properties in 1900. Yet practically all of them claimed they knew how to save China politically as well as spiritually. Considering their limited qualifications, such attitudes can mean only extreme bigotry and ignorance. Small wonder, then, that investigative teams sent out by the home boards of American churches invariably criticized their own missionaries. It is hard to understand how a writer with a Chinese background can develop an entirely opposite point of view and see the missionaries as the main social and moral force before the rise of the Communist Party in China.

Ch'en also sees the KMT as defenders of the Confucian faith. In his view, this alone was sufficient to make the KMT an anachronism and to assure its downfall. But Ch'en apparently forgets the ecstatic acclaims of Western missionaries that Chiang Kai-shek was a devout Christian. Ch'en also ignores many of Chiang's non-Confucian features, such as his tendency to neglect the peasants, his willingness to bribe or assassinate as the occasion demanded, and his great trust in Tai Li, the dreaded secret police chief, and in Kung and Soong, both men of admittedly comprador origin. Can a party be Confucian when headed by an un-Confucian Führer?

A very notable feature of Ch'en's writing is his unbounded enthusiasm for Mao Tse-tung. Here his antagonist is none other than "the powerful Communist Party" itself. With Mao's widow under arrest and many of Mao's policies reversed, it is hard to see how both the man and the party can be eulogized at the same time. To be sure, it has been suggested in some quarters that Mao was not wrong and that his wife was the one who misled him. But can a messiah be misled—and for many years? Perhaps the author can enlighten us in his future publications.

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JAMES REARDON-ANDERSON. *Yenan and the Great Powers: The Origins of Chinese Communist Foreign Policy, 1944-1946*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1980. Pp. x, 216. \$15.00.

One of the strengths of James Reardon-Anderson's book lies in its challenge to the assumption, implicit in a number of other studies of policy making in China during the Second World War, that the Chinese Communist Party can be treated as a force almost monolithic in nature. He looks in some detail at the various factors that went into the determination of the CCP's foreign policy from 1944 to 1946, the years in which Japanese military strategy, American intervention, and, finally, the Russian entry into China encouraged, indeed forced, Yenan to break out of its isolation and led the party to develop for the first time a foreign policy of its own.

It was, in the author's view, the launching of the last great Japanese offensive (Ichigo) in 1944 and the prospect of some American military and diplomatic support (the Dixie Mission) that forced Yenan to choose between a diplomatic initiative (trying to rebuild the United Front) and a military initiative (territorial expansion in the wake of the Japanese advance against the Nationalists). Mao Tse-tung and Chou En-lai, he argues, favored the diplomatic approach, but paradoxically it was Mao's own *cheng-feng* movement of 1942 that seriously weakened the internationalist and United Front wing of the CCP and strengthened advocates of military self-reliance like Chu Teh and Lin Piao. Mao and Chou thus found their diplomatic efforts undercut by the success of Communist arms, as well as by the antics of General Hurley and his entourage. The entry of Russia into the war and its rapid advance into Manchuria brought new problems, and the fear arose that Moscow and Washington would, for different reasons, help strengthen Chiang Kai-shek in his bid to emerge as the legitimate ruler of postwar China. Once again, in late 1945, Mao and Chou tried to advance their aims by diplomacy, and once again the Yenan militarists opposed them.

It is Reardon-Anderson's thesis that the Communists were governed, in both their military and diplomatic policies, not by an ideological world view, but by pragmatic consideration of what was possible, given the relative strengths of themselves and their opponents. In the end, he states, what made possible the resumption of civil war was the withdrawal of both Russian and American forces from North China; this gave the CCP a new freedom, and at that point it seemed that the battlefield was more likely than the peace table to lead to appreciable gains.

Yet despite this view of Communist pragmatism, the author does not allow himself to imagine that a

less partisan American policy might have avoided the long blight in Sino-American relations that followed. He is right, I think; the Communists were starting to make their own policy, and to imply that American friendship (or simple neutrality) might have changed their outlook toward us is to fall victim to the old arrogant view of American omnipotence, the view that believes it to be within Washington's power to determine how other nations behave.

NICHOLAS R. CLIFFORD
Middlebury College

CARL BOYD. *The Extraordinary Envoy: General Hiroshi Ōshima and Diplomacy in the Third Reich, 1934-1939*. Washington: University Press of America. 1980. Pp. x, 235. Cloth \$17.00, paper \$9.40.

This *envoy extraordinaire* was an army colonel posted to Berlin in 1934 as Japan's military attaché who so successfully ingratiated himself with the Nazi leaders, including Adolf Hitler, that he was able to play a major role in German-Japanese relations for most of the next decade. Infighting and indecision in Tokyo and ignorance of European affairs on the part of army officers rising to political power there enabled General Ōshima (as he shortly became) both to advance his own career and to bring his strong pro-Nazi sympathies to bear on Japan's foreign policy by exploiting his privileged access to the German leadership. His machinations in the late 1930s, which often amounted to insubordination to his civilian—and occasionally even his military—superiors, went largely unchecked, like those of some of his Imperial Army contemporaries on the Asian mainland. At the same time his diplomatic maneuvers indirectly aided army leaders in increasing their grip on the home front and, more directly, helped them to broaden their focus from the Far East to include Europe and the West as well.

According to Carl Boyd, the author of this well-researched monograph, Ōshima was "extraordinary" for initiating, through his carefully cultivated acquaintance with Joachim von Ribbentrop, the Anti-Comintern Pact of 1936, thus setting Japan on the road to Axis alignment in the Tripartite Pact four years later. Subsequently during the Second World War, Ōshima was even more important to United States' leaders for the full and frank revelations of the Führer's thinking that American cryptanalysts in Operation "Magic" provided by deciphering the Japanese ambassador's messages home. While the present work concentrates on the prewar period, during which the envoy established himself in the Nazis' confidence, Boyd anticipates some of the wartime developments as well, making use of the recently released "Magic" documents and in-

cluding a good deal of material from the later period in his numerous and valuable appendixes. The concluding chapter briefly brings Ōshima's story down to his sentencing at the Tokyo War Crimes Tribunal in November 1948.

Boyd reveals that Ōshima had closer contact with Heinrich Himmler than he cared to admit at his postwar trial. It is regrettable, if quite understandable, that the author has been unable fully to document other aspects of Ōshima's undoubtedly important role in collaborative clandestine activities directed against the USSR.

This book is indexed, and it offers an extensive bibliography and several interesting photographs of Ōshima with Nazi leaders and, later, at his postwar trial.

WILLIAM LIVINGSTON SPALDING, JR.
Columbus, Georgia

J. T. F. JORDENS. *Dayānanda Sarasvatī: His Life and Ideas*. New York: Oxford University Press. 1978. Pp. xvii, 368. \$15.50.

In the dozen chapters of this volume, J. T. F. Jordens has satisfied the need for a scholarly, broad, readable study of Dayananda Sarasvatī, the founder of Arya Samaj and a co-founder, along with Ram Mohan Roy and Vivekananda, of modern Hinduism. Building on the earlier writings of Lekhram, Mukhopadhyay, and Ghasiram, all of whom seldom rose above useful chronicle, Jordens has assembled a true historical biography that places its hitherto elusive subject in a secure temporal and cultural context. Dayananda's thought and activity are related to his historical environment with freshness and sympathy. One can understand and appreciate better than ever how the man contributed to the rise of modern India.

More specifically, Jordens has accomplished four things. First, he has clarified the full extent of Dayananda's activity as a reformer and as a catalyst for social and cultural change during many years of wandering in northern India. Second, he has identified and analysed regional influences on Dayananda's thought and values, most notably his experiences in Kathiawar, Mathura, and Rajputana. Third, he has provided a reliably documented, coherent, organically conceived account of Dayananda's ideas and their complex development. Dayananda's originality within the Hindu tradition is due to a strikingly "liberal" vision of man, in which "freedom, activity, and involvement in the world constituted . . . the basic nobility of man, the source of his greatness, for which he owes God gratitude, but for which he is wholly and solely responsible" (p. 282). Fourth, the biographical thread is woven deftly into the intricate pattern of late nine-

teenth-century Indian history, with the happy result that Dayananda's life lights up an era. Despite the profusion of topics, Jordens keeps a firm grip on his material and seldom allows the narrative to lag. In addition, Jordens has added significant new materials on Dayananda to the existing stock.

The bibliography includes the complete works of Dayananda, various letters, notices, and reports associated with him, and select writings on him in English, French, German, Gujarati, Hindi, and Marathi. Each chapter is supported by detailed notes. A useful introduction acknowledges earlier students of Dayananda's life and work while explaining their limitations. Jordens's sensible compromise with the irksome problem of diacritics is most welcome. None are used for persons or places, which greatly relieves the burden of print on each page.

Jordens has produced a biography likely to remain standard for some years to come. Students of nineteenth-century India will find it a steady and helpful companion.

KENNETH R. STUNKEL
Mornmouth College

K. N. CHAUDHURI and CLIVE J. DEWEY, editors. *Economy and Society: Essays in Indian Economic and Social History*. New York: Oxford University Press. 1979. Pp. x, 358. \$16.95.

Assembled in this volume under the appropriately general title of *Economy and Society* are thirteen disparate essays that were originally papers delivered at a conference held at St. John's College, Cambridge, in July 1975. The ostensible purpose of this gathering was to converge the "invisible college" of South Asian economic historians from India, America, Western Europe, and Australia to refine existing research and to chart future research directions.

Promising in both these respects are particularly the essays in the first two parts of the collection on "Agrarian Structure and Agricultural Development" and "Trade and Finance." A strong beginning is provided by P. J. Musgrave's provocative article on the changing structure of power in rural United Provinces in the late nineteenth and early twentieth centuries. Focusing on the same region, but on a micro-level are the pieces by Ian Stone and Dirk H. A. Kolff. The former examines the Ganges canal tract in Muzaffarnagar district between 1840 and 1900 to counter the thesis that canals had deleterious effects on the rural economy; the latter draws on rich manuscript settlement records to document the transfer of landholding rights at the sub-Jhansi district level.

The other two essays in this section shift attention to the south and west of India. Christopher Baker makes an interesting case for the twentieth-century

transformation of Madras headmen and other village officers from "leaders of rural society to petty bureaucrats" (p. 46). And Neil Charlesworth claims from a scrutiny of overall agricultural trends in the Bombay Presidency between 1900 and 1920 that there was considerable growth in crop and total acreage.

K. N. Chaudhuri's "Markets and Traders in India during the Seventeenth and Eighteenth Centuries" opens the second section with a broad and stimulating set of questions. While complete answers are not furnished here, it is an engaging fragment of the author's recently published book. Similarly, B. R. Tomlinson's discussion of monetary policy and the rupee ratio question is better dealt with in his recent monograph. A more self-contained work is Marika Vicziany's carefully documented essay that establishes convincingly the increasing domination by foreign business houses of the raw cotton export trade of Bombay between 1850 and 1880.

Three very different essays constitute the section "Industry and Labour." Clive Dewey discusses at length "The Government of India's 'New Industrial Policy,' 1900-1925: Formation and Failure." Its curious conclusion is that lack of industrialization in India was not due to government policies or existing social and economic conditions internally or internationally but that underdevelopment resulted because "Indian businessmen were unable to build up permanent alliances with Indian civilians through systematic *douceurs*, or to bring continuously effective political pressure to bear upon them" (p. 252). It further contends that "perhaps no country has ever industrialized—in which entrepreneurs have been unable to corrupt the state" (p. 252). In briefer pieces, J. Krishnamurty applies statistical techniques to census data to show little change in "The Distribution of the Indian Working Force, 1901-1951," while R. K. Newman highlights the important role of "jobbers," or intermediaries, between cotton mill workers and their employers in his analysis of the "Social Factors in the Recruitment of the Bombay Millhands."

Two essays comprise the final section on "Towns." By looking at the origin, development, and changing form and function of Madurai and Madras, Susan J. Lewandowski sets up perhaps too stark a dichotomy between "two urban types; one based on traditional notions of design motivated by a particular Hindu world view, and the other based on particular Western notions of economic and political dominance" (p. 325). The volume ends on a rather diffuse note with H. E. Meller's "Urbanization and the Introduction of Modern Town Planning Ideas in India, 1900-1925."

While most of the essays in this volume are quite suggestive, very few are exhaustive. Moreover, this

collection has little thematic unity, as is obvious from its lack of an introduction or a conclusion. It should also be mentioned that this work is not representative of existing scholarship since only four of its contributors are non-British: one each from the Netherlands, Australia, India, and the United States.

ANAND A. YANG
University of Utah

D. E. U. BAKER. *Changing Political Leadership in an Indian Province: The Central Provinces and Berar, 1919-1939*. New York: Oxford University Press. 1979. Pp. x, 233. \$11.95.

D. E. U. Baker's thesis in this work is that in the area that came to be called the Central Provinces and Berar two distinct politics developed that focused around a loyalty either to Hindi or to Marathi institutions, and that by the time the Congress ministry resigned from power in 1939 the control of the area no longer lay with the Marathi politicians but was in the hands of the Hindi speakers. The research base of this work is the large number of newspapers, government documents, private papers, biographies, and personal interviews that concern these developments. This book is the first work to undertake an analysis of politics in this area.

One of the great strengths of the work is that it seeks to document the demise of Marathi politicians in the face of increasing British patronage and appreciation for Hindi politicians. The political leadership of the area before and after World War I was mostly drawn from the Marathi Konkanastha Brahmin community because they took to the education offered by the British and because the urban structure out of which they emerged was more highly developed. Even in 1920 the political arena was largely in the hands of these Tilakites. Later in the decade Marathi control continued first under the guise of the Swarajists and then under the Co-operative Responsivists who, like the Swarajists, wanted to participate in the Legislative Council elections, but, unlike them, also wanted a taste of power. One member of this group, S. B. Tambe (p. 79), accepted the British invitation to be the Home Member in the Executive Council in 1925 to the dismay of the Gandhian No-Changers and many of the Swarajists. Equally important was the opposition of Marathi politicians to the Congress from another perspective. One Deshastha Brahmin medical doctor from Nagpur, K. B. Hegdewar, founded an extremist Hindu organization, the Rashtriya Swayam Sevak Sangha (p. 104). Hegdewar hated the British for having usurped the rights of the Maratha kings, was convinced of a Muslim threat, and in the late 1920s helped to organize Hindu drill

teams and festivals. In 1927 serious communal riots broke out in Nagpur in which twenty-two people were killed. By 1940 when Hegdewar died the RSS had a membership of 100,000.

Part of Baker's thesis concerning the emergence of middle-class politicians in the area is based on his idea that these men were largely able to dispense with their dependence on landed notables in 1918 (p. 26). Yet much of his evidence suggests that this dependence on bankers and landholders continued for a much longer time. The Tilakites in 1921, for example, got valuable support from influential landlords (p. 78). Between 1925 and 1929 the Gandhian Congress, according to Baker's own evidence, increased its influence among landholders and tenants (p. 83). There are many other instances that suggest that the transformation to which Baker refers was, in fact, much more gradual. The main drawback of this work is its preoccupation with "politics" to the neglect of a broader consideration of what this politics was based on, even though Baker has a chapter on the development of economic and social institutions. For instance, he demonstrates that there was a substantial fall in the industrial labor force between 1929 and 1939 but does not relate this to any other significant development. However, Baker's appreciation for the British contributions are ably presented in his chapter on "Government and the Politicians." In this chapter he shows how Sir Frank Sly, the governor, did little to cultivate Hindi notables and politicians in the early 1920s. The appointment in 1925 of Montagu Butler, an ICS officer from the Punjab, meant more and more support for the Hindu region, including aid to E. V. Raghavendra Rao, a Telugu Brahmin whose forebears had come into the area on the coat-tails of the British. The Congress ministry of 1937 and 1939 simply formed the arena for the final dispute between Hindi- and Marathi-speaking politicians.

EUGENE F. IRSCHICK
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MOHAMMAD ISHAQ KHAN. *History of Srinagar, 1846-1947: A Study in Socio-Cultural Change*. Srinagar: Aamir Publications. 1978. Pp. 231. \$21.00.

Kashmir, southern Asia's most famous strategic region, centers upon Srinagar. As political capital of the Dogra rulers and focal point of European tourism, the city has inspired or provoked a myriad of travel accounts and official papers. Mohammad Ishaq Khan has utilized these to compile this study of Srinagar's economic, social, and cultural life from 1846 to 1947. Urban historians will find interesting details here but may be frustrated by the paucity of

material on the physical patterns of Srinagar's growth or on the distribution of its various spheres of economic and cultural activity. (The government of India has added to the problem by not granting the author permission to include a map of the city.)

Srinagar's scenic setting with its lakes, river, and canals proved a growing attraction for the colonial European elite after communications had been improved in the late nineteenth century. Yet this "Venice of the East" surpassed its namesake in inadequate drainage and conservancy. Tourism provided a concern for public health and stimulated efforts at municipal improvement. One visitor found "a city reared on filth, a people born in filth, living in filth and drinking filth" (p. 21). Khan perhaps takes such assessments too literally, but he demonstrates the ties between the imperatives of colonial health and the grudging acquiescence of Srinagar's citizens in the costly rigors of "municipal improvement."

Tourism and communication worked other changes in the city. Some handicrafts were subverted by newly available outside products. The shawl industry apparently declined as much from the taxation policies of the Dogra rulers as from external competition. Khan portrays shawl making in decline but never clarifies whether this forced weavers into other occupations or whether it was the attractive alternatives in rug making that drew them away. Tourism stimulated several new "handicraft" industries including furniture making and boat building.

Khan's discussion of social and cultural life proceeds in a detailed descriptive fashion that does not often attempt comparison or generalization. The growth of education and public opinion and the relations of Hindus and Muslims are presented with sensitivity. There is a strong undercurrent of criticism of the Dogra maharajas throughout. Khan does not yield to the temptation to label them religious tyrants, but he does present the rulers as obstacles to progress for all Kashmiris. In the process, British missionaries are credited for stimulating reform ideas that aimed at a general transformation of the society. Clearly some changes were cosmetic. The dress and decoration of Kashmiri Pandit boys enrolled in the mission school quickly changed when, in an effort to overcome their "lethargy," compulsory sports and games were introduced. As Khan remarks in a rather unnerving fashion, the boys' noserings and earrings "were found to be a painful accessory when boxing was introduced" (pp. 135-36). Other changes had deeper consequences for the minority of educated Muslims and Hindus. Khan clarifies the cross-cutting elements of religious revivalism, secular social reform, and nationalism.

There are real shortcomings. Khan's explanations

are not always consistent. Some of his conclusions do not seem grounded in the text. Footnotes and bibliography are extensive but difficult to reconcile and utilize. In the end the reader grasps Srinagar's past uncertainly but may appreciate more fully the socioeconomic complexities underlying modern Kashmir's turbulent history.

FRANK F. CONLON

University of Washington

OLIVER B. POLLAK. *Empires in Collision: Anglo-Burmese Relations in the Mid-Nineteenth Century*. (Contributions in Comparative Colonial Studies, number 1.) Westport, Conn.: Greenwood Press. 1979. Pp. xiv, 214. \$25.00.

Perhaps more has been written on Anglo-Burman diplomatic relations and the three Anglo-Burman wars of the nineteenth century than any other aspect of Burmese history. The fact that yet another work should appear on these exchanges and conflicts when so much of the Burmese past has been neglected and in a time when so few new works are being published on Burma is indicative of the frustrations facing those who wish to do serious research relating to Burma. Denied access to the invaluable (and slowly perishing) sources in Burma itself, most scholars, at least since the early 1960s, have had to be content with the special collections of Burmese-language sources and the Western-language archival records found mainly in London, or they have been forced to retool and find other, more accessible areas in which to focus their research. Oliver B. Pollak belongs to the small band of Western scholars that has persisted in its determination to write the history of Burma, and of that group, those who continue to mine the English-language official records housed in the India Office in London.

Pollak's *Empires in Collision: Anglo-Burmese Relations in the Mid-Nineteenth Century* centers on the events leading up to and the aftermath of the second Anglo-Burman war of 1852. As he correctly argues in the introduction, this is the least-studied period of Anglo-Burman interaction. Pollak's work provides numerous correctives to earlier writers' accounts of motives and events. It also includes painstakingly documented descriptions of the tortuous relations and frequent confrontations between British officials, merchants, and missionaries and the Burmese who were so determined to keep these unwelcome intruders beyond the boundaries, or at worst on the periphery, of their beleaguered kingdom. Pollak also attempts—especially in the chapters on the period after 1852—to deal with Burman reactions and changes within the rump Konbaung kingdom that remained. Because of his reliance on English-language sources, however, and his tendency to take

British reports at face value, these portions of *Empires in Collision* are the least successful and provide us with little new information. In this connection, it is surprising that Pollak makes no use of the recent and superb work of scholars like Victor Lieberman and William Koenig who have analyzed pre-colonial Burmese political history on the basis of both indigenous and English sources. The band of Burma scholars is so small and the sources available so limited that the neglect of works of high quality, like those of Lieberman and Koenig, greatly impairs Pollak's ability to develop a convincing and insightful account of Burman responses to the British imperialist challenge.

Although useful for the additional information it provides regarding the motivations and actions of the men who led the British advance into Burma, *Empires in Collision* offers little in the way of new interpretations or understandings, either of the British-Burman confrontations specifically or of the nature of nineteenth-century diplomatic or imperial history more generally. Pollak is aware of the conflicting views of the events with which he is dealing and dutifully cites the authors and works that set these forth. Beyond that, his approach amounts largely to providing all the facts he can locate and letting the reader decide. His allusions to turbulent frontiers, poor communications, and the aggression of the notorious "men on the spot" tell us little that is new. Most surprisingly, he sets aside with little comment theoretical frameworks, like that developed by Ronald Robinson and John Gallagher in their essays on the imperialism of free trade, that strike one as so suited to the analysis of the events Pollak describes.

MICHAEL ADAS
Rutgers University

UNITED STATES

JEROME O. STEFFEN. *Comparative Frontiers: A Proposal for Studying the American West*. Norman: University of Oklahoma Press. 1980. Pp. xix, 139. \$10.95.

In this brief volume, Jerome O. Steffen, already well known for his studies of comparative frontiers, suggests a procedure designed to rescue historians of the West from their own "closed society" and return them to the mainstream of historical studies. This can be done, he feels, only if the frontier experience is viewed as a contest between "the demands of the environment and the mindset of those entering each new West." Such recognition would disclose two types of change: "overt," in which the "conceptual foundation remains essentially the same," and "fun-

damental," involving the "replacement or significant alteration of the very assumptions upon which given practices were based." Steffen believes that most American frontiers were "overt"; thus the technological conquest of the Great Plains so glorified by Walter Prescott Webb embraced only a hundred-year timespan and "the so-called victory may represent only a dysfunctional stage of adaptation."

The nature and extent of alteration on successive frontiers, he suggests, could be evaluated by determining the "variables or combinations of variables causing different kinds of change." He suspects that two types would emerge: "cosmopolitan," where interacting links with the past remained dominant and the only changes were modal or caused by non-environmental factors, and "insular," where change was governed by factors exclusive to the frontier process.

Building on these hypotheses, he examines four frontiers—cis-Mississippi agricultural, fur trading, placer mining, and cattle ranching. The latter three he classifies as "cosmopolitan," with interacting links governing alterations and innovation minimal. On the "insular" cis-Mississippi frontier Steffen finds alterations of a sort to challenge modern social scientists: environmental influences of interest to cultural ecologists; the emergence of "identity crises" suitable to study by disciples of Abram Kardiner, Erich Fromm, and Erik Erikson; the socialization of individuals in patterns conforming to the theories of David Riesman; the possibility of model building as successfully explored by Stanley Elkins and Eric McKittrick; the "decline of a deferential citizenry" to substantiate findings of sociologists and students of religion; and many more.

Steffen's thoughtful suggestions should challenge the next generation of frontier historians. He states his case well, and should his hypotheses stand the test of investigation, they should not only help re-establish communication between frontier historians and the larger body of scholarship but also broaden our understanding of the westering process and of the American past.

RAY ALLEN BILLINGTON
Huntington Library

FREDERICK TURNER. *Beyond Geography: The Western Spirit Against the Wilderness*. New York: Viking Press. 1980. Pp. xviii, 329. \$16.95.

This eloquently maudlin book might fairly be retitled "Beyond Reason," for it preaches that devastation has been wrought by the rationality of Christianity that led invading Europeans to miss the great spiritual opportunity presented by the New World. Salvation was and is possible only through

revelation and myth. What happened instead was a manifestation of "spiritual bankruptcy" (p. 262), although it was also, as in his subtitle, "the western spirit against the wilderness."

The book is "an essay in spiritual history," says Frederick Turner (p. 7); and again, "what I wanted to write about was my vivid sense—let me risk calling it a vision—that the real story of the coming of European civilization to the wildernesses of the world is a spiritual story" (p. xi). The word for this in English is theology.

The first third of the book discourses on the suppression by church fathers of ever-refreshing new revelations, and one of the crimes thus committed was "Christianity's turn from myth toward history" (p. 62). For, "regardless of whatever local, temporal excesses may fairly be charged to myth's account, the gigantic and general purpose of archaic myth is the celebration of Life," and "whatever sacrifices" such myths "demand in 'objective truth' are outweighed and justified by the comforts of old truths, old necessities, we carry within us" (pp. 16–17).

The book's middle third takes it into America where its deeper truths permit a chapter on "Defloration" to precede one on "Penetration." There are chapters on Columbus, Cortés, Roanoke, and Massachusetts, followed in the final third by attention to captivity narratives, "white Indians," frontier expansion, land hunger, and dispossession of the Indians, climaxing at Wounded Knee. The method is impressionistic, though informed by wide reading in up-to-date studies, and the colors are black and white. Without distinction and without amelioration, "the monstrous cathedral of European christendom" (p. 209) generates the motives that sent men forth to kill Indians and each other and to seize and defile the countryside, creating in the process such horrors as "the cancerous stretch of the eastern seaboard" (p. 255).

Nevertheless, the book is gracefully, even charmingly written, and much of what it says so sweepingly will please readers disposed against rationalism and materialism. In the narrow sense, factuality is not violated, and the argument is sophisticated. But I will stick to history.

FRANCIS JENNINGS
Newberry Library

G. EDWARD WHITE. *Tort Law in America: An Intellectual History*. New York: Oxford University Press. 1980. Pp. xvi, 283. \$19.95.

One of the most interesting and promising aspects of contemporary legal scholarship is the remarkable growth of interest among law professors in legal history. This brief intellectual history of American tort law, by a member of the faculty of the University of

Virginia Law School, is a splendid example of this development. Classically, tort law is defined as that body of law that is concerned with all civil wrongs not arising from contracts. Actually, this explains very little since there are so many different kinds of torts that the construction of a consistent and useful unified rationale is not possible. G. Edward White's basic theme is that tort law cannot be ascribed, as has often been done, simply to the consequences of industrialization. He demonstrates that tort law has always reflected the changing patterns concerned with the character of knowledge.

Beginning with the collapse of the writ system, White examines the impact of legal science from 1880 to 1910, which was based on the "case" or inductive method of legal study, then the impact of legal realism, from 1910 to 1945, which rejected scientific conceptualism in favor of an objective, empirical, and anti-universalist approach based on a perception of social interdependence. A great deal of attention is devoted to such eminent legal scholars as Leon Green, Francis Bohlen, and William Prosser and such creative appellate judges as Oliver Wendell Holmes, Benjamin Cardozo, and Roger Traynor.

Contemporary thinking about tort law is rooted in what White calls "consensus thought," described as a search for core values around which the makers of law may rally. Thus, recent scholarship regarding tort law is deeply concerned with welfare economics and moral philosophy; all of which treats tort law not as a body of private law but as a public law subject. White concludes that "torts is not a unified subject but a complex of diverse wrongs whose policy implications point in different directions" (p. 233). It follows that "tort law more closely resembles a shifting mass of diverse wrongs than a tidy, conceptually unified subject. Multiple purposes for tort law, multiple standards of tort liability, and individualized determinations of tort claims reflect the innate character of the field" (p. 240). White's analysis of the weighty contributions of academic scholars to the shaping of the law is especially effective, for in many ways contemporary tort law is a product of casebooks, treatises, and law review articles. Except for some unfortunate lapses in proof-reading, this is a splendid work of historical legal scholarship.

DAVID FELLMAN
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Madison

EDWARD W. CHESTER. *The United States and Six Atlantic Outposts: The Military and Economic Considerations*. (National University Publications Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1980. Pp. xi, 260. \$17.50.

This volume will be of no use to the serious scholar, and of little use to anyone else. Edward W. Chester traces the relations between the United States and the Bahamas, Jamaica, Bermuda, Iceland, the Azores, and Greenland, provides a brief introduction to the destroyers-for-bases deal in 1940, and offers a fragmented conclusion. Each of the substantive chapters follows a similar outline. There is a cursory introduction to the early history of each "island outpost," followed by sections dealing with relations between the islands and the United States within the framework of major events in American history: the American Revolution, the War of 1812, the Civil War, World Wars I and II, the post-World War II era. In addition, various chapters have separate sections on United States economic involvement with the islands, liquor smuggling, the tourist trade, the impact of U.S. air and naval bases on island life, scientific expeditions, and assorted other subjects. The concluding chapter summarizes the author's findings, briefly compares U.S. interventions in Cuba, Haiti, and the Dominican Republic with U.S. policy toward the six "island outposts" under study, and offers as fresh insights some threadbare conclusions: the Roosevelt Corollary did not restrain private American investment in the Caribbean and Latin American regions; U.S. opposition to foreign military interventions in the New World did not preclude similar interventions by the United States.

No unifying themes bind the chapters of this volume together; nor is it grounded in substantive research. The author did not consult foreign archival material, and while he utilizes approximately a dozen State Department decimal files, each is cited only a time or two in the footnotes. The book is based largely on newspaper stories, journal articles, and the previous works of other historians. It is a narrative history or, more accurately, a catalog of often disconnected episodes and anecdotes related in a prose that sometimes degenerates into cliché. The author has *The Times* of London "shed[ding] tears," a "tempest in a teapot," "winds of change" that "swirl," and "storm clouds" with "silver linings."

MICHAEL J. HOGAN
Miami University

W. DAVID BAIRD. *The Quapaw Indians: A History of the Downstream People*. (Civilization of the American Indian Series, number 152.) Norman: University of Oklahoma Press. 1980. Pp. xiv, 290. \$19.95.

W. David Baird's excellent history, thoroughly researched and well written, has a familiar ring. Early in the eighteenth century a small Ohio Valley tribe

is driven westward by its Indian neighbors who have secured firearms. After making its own alliance with a European power, the tribe briefly flourishes only to be decimated by the diseases that are the fatal accompaniment of the cherished trade goods. Reduced to a population of a few hundred, the tribe is in no position to resist the new United States government when it pressures the Indians to cede large areas. Nor does the tribe exhibit any enthusiasm for the schools and missionaries that are part of the government's civilization programs. Badly split by their removal experience, late in the nineteenth century they are about to be subjected to the final indignity—allotment in severalty.

At this point the reader suddenly realizes that the story is not taking the usual course. The Quapaws reversed their population decline by freely adopting Indians from other tribes as well as some whites. One of the adoptees, a Stockbridge Indian with an acquisitive instinct that would have done credit to a robber baron, guided the Quapaws to a real coup. Instead of resisting allotment, the Quapaws seized the initiative from the Indian Office and lobbied Congress for it on their terms. In this fashion they divided up the whole reservation and secured 240-acre allotments instead of the 80 to 160 to which other tribesmen were restricted.

A number of whites have roles in the Quapaw story, getting some adoptions approved and others disapproved and expediting the allotment process. They included some of the most imaginative and unethical politicians in Indian Territory, including former governors, former territorial representatives, and former judges, a regular rogues' gallery whose names are familiar to students of Oklahoma history.

When a few of the Quapaw families received allotments rich in zinc and lead, they became the targets of every crooked Indian and white in the area. Baird is particularly effective in telling the story of what he calls these "Poor, Rich Quapaws."

In a belated and not very effective effort to protect them, the federal government reasserted its guardianship. This is the final irony. A tribe in name only, a collection of people of diverse backgrounds of whom only a few have any recollection of Quapaw culture, the tribe will be maintained as a legal entity by this wardship status. Baird closes with a reference to the Quapaw pride in the success their Business Council is having in securing federal grants. Although clinging to the Quapaw identification, these native Americans have taken their place in the mainstream to a degree that would have cheered the mid-nineteenth-century architects of our Indian policy.

WILLIAM T. HAGAN
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Fredonia

RUSSEL LAWRENCE BARSH and JAMES YOUNGBLOOD HENDERSON. *The Road: Indian Tribes and Political Liberty*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xv, 301. \$14.95.

Russel Lawrence Barsh and James Youngblood Henderson claim that American Indian tribes practice a distinct way of life, "the Road," which should be preserved and respected by others who now occupy native land and oppress native liberty. Protection of this Indian tribalism depends upon a doctrine of sovereignty expressed by John Marshall in *Worcester v. Georgia* (1832). Marshall's recognition of native sovereignty, we are told, was consistent with the earlier pluralism of the Founding Fathers. After *Worcester*, the Supreme Court apparently neglected Marshall's concept, with the result that Barsh and Henderson can find no fundamental principles governing Indian law for the past century. They contend that a hodge-podge of expedient, paternalistic and self-serving laws and court decisions resulted, thereby ensuring tribal dependence upon white society.

Much of the book's argument is legal commentary. Classic Supreme Court cases are reviewed, and many neglected documents are examined. According to the authors, the future remedy for past injustices and unprincipled law is to legislatively recognize American Indians as now forming two diverse movements: the ethnic and the political. Urban Indians are described as seeking a distinct ethnic niche within white society, whereas reservation Indians strive for political self-determination as tribes. A tribe is defined as a nonracial body with membership open to anyone who accepts its values. Tribal sovereignty can be secured, Barsh and Henderson believe, by a constitutional amendment providing a special place for Indian tribes in Congress and defining tribal political status with precision. Individual Indians would then be forced to choose between assimilation and separatism.

The book has several strong points. Analysis of the 1880-1900 Supreme Court decisions and the 1934 Indian Reorganization Act is well done, as is a section on the Court's ideological confusion since 1950. Too often, however, there is little feeling for the dilemmas and extralegal forces in human history, partly because Barsh and Henderson neglect important sociocultural research and do not seem to be aware of relevant studies by historians. As a result, they have produced a legal survey that glosses over violence and ignores material power in the conquest of North America. Taken out of this historical context, the U.S. Supreme Court can often appear evil or stupid in its Indian decisions, which was seldom the case.

The authors' analogies are often unconvincing,

their secondary sources too limited, and the book's organization inadequately controlled. Digression, jargon, and biased language will annoy some readers. No bibliography is provided. No significant use is made of native sources; the "Road" described is not that of native Americans but of the Supreme Court and Congress. With these limits in mind, and if one accepts their presuppositions concerning the feasibility of political autonomy for one-half of one percent of the United States' population, Barsh and Henderson provide an instructive excursion through legal doctrine.

ROBERT H. KELLER
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BENJAMIN W. LABAREE. *Colonial Massachusetts: A History*. (A History of the American Colonies.) Millwood, N.Y.: KTO Press. 1979. Pp. xvii, 349. \$17.00.

The volumes in the History of the American Colonies series have filled diverse needs and have emphasized various historical themes. Volumes such as Sydney V. James's *Colonial Rhode Island* and John E. Pomfret's *Colonial New Jersey* draw heavily on the authors' original research to provide long-needed accounts of the histories of neglected states. Other books, including Michael Kammen's *Colonial New York* and Robert J. Taylor's *Colonial Connecticut* serve as syntheses of the literatures of historiographically better-endowed states. Most are predominately political in theme, as befit histories of early America's primary political units, but most also contain chapters on economy, society, and culture. Reflecting the state of social history in the states, the social chapters tend to fit a traditional cultural and daily life mold. Most volumes also discuss the local development of the Revolutionary movement. It is a measure of Massachusetts's prominence in colonial historiography that Benjamin W. Labaree's superbly written book combines the local political history with a sophisticated synthesis of religious and social history, as well as with a full and lucid narrative of some of the critical events of the Revolutionary background.

Labaree retells the familiar story of *Colonial Massachusetts* by building each chapter around a sprightly biographical vignette of an illustrative figure. Descriptions of Massasoit's Indian land of New England and John Winthrop's England introduce the settlement in Plymouth and Massachusetts Bay and the economic and political expansion by which the Massachusetts Bay Colony became synonymous with New England in seventeenth-century minds. A chapter of extraordinary clarity explains the complexities of seventeenth-century Puritanism and fol-

lows the Mather dynasty through the evolution of "Zion in the Wilderness." Samuel Sewall guides the account through the perplexities of imperial integration, the Dominion, and Salem witchcraft to the second chapter and the new century, while the ambitions of the Otis family illustrate mid-eighteenth-century provincial politics. The intellectual and pietistic duality of Jonathan Edwards clarifies the religious and rationalistic tension that characterized provincial culture. Thomas Hutchinson and Samuel Adams provide poles for the narrative of the imperial crisis, and John Adams sees the story through to his great constitutional triumph in the 1780 Constitution.

While the vignettes from Winthrop to Adams bespeak a modern synthesis of the often-told public history, the equal billing accorded Edward Rice and John Parker indicates Labaree's skillful integration of the fruits of the social history boom of recent decades into the larger history. Rice, the yeoman of Sudbury, introduces a chapter that illustrates the centrality of town formation, local social institutions, and the standard of rural living to social and political dynamics in seventeenth-century New England. Parker, the Lexington militia captain, introduces a chapter on the Revolution but symbolizes the integration of ordinary men and social history into the narrative of provincial development and imperial protest. In this book the political divisions of Dedham and the mobility of Andover's yeomen become key components in the telling of the colony's public story.

Precisely because Labaree is so perceptive in synthesizing the secondary literature on Massachusetts, it is possible to see in his account the weak points in the colony's impressive historiography. Most notable is the thinness of the narrative of provincial, as opposed to imperial, politics from the mid-seventeenth century until the better-documented administration of Jonathan Belcher. The author rightly concludes that the towns were fundamental to the political process, but we know precious little about how their people viewed or used the system. And although the intellectual record of the Great Awakening is clear, little agreement exists about how its social aspects intersected with surrounding social and political movements. Until these and other puzzles are solved, however, Labaree has given us a lucid and readable synthesis of the Bay Colony's history.

EDWARD M. COOK, JR.
University of Chicago

RAYMOND C. BAILEY. *Popular Influence upon Public Policy: Petitioning in Eighteenth-Century Virginia*. (Contributions in Legal Studies, number 10.) Westport, Conn.: Greenwood Press. 1979. Pp. xii, 203. \$18.50.

Many petitions were presented to the ruling authorities in Virginia during the eighteenth century. There were requests for the division of counties, for the establishment of tobacco warehouses, for the relocation of parish churches, as well as for the restraint of hogs from roaming in townships and for the prohibition of slaves from keeping dogs. Raymond C. Bailey has studiously sought out all these petitions in legislative journals and state archives and classified them under broad headings such as local government, economy, and religion. No attempt is made to settle conclusively the hoary debate as to whether the Old Dominion was "either 'aristocratic' or 'democratic'" (p. xi), but the purpose of this careful scholarship is stated to be a demonstration that "Virginia citizens were able to play an active and extremely significant role in . . . political affairs . . ." (p. 6).

Few would deny the responsiveness of the rulers of the Old Dominion to appeals of certain kinds from property owners great and small, although the listing of a small number of petitions received from women and blacks does not prove, as the author implies (pp. 41, 44, 45, 46), that the authorities readily received and attended to the appeals of "all classes" upon all subjects.

It is half a century since Lewis Namier's *Structure of Politics at the Accession of George III* (1928) was first published, yet no attempt is made in Bailey's book, even by way of case study, to get beneath the surface and explore the kinds of local contexts in which petitions were formed and the means by which the success of some petitions was secured while others failed. Although quoted documents refer to "Partys and Factions" (p. 143), the author makes the bland assumption that differences between petitioners and counterpetitioners were always settled equitably and not by the superior influence of one group over another.

A brief concluding chapter serves to mitigate, or perhaps to underscore, the limitations of the book. In these few pages topics are introduced that could profitably have informed the earlier substantive analysis. A scrutiny of the role of petitioning in relation to the changing theory and practice of political representation (pp. 172-73) would have been much more valuable than the classification of subject matter into broad categories that cut across more than a century's change and a revolution in the political system. Indeed what is most sorely missing in the book is a concept of political culture or even an application of the notion of "the political process" that is introduced at the outset (p. 19). The lack of any systematic exploration of the relationship between legislative practices and changing beliefs concerning the nature and scope of government is particularly to be deplored in a work appearing in a series entitled "Contributions to Legal Studies."

The awareness of an important profession will be impoverished if historical perspectives do not contribute to a consciousness of the integral relationship of lawmakers to the prevailing social system and ideas of their own age.

RHYS ISAAC
La Trobe University,
Australia

FREDERICK V. MILLS, SR. *Bishops by Ballot: An Eighteenth-Century Ecclesiastical Revolution*. New York: Oxford University Press. 1978. Pp. xii, 367. \$14.95.

In *Bishops by Ballot* Frederick V. Mills, Sr., reinterprets the colonial Church of England's reorganization into the Protestant Episcopal Church. He argues that there took place within American Anglicanism an ecclesiastical revolution that paralleled the political revolution of these same years. The decisive change involved "the substitution and canonization of republican concepts and practices in place of hierarchical ones" (p. xii) within American Episcopalianism.

The study divides neatly into two parts. In the first Mills discusses the state of colonial Anglicanism, paying careful attention to the dispute over the establishment of bishops in America, an issue he considers the central symbol of the internal conflict within the American Church of England. The author's special contribution to this aspect of his subject is his emphasis on the anti-episcopal sentiment of the generality of Anglican laymen and of the majority of the Anglican clergy. He describes a vigorous, expanding colonial Church of England, but one divided into a clerical "High Church" party centered in Connecticut, New York, and New Jersey and a "Low Church" majority of clergymen elsewhere and laymen everywhere.

In the second half of the book he analyzes more closely the reorganization of the church between 1782 and 1789. The churchmen who directed this process, he insists, were American patriots who had supported the opposition to British authority. They were also largely "Low Church" Anglicans committed by their own experience to ecclesiastical self-government and lay participation in church affairs. These Episcopalians implemented republican principles in the formation of the new church constitution. Led by Pennsylvania's William White, they narrowly avoided a schism between the "High Church" Connecticut Episcopalians who supported the consecration of Samuel Seabury and the "Low Church" majority of American Episcopalians. The church that resulted, he concludes, was an authentic creation of the American Anglican religious experience in that it institutionalized lay participation, clerical leadership, and the kind of spiritual

episcopacy for which prerevolutionary American Anglicans had pleaded.

The author has constructed and developed his argument with care and patience. On one key issue he is, alas, too convincing. If Mills is correct in his conclusion that the majority of Episcopalians would have been undisturbed by the failure of the episcopal plan in the late 1780s, then we need a better explanation of why they did not simply eliminate bishops and form the church on a congregationalist or presbyterian model. One of his operating assumptions is that the political environments of the various colonies decisively influenced the attitudes of churchmen toward episcopacy. This leads him, in this reviewer's judgment, to overlook some other, cultural factors.

Having said that, it must be emphasized that *Bishops by Ballot* offers a fresh look at the episcopacy controversy. And its analysis of the formation of the Protestant Episcopal Church makes it the indispensable work on that subject. Mills's work supplements Arthur L. Cross's classic *Anglican Episcopate and the American Colonies* (1902), and it corrects some of the distortions of Carl Bridenbaugh's *Mitre and Sceptre* (1962). Readers will readily understand why Mills's admirable work won the Brewer Prize for 1975 awarded by the American Society of Church History.

GERALD J. GOODWIN
University of Houston

CHARLES ROYSTER. *A Revolutionary People at War: The Continental Army and American Character, 1775-1783*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1979. Pp. xi, 452. \$19.50.

In this thoughtful, highly original volume Charles Royster brings military and intellectual history together by examining the Continental Army in terms of its relationship to the ideals of the American Revolution. The complexity of the subject and the subtlety of the presentation can scarcely be detailed in a fairly short review. Although a number of aspects of the topic are not treated as completely as one wishes, the author performs an important service in opening up new ways of thinking about civil-military relations and the nature of the American Revolution.

The high tide of patriotism during the War of Independence is usually described as the spirit of '76, but Royster convincingly maintains that it is really the spirit of '75—the *rage militaire*, as he calls it—that defined the ideals Americans sought to live and fight by. Royster asserts that these ideals of 1775 constituted the American character at that time—or, at any rate, those traits that Americans most fer-

vently wanted to believe formed their character. And what were those ideals? They were probably best summed up by the word *virtue*, which the patriots might well have defined as including bravery, devotion, morality, and sacrifice.

If it is not so clear where this conception of the American character came from—from Puritanism, or English left-wing whiggery, or the Great Awakening, or a combination thereof—Royster is nonetheless persuasive in his contention that these attitudes of 1775 exceeded the reality of the human condition. Consequently, the Continental Army inevitably fell far short of the revolutionists' expectations for it. The initial fervor, the *rage militaire*, evaporated in the six months or so following Lexington and Concord, never to return, although Royster's evidence indicates that at certain times and places it may have revived more than he seems willing to admit.

To be sure, the Continentals were never the "Army of Israel" that Americans wanted, and their generals were hardly Joshuas. Increasingly the notion of the altruistic citizen-soldier had to be modified as stronger forms of discipline were necessary and as lengthy enlistments were required to fill up the regiments. Correspondingly, fear of the army grew as it seemed to be less a reflection of society as a whole. The public wanted the soldiers' strengths to be the people's strengths, just as it wished a popular triumph rather than the victory of a professional military establishment. And yet, as Royster wisely observes, recent efforts to see the Continental Army of the late war years as little more than a European professional army miss the mark. No one knew that better than the Prussian drillmaster, Steuben, whose sensitivity to the differences between American and European soldiers is admirably described here.

If the public, however reluctantly, acceded to the need for a well-trained, long-service army, it continued to give expression to the ideals of '75, and in time it denied that victory in 1783 was the possession of the army; rather, since Providence was on the patriot side, it belonged to all the people. That was hardly the case, for both the army and the civilian population had at times drifted sharply away from their professed idealism. If the army had its cowards and deserters, its prima donnas and its intriguers, so too did the home front have its hoarders and profiteers, its apathetic and fainthearted souls.

Yet for all their shortcomings, Americans, both civilian and military, had continued to express their ideals. Had they not done so, they would doubtless have fallen even shorter of those lofty objectives than they actually did. In bringing this splendid study to a close, Royster states that had Americans "not widely shared this ability to see beyond material facts and to reinterpret events in order to make

an ideal seem true, they probably would not have sought independence and surely would not have won it when they did."

DON HIGGINBOTHAM
University of North Carolina,
Chapel Hill

J. WORTH ESTES. *Hall Jackson and the Purple Foxglove: Medical Practice and Research in Revolutionary America, 1760-1820*. Hanover, N.H.: University Press of New England. 1979. Pp. xvi, 291. \$15.00.

Dr. Hall Jackson (1739-97) introduced digitalis into the United States as a therapy to correct the malfunctioning of the human heart. His biographer, a professor of pharmacology and experimental therapeutics at Boston University, is eminently qualified to summarize the achievements of this versatile New Englander. Although Dr. J. Worth Estes has published portions of this monograph in numerous journals, he places the accomplishments of this perceptive New Hampshire physician within the more comprehensive scientific milieu of late eighteenth-century America.

In Portsmouth, New Hampshire, Jackson gained a reputation in surgery, cataract operations, and obstetrics. Estes notes the innumerable obstacles confronting doctors who combated smallpox, malaria, and diphtheria. The writer provides colorful details about Jackson's practice; he traces the patriotic doctor's brief experience in the Continental Army, and he includes Jackson's fee table, which reminds one how limited was even a trained practitioner's professional knowledge in this era.

Commenting about the public health of Portsmouth, the author shifts to comparisons of morbidity and mortality data compiled by Jackson from 1774 through 1795 with related studies penned by colleagues for the next twenty-three years. We learn that New Hampshire's population was relatively healthy and that its citizenry "died of the same diseases, and in the same relative proportions, barring epidemics, as did the populations elsewhere in the United States and Europe" (p. 87). We also discover from this wealth of statistics that infectious diseases were the major killers, that the mean age at death was thirty-four years, and that youngsters until the age of ten faced awesome odds against surviving the ravages of pestilence.

Realizing that nearly 5 percent of his patients suffered from dropsy, Jackson sought a remedy. To explain the state of contemporary beliefs about the pathogenesis of heart disease, Estes astutely traces the evolution of thought, through the Enlightenment concerning hemodynamics and the pathophysiology of the cardiovascular system to the publication in 1785 of Dr. William Withering's *Account*

of *Foxglove*. The English physician demonstrated the therapeutic benefits of digitalis (extracted from the foxglove plant) for victims of dropsy, or today's edema, which results from an excessive accumulation of fluid in bodily organs due to a sluggish heart. Properly calibrated, digitalis increases the amount of calcium in the heart muscle and stimulates it to function more efficiently. As increased blood circulates through the kidneys, the accumulation of fluids in bodily organs is drawn by osmosis back into normal circulation and is removed by an increased urine discharge. While several British scientists soon qualified and quantified Withering's findings, some Americans also experimented with this "wonder drug." But it was Jackson who first corresponded with Withering about foxglove in 1789, who championed the efficacy of the treatment, and who popularized its use in North America.

This is a fine book—it is quite readable, crammed with information about the prevalence of serious diseases and about the state of the medical profession in the early Republic. Its copious footnotes attest to the author's diligent search for fresh source material. The work is not the typical biography of a famous man—for we discern relatively little about Jackson himself—but it is an intellectual history of how theories about heart disease were disseminated in Europe and transmitted across the Atlantic. The only minor flaw in this thoughtful study is the omission of material about the level of pharmacology in Jackson's day. Dr. Estes merits high praise for his rewarding study of a significant figure in American medical history.

RICHARD L. BLANCO
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Brockport

CHARLES W. AKERS. *Abigail Adams: An American Woman*. (Library of American Biography.) Boston: Little, Brown. 1980. Pp. x, 207. \$9.95.

While the choice of Abigail Adams as one of the first two women (Elizabeth Cady Stanton is the other) to be included among the two dozen volumes in the Library of American Biography can only be applauded, 192 pages of text simply cannot do justice to a life and personality as complex as Abigail Adams's. The reviewer sympathizes with Charles W. Akers, who had to write within a size limitation imposed by the series. He could have done more justice to his subject if he had not had to compress a multi-dimensional life into so few pages. Abigail's relations with her children or sisters alone would each fill a larger book.

There are glimpses of the real Abigail in this admirably summarized, clearly written biography

based on her copious correspondence. Yet she cannot come alive. The effervescence and spontaneity of her spirit, her proud love of her offspring, the unique flavor of her speech and humor are lost in indirect quotations. Her intellectual curiosity is here, but one must know what to look for. Furthermore, historical personalities who shared the stage with her—Washington, Franklin, Jefferson, Hancock, Mercy Otis Warren, and countless others, including John Adams—are but silhouettes passing in the shadows. Even the gestation and birth of the United States is hazily sketched in.

In her letters, Abigail bristles with emotions, longing for her mate day and night, missing him physically, spiritually, and intellectually. No woman could believe that viewing John Adams's portrait would provide Abigail with "a measure of emotional compensation" for the loneliness of "two decades [sic]" (p. 83). Only another woman could sense the true anguish of the sensitive Abigail left for months without news of her husband and sons. A woman would also not neglect to detail the orders Abigail kept sending to her husband abroad. Although they might seem trivial to men, they were practical with an eye on the marketability of laces, gloves, and yard goods. Abigail's black marketeering casts a dubious shadow on her patriotic principles, but it paid the war taxes and kept her family alive, while in the absence of John she ran the farm, dabbled in real estate, and conducted her own political campaigns in his interest.

Although Abigail had liberal ideas about the status of women, she was not, according to the author, a feminist. The "Remember the Ladies" phrase, often quoted out of context, was an isolated plea for the legal safeguard of wives against abusive husbands. She dropped the subject when John considered it a joke. When she met a truly liberated woman, Franklin's lady friend, she recoiled from her too-liberated display of behavior toward Franklin and John. Squeezed into the straitjacket of her milieu, Abigail could not become an open advocate of equality between the sexes.

Although this is a scholarly treatise rather than a lively biography, it is devoid of documentation because of series policy. There is an excellent "Note on the Sources," but the index is skimpy and neglects Franklin entirely, perhaps out of sympathy with Abigail and John Adams, who had no love lost for the "electric ambassador."

Akers is the respected author of *Called Unto Liberty: A Life of Jonathan Mayhew* (1964), but the sensuous, vibrant, intellectually eclectic Abigail, a mosaic of passionate woman, dedicated mother, pioneer for women's rights, calculating businesswoman and farmer, and by no means guileless politician, was not his cup of tea. He did not get under her skin; he observed her from the outside. Nevertheless, he

should be handed laurels for a masterful condensation. The book fulfills the purpose of the series and would make informative collateral reading for high schools and colleges.

VERA LASKA
Regis College
Weston, Massachusetts

WILLIAM M. FOWLER, JR. *The Baron of Beacon Hill: A Biography of John Hancock*. Boston: Houghton Mifflin. 1979. Pp. xiii, 366. \$15.00.

This popularly written trade book will please the general reader, but offers little for the serious student. It is a conventional biography that sketches a fairly accurate picture of the often inscrutable John Hancock, the Massachusetts merchant and revolutionary. Its strength is that it dispenses with most of the myth, avoiding Herbert S. Allan's overly sympathetic view at the same time that it rejects James Truslow Adams's equally biased perception of Hancock as "an empty barrel." William M. Fowler's judgments are good, if not highly original. Like most recent historians of revolutionary Massachusetts, he finds no evidence that Hancock was simply a "rich empty-headed dilettante" (p. 56) who had fallen into "the sinister clutches of Sam Adams" (p. 60).

Fowler's judgments, however, sometimes rest on a less than thorough examination of conventional literary sources. He relies heavily on Hancock's personal papers. Those published, as experienced scholars know, reveal remarkably little about the man. The unpublished, although extensive (21,000 items in the New England Historical and Genealogical Society alone, according to a recent advertisement), seem to provide Fowler with no significant breakthroughs. The observations of contemporaries are thus crucial, and yet Fowler makes no use whatever of Governor Francis Bernard's unpublished papers, the most extensive commentary on Massachusetts politics in the 1760s, and very limited use of Thomas Hutchinson's papers. Granted, in both cases, the sources are sometimes fiercely biased against Hancock; but nearly every contemporary who wrote extensively about Hancock was critical of him, a notable fact with which Fowler does not deal.

Working from thin resources, Fowler is forced to familiar conclusions. Hancock lacked the intellectual power and depth of Adams and Otis, and "wore his political ideology much as he did his religion, as a cloak" (p. 136). He was both an aristocrat and a democrat whose rejection of rigid ideology made him flexible and adaptable. By lying low during controversy, he could emerge as the healer, drawing conflicting factions towards a consensus as

he did in the aftermath of Shays's rebellion and again with the ratification of the federal constitution. "Perhaps more than any other of his contemporaries," Fowler concludes, "he deserves to be enshrined as America's first modern politician" (p. 280). These are sound observations that fall tantalizingly short of satisfying the curious reader. What kind of society was it that chose its wealthiest member as a symbol of revolution? Perhaps this and other such questions cannot be answered in a biography. Yet, given the meager yield from Hancock's papers, one may wonder whether Hancock can ever be properly served by a conventional biography. Hancock's success as a politician may well be better shown against a more extensive historical analysis of Massachusetts society and politics, the complexities of which are barely suggested in this book.

STEPHEN E. PATTERSON
University of New Brunswick

ROBERT DAWIDOFF. *The Education of John Randolph*. New York: W. W. Norton. 1979. Pp. 346. \$17.95.

Of all the political figures of postrevolutionary Virginia, none has left a more vivid imprint than John Randolph. Paradoxical and enigmatic, he has defied the arts of past biographers. He will probably never be completely accessible to historians, but Robert Dawidoff has come closer than any previous writer in making Randolph's career, personality, and political ideas intelligible. His study is not a full-scale biography—others have written adequately about Randolph's political concepts and his congressional career. Nor is it a psychobiography except in a limited sense, for Dawidoff rightly insists that excessive reliance upon psychoanalytical techniques removes the subject from his place in time and projects a modern context that distorts the reality.

Dawidoff has sought the key to understanding Randolph in terms of his education, which comprises his family upbringing as well as his cultural and political education. All three were complementary to each other and uniquely suited his character. Dawidoff effectively demonstrates that behind Randolph's eccentricities (even at their most fantastic) there was a kind of consistency of thought and action relevant to his social milieu. Dawidoff's analysis of Randolph's conduct and ideas can be best summed up in the phrase that Randolph was guided by a "vision of America compounded of nostalgia for the Revolutionary and English past as well as for a *personal* past" (p. 210). It was a world more imaginary than real. His conservative views and his admiration for the values of a Virginia rooted in the colonial past were shaped in his child-

hood by his widowed mother to whom he was deeply attached. This stable world was shattered in his early twenties by the illness that rendered him impotent and by the scandal surrounding his elder brother, who died when Randolph was twenty-five.

His reading, for his formal education was sporadic, tended to complement this private sense of loss. He was most influenced by the early eighteenth-century English writers, largely of a Whig or "country" persuasion—writers who saw a world in decline and whose chief weapon was satire. Their wit, their bitterness, and their habitual opposition perfectly suited his temperament. They became his models and colored his public style that was so devoid of the then-favored legalistic mode of argumentation. The only contemporary writer for whom he felt an affinity was Lord Byron, who, like Randolph, was locked in an unremitting protest against the social standards of his time. This offers an important clue for understanding Randolph's behavior. Admittedly eccentric, at times unstable, and perhaps even mad, there was apparent a self-conscious dramatization in most of Randolph's actions. His very aberrations were half cultivated as a means of registering his condemnation of the corrupt and decadent world of American democracy. Nothing stimulated him to rise to peaks of vituperation or extremities of behavior more than the presence of an audience. Dawidoff has successfully shown that Randolph used his personal style as a means of stating his disillusionment with the values of American society in his own day.

HARRY AMMON
*Southern Illinois University,
Carbondale*

RICHARD M. CLOKEY. *William H. Ashley: Enterprise and Politics in the Trans-Mississippi West*. Norman: University of Oklahoma Press. 1980. Pp. xiii, 305. \$18.95.

The broad pattern of William H. Ashley's role in the Rocky Mountain fur trade is well-known to historians of the American West. Never before has his trading activity been so carefully analyzed and integrated with other facets of his career as in this first full-length biography. Ashley, born in obscurity and poverty, abandoned his Virginia home at twenty years of age and headed west in 1798, ending his migration in Missouri. Here he became an entrepreneur in the lead mines, engaged in trading and land speculation, experimented with the making of gunpowder, joined and commanded the militia during the War of 1812, attaining the title of "General." Moving to St. Louis, he was enmeshed in politics, elected lieutenant governor, married,

and caught up in the life of the community. Forming a partnership with Andrew Henry, he resolved to break the established patterns of the fur trade by using hired men to replace Indian hunters. The first two seasons were disastrous, first with the supply ship sinking in the Missouri River and second with the Ree Indians attacking and blocking the accustomed movement of traders up that stream. This development proved a blessing in disguise because Ashley resolved to abandon the Missouri River system, send caravans overland, work the mountain streams, and, in time, substitute the rendezvous for the fixed trading post. After being defeated for the governorship, Ashley headed west and personally directed operations in the field in 1824–25 and at last attained the financial success he had been seeking. Andrew Henry had already retired, in disappointment, as Ashley's partner in the fur trade. At this juncture Ashley formed a new partnership with perhaps the ablest of his hired men, Jedediah Smith. In 1826, Ashley sold his interests to Bill Sublette and David E. Jackson, who with Smith formed the Rocky Mountain Fur Company. Henceforth, Ashley acted as supplier, banker, and agent for this company and its successors.

Ashley became a self-appointed publicist for the West and gained the reputation in government circles as both an informed and an effective expert. Although nominally a Jacksonian in politics, his ties with the business interests in St. Louis led him to support the national bank, thereby casting doubt on his party loyalty. In Missouri he attempted to follow an independent political course between the entrenched Democrats and rising Whigs. In 1831, he was elected to Congress, but gained the enmity of Thomas Hart Benton whose forces tried in vain to unseat him for the next five years. Never a skilled debator, Ashley failed to gain national prominence while Congress struggled with the great issues of the day—the national bank, nullification, land and transportation policy, and abolition agitation. Instead he worked for the interests of his constituents, seeking to extend the National Road to Missouri, extinguishing Indian titles to land, and promoting river clearance. While still a member of Congress, he ran for the governorship of Missouri in 1836 and was defeated by a unified Jacksonian party. He was moving toward affiliation with the Whigs when death came suddenly in 1838.

Perhaps Richard M. Clokey's greatest contribution is delineating the intricacies of Missouri politics in the 1830s, analyzing Ashley's role in Congress, and describing his personal and social life with three wives. Objectively he notes Ashley's weaknesses as well as his strengths in this long overdue biography. It is ironic that Ashley, who launched the careers of many a young mountain man already immortalized by biographers, novelists, and their

own memoirs, should wait so long to be rescued from comparative obscurity.

The research for this study has been wide-ranging and thorough. The notes, mostly to manuscript collections, provide testimony. The author has also ably utilized the research of other scholars interested in Ashley, notably Dale Morgan. The bibliography alone is a treasure.

W. TURRENTINE JACKSON
University of California,
Davis

ELBERT B. SMITH. *Francis Preston Blair*. New York: Free Press. 1980. Pp. xv, 481. \$15.00.

It is extraordinary that with all the scholarship devoted to the Jacksonian era over the past thirty-five years so little of it has been directed at the men who were the Hero's closest friends and advisers. Van Buren, Livingston, Taney, Kendall, Blair, Hill, Eaton, Lewis, Donelson, Overton, Coffee, and virtually all the members of Jackson's cabinet (with the notable exception of Louis McLane) lack modern scholarly biographies. The newest generation of historians apparently prefers to count Presbyterian churchgoers from 1830 to 1850 in Jacksonville, U.S.A. than grapple with the lives of the men who tried to steer the nation's attitudes on banking, the tariff, internal improvements, Indian removal, and the like.

Elbert B. Smith has attempted to remedy this lamentable situation with respect to at least one of the leading Jacksonians. His biography of Francis Preston Blair, founder and editor of the *Washington Globe* and an influential voice in shaping and propagating the Democratic Party line, is complete, useful, widely researched, and pleasingly written. Among other things it provides a great deal of valuable information about the establishment of the *Globe* not generally known and not presently available in any other printed source. The chapters concerned with the Bank War are particularly helpful.

Despite these merits and the satisfaction of having at last a one-volume study of a major personality of the middle period, the book is a disappointment. The principal fault is the lack of a detailed analysis of Blair's participation in the major events with which he was directly involved. Blair's journalistic prowess and the influence he exerted on Jackson and the Democratic Party are not fully revealed or explored; too much is glossed over. The author apparently feels it is enough to quote a few sharply worded editorials to explain Blair's success as an editor. In addition, the scholarship is sometimes sloppy. One paragraph presents Kendall's description of Jackson's first inaugural woven from quotations from several letters and newspaper accounts,

and the footnote simply states: "From January through June 1829 Kendall wrote Blair at least twice weekly" (p. 443 n. 10). Moreover, the discussion of the founding of the *Globe* is necessarily incomplete without reference to Kendall's long and important letter to Blair dated November 20, 1830, which provides significant details on the production and distribution of the newspaper.

Further doubts about the book are aroused when the author undertakes to inform the reader what Blair "would have" thought about the various interpretations of the Jacksonian era advanced by modern scholars. According to Smith, Blair "would have been both delighted and puzzled" by Schlesinger's *Age of Jackson* (p. 113), "would have agreed fully with John Ward's explanations" (p. 115), and "would have respected Edward Pessen's willingness to let opposing viewpoints speak for themselves" (p. 116). Smith carries this on for several pages and manages to include most present-day historians. But unlike Blair, I think, Smith has something agreeable to say about all of them. It is difficult to fathom what possessed the author to engage in this questionable exercise.

It would be unfair and a disservice to close this review on a negative note. Let it be clearly recorded that there is much to admire in this biography, particularly the amount of useful information it provides. Furthermore, it not only offers a lively account of an extraordinary man but also a portrait of a unique family and a dramatic narrative of a colorful political era in American history.

ROBERT V. REMINI
University of Illinois,
Chicago Circle

CHARLES H. BROWN. *Agents of Manifest Destiny: The Lives and Times of the Filibusters*. Chapel Hill: University of North Carolina Press. 1980. Pp. xi, 525. \$25.00.

After a chapter introducing early nineteenth-century prototype filibusters Francisco de Miranda and Aaron Burr, Charles H. Brown has written a history of filibustering from 1848 until 1860. His story intertwines personalities, dramatic events, and anecdotes. He refers to possible themes such as slave expansionism, financial, merchant, and capitalist adventurers seeking fortunes, and idealists seeking to export American concepts of freedom, liberty, and democracy, without, however, pursuing any at length. Finally, undermining his few references to the broader significance of his study, Brown claims that social forces were not important—filibustering boils down to personality.

Most of Brown's tales are well known; despite the dust jacket reference to "thoroughly researched contemporary materials" and a moderately extensive

list of manuscript collections (which are seldom cited), this book is not based upon new original research. The manuscript holdings listed do not include such obvious materials as the State Department consular and diplomatic records, Navy Department records, British Foreign Office materials, and various private holdings such as the John Quitman, William Cazneau, and John Heiss collections, to name only a few. Since half of the text draws very heavily on William Walker's autobiographical account, Brown finds himself in a dilemma. When discussing his sources (pp. 442-44), Brown credits Walker with remarkable objectivity, while in the text he constantly observes the self-serving, deceptive, and deceitful character of Walker's narrative (pp. 200-04, 279-81, 288, 314). Important secondary studies relevant to filibustering, Southern expansion, British policy in Central America, including works by Eugene Genovese, Robert May, Wilbur Jones, and Kenneth Bourne, are not in the bibliography or notes.

When Brown does address himself to the serious broad questions involved (pp. 18, 41, 460), he refers neither to the history nor the historiography of such matters as slave system expansionism, commercial expansionism, or the role of the Monroe Doctrine. His indecisiveness often leads him to tell conflicting tales of personalities or events with no effort to resolve the issue (pp. 177-78, 197-99, 202-04, 324-25, 460). Finally, Brown assumes the "traditional" view that filibustering died with the Civil War, which by implication ties it to slavery. In fact there was further filibuster activity in the 1860s and after in Mexico, and at the turn of the century in Central America, where Yankee soldiers of fortune (Machine Gun Molony, Tex O'Reilly, Hustler Burns, Tracy Richardson, Sammy Dreben, and Lee Christmas) served political factions and business interests. Nonetheless, to recognize filibustering's continuation also suggests that slavery was not its necessary cause but rather that its roots were deeper and more enduring in American relations with the world. Were it not for the price, the book might well appeal to the interests of "military and popular history buffs," but it will hardly appeal at any price to "professional historians" who seek to understand the whys of filibustering.

THOMAS SCHOONOVER
Lafayette, Louisiana

ALBERT CASTEL. *The Presidency of Andrew Johnson*. (American Presidency Series.) Lawrence: Regents Press of Kansas. 1979. Pp. viii, 262. \$15.00.

It is a pleasure to welcome a book about Andrew Johnson to which the adjective "balanced" can be fairly applied. This study makes no claim to origi-

nal research (among unpublished sources only the presidential papers appear in the bibliography), but Albert Castel has studied the voluminous literature on Reconstruction thoroughly, patiently, and judiciously. Rejecting the absurd estimate of Johnson as a "near great" president and the equally unhistorical judgment that he was malignant, unscrupulous, and guided by an overpowering racism, he concludes that the presidency must be judged a failure but that "Johnson was far from being altogether wrong about Reconstruction, and the Republicans were far from being altogether right" (p. 230).

He argues that Presidential Reconstruction could have provided the basis for an enduring settlement. At the close of 1865 Johnson believed himself to be, and in fact was, on the verge of success but then ruined himself and the policy in which he believed by ineptitude in the use of power. Accepting the retrospective judgment of Gideon Welles that Johnson's fatal error lay in seeking to secure himself a further four years in the White House, Castel places more emphasis than some historians on driving ambition as an explanation for his more questionable decisions. This is, for instance, offered as the principal explanation for the wholesale issue of pardons in the fall of 1865; foreseeing that he would not be the nominee of the Republican Party in 1868 and realizing somewhat tardily the weakness of Southern unionism, Johnson planned to create an alliance between restored Southern leaders, conservative Republicans, and Democrats. Later he argues against the contention that Johnson ought to have endorsed the fourteenth amendment as a matter of political expediency, for this is "to ask for a Johnson who did not believe in white supremacy, who did not cherish states rights, who was willing to concede victory to his enemies, and who did not have political ambition. No such Johnson existed" (p. 105). There are other explanations for both incidents, but it must be conceded that none can be proved.

With a president who wrote so little and confided in few, a good deal of informed guesswork is permissible; but that being so Castel might have ventured some hypotheses to explain Johnson's apparent indifference to the plight of white Southern Unionists and his failure to use, in controversy with Congress, his real knowledge of the practical difficulty of making the South an ordered society if all members of the old ruling class were driven from public life. Castel may also be criticized for failing to discuss Johnson's decision to check and then reverse the distribution of land to freedmen. Final balance is achieved, however, in a chapter on "Johnson before the bar of history" and in an excellent bibliographical essay. A bonus not usually found in histories of Reconstruction is the dis-

cussion of two not unimportant events: the acquisition of Alaska and the initiation of an Indian policy aimed to civilize and protect.

WILLIAM R. BROCK
University of Glasgow

RALPH J. ROSKE. *His Own Counsel: The Life and Times of Lyman Trumbull*. (Nevada Studies in History and Political Science, number 14.) Reno: University of Nevada Press. 1979. Pp. 232. \$7.50.

This is number 14 of the Nevada series and the first to treat a subject of larger scope than state history. It is also the third "life and times" account of Lyman Trumbull. The first appeared in 1913 by editor and political ally, Horace White; the second in 1965 by Mark Krug of the University of Chicago. Ralph J. Roske after thirty years of a busy life in teaching, administration, and publication now publishes the subject of his doctoral dissertation.

The editors of the Nevada series were so strict in space limitations that Roske had to cut furiously from his original manuscript, which once was three times the size of the present volume. The result is a mixed blessing. Roske cut mostly "the times" out of his work and left Trumbull pretty well intact, a definite blessing, but stylistically the work is choppy and occasionally the flavor of old interpretations remains. One wishes that Roske had used the editors' limitations to confine himself still more exclusively to Trumbull the man, particularly since Krug wrote such an excellent life and times account.

Krug's title, "Conservative Radical," and Roske's "His Own Counsel" show agreement on Trumbull's political independence. Krug, however, pictured Trumbull as a moderate who often worked with Fessenden, Dixon, Doolittle, and Democrats to thwart radical measures and to control Congress. Trumbull's controversial vote for Johnson's acquittal fits in with this interpretation.

Roske portrays a more radical politician. He stresses Trumbull's active role in drafting the two confiscation acts; pressing for Scott's retirement; supporting the Peninsula campaign investigation, the Freedmen's Bureau, the Civil Rights Act, confiscation of abandoned lands for blacks, and low tariffs; and his opposition to big business. In marked contrast to Horace White's old biography, Trumbull is usually at odds with Lincoln rather than in agreement with him. Trumbull's demands for equal pay for equal work, his influence on William Jennings Bryan, and his support for the Populist party round out the radical picture. It is a fairly convincing argument.

Roske's most interesting contributions are his comments on the intimate side of Trumbull's life. Here we see a more ambitious politician than ever

before presented, one with expensive tastes and a love for high living. We see him ably and devotedly assisted by a down-state editor, George T. Brown, a heretofore unrecognized force in the Trumbull political organization. Trumbull was famous for obtaining patronage favors; now we see him cashing "his checks of political support" (p. 91). We get, also, a much larger view into Trumbull's family life.

A life of Lyman Trumbull is particularly difficult to write because the largest pertinent manuscript collections contain only letters to Trumbull. By prodigious work and long study, Roske has overcome this handicap and has produced the most authoritative work on Trumbull to date.

JAMES G. SMART
Keene State College

LAWRENCE N. POWELL. *New Masters: Northern Planters During the Civil War and Reconstruction*. (Yale Historical Publications, Miscellany, number 124.) New Haven: Yale University Press. 1980. Pp. xiv, 253. \$15.00.

Lawrence N. Powell's monograph on Northern planters in the South during the Civil War and Reconstruction is not only an important addition to Reconstruction literature but also a demonstration as to how American history should ideally be written. Following the example of Willie Lee Rose, who investigated the wartime experiment of growing Sea Island cotton with free labor under the direction of white Northerners, Powell undertakes to examine a much larger subject, the cultivation throughout the South of staples, mainly upland cotton, by Northerners during and immediately after the Civil War. Powell's thorough and imaginative research reveals a story of high adventure worthy of the Elizabethans.

Powell discovers that several thousand Northern former soldiers and civilians plunged into upland cotton growing after Appomattox. Hypnotized by high wartime prices, some army officers and civilians bought or leased plantations while others formed partnerships with Southerners to work the latter's plantations. For a time impoverished Southern landowners generally welcomed the influx of Northern would-be planters. In that period both Northern and Southern whites believed that the freedmen would work more willingly under the management of Northern whites than they would for their old masters. The new Northern cotton growers also believed that they could revolutionize Southern agriculture with superior Northern implements and farming methods. Consequently, the Northerners entered the cotton business expecting to become wealthy within three or four years.

All but a few of the Northern planters were

quickly disillusioned, according to Powell. The freedmen who wanted farms of their own worked without enthusiasm for their Northern employers. Worse still, cotton prices declined disastrously after 1867, and cotton crops were repeatedly damaged by unseasonably bad weather and by invasions of army worms and other cotton pests. Finally, federal taxes on cotton weighed as heavily on Northern planters as on former Confederates.

As prospects for large mutual profits dimmed, relations between Northern and Southern business associates turned sour. When many of the Northern planters became active in the various state Republican parties during the period of Congressional Reconstruction, local tolerance for them was replaced by hatred, and some discovered that their lives as well as their fortunes were in peril. By the mid-1870s most of the Northern cotton growers had left the South poorer than when they arrived. Powell reaches the conclusion that relations between the Northern planters and their Southern neighbors would have gradually improved through the years rather than deteriorating had the Northerners and their Southern associates been financially successful.

Anyone interested in Southern history, whether professional historian or casual reader, will find Powell's book difficult to lay aside until it has been read from start to finish.

JOHN HEBRON MCORE
Florida State University

LEON F. LITWACK. *Been in the Storm So Long: The Aftermath of Slavery*. New York: Alfred A. Knopf. 1979. Pp. xvi, 651. \$20.00.

We often forget that the Civil War began a social revolution in the South. If the Northern victory brought neither political democracy nor social equality to the region, it did end slavery, thereby depriving the planters of much of their wealth, but, more important, destroying the slave-based social and economic system. For the four million blacks, the Yankee invasion and victory meant freedom, a condition they soon learned would have real meaning only to the extent that they were able to give it such meaning. In this richly documented, beautifully written, and sensitive book, Leon F. Litwack describes "the countless ways in which freedom was perceived and experienced by the black men and women who had been born into slavery and how they acted on every level to help shape their condition and future as freedmen and freedwomen" (p. xii).

Readers seeking an analysis of the changing federal policies towards the freedmen, the origins of the new free labor system, the political and economic impact of reconstruction, or of the development of

black leadership will not find it in this book, for such analyses are not what Litwack intends. Yet anyone interested in these and similar problems cannot ignore this book because it contains much that is relevant—indeed, indispensable—for such analyses.

What Litwack intends—and achieves admirably—is to describe the exhilarations, fears, expectations, and uncertainties of a people who found themselves free after generations of enslavement. It is a story of a people who had learned to survive as slaves subject to the will and the whim of their owners. Survival had always required accommodation to the will of the master and a struggle to maintain a semblance of dignity and to carve out a meaningful life within the confines of slavery.

Emancipation lifted the burdens of slavery and, the blacks soon learned, replaced them with the burdens of freedom, burdens that weighed heavily on the poor everywhere in the nation but were especially heavy for a people set free with neither wealth nor property and despised and feared by their former masters and often by their Northern liberators as well. Survival required new forms of accommodation, even as emancipation opened new opportunities.

Litwack's study is truly history from the bottom up—not in the form of cold aggregated statistics that are so characteristic of this genre, but in the form of descriptions of the actions, the thoughts, and the words of the common folk as they struggled with themselves and with their friends and enemies to make freedom meaningful. Reconstruction—political, economic, and social—cannot be understood apart from such struggles. Litwack gives detailed attention to each of the questions that historians have long considered—the initial response to emancipation, the efforts to reunite families, the struggle to achieve economic and political independence, the importance placed upon education, the emergence of black leadership, and the gradual evolution of a new ethnic consciousness under new conditions. Each is presented from the point of view or, more precisely, from the points of view of the blacks themselves. With remarkable ingenuity Litwack teases information from a vast array of sources, including planters' records, visitors' impressions, W.P.A. interviews, government documents, and secondary literature.

Some readers might object that Litwack is too long on detail and too short on generalization and analysis. He usually follows every discussion of one kind of response by a discussion of another, often contrary, response and then by variations of all kinds. He makes no effort to judge which responses were typical, which attitudes most common. It is the variety, the diversity of responses that Litwack wants his readers to see and appreciate.

Nevertheless, within this description of variety lies the underlying theme of the black quest for self-definition as a free people. Litwack writes that the struggle for education and the establishment of separate churches "reflected a growing if not fully developed sense of community and racial pride, even as it sharpened the separation from and accentuated the differences with both their northern friends and native whites" (p. 500). Like W. E. B. DuBois, whom he quotes approvingly, Litwack describes the two conflicting souls within the body of the black as he strives for self-definition in a society that is at once his own and alien. The book's final chapter describes black political organization and activities, but its title, "Becoming a People," summarizes this theme, a theme that gives this massive but always absorbing book coherence and makes it a unique and important contribution to our understanding of the meaning of the social revolution brought by emancipation.

HAROLD D. WOODMAN
Purdue University

ROBERT F. ENGS. *Freedom's First Generation: Black Hampton, Virginia, 1861-1890*. Philadelphia: University of Pennsylvania Press. 1979. Pp. xx, 236. \$15.00.

The signal contribution of this account of how blacks responded to freedom is that it keeps the focus on the significant strides taken by Hampton blacks in the thirty years after 1861 to escape the encumbrances of illiteracy, poverty, and accommodation to whites imposed by slavery. At the same time, Robert F. Engs tells the familiar story of how black progress was impeded by maltreatment at the hands of federal agencies, the prejudices of Northern missionaries, the accommodationist philosophy of Hampton Institute, and, finally, the hostility of Southern whites.

When black refugees began pouring into Hampton from surrounding plantations at the onset of the Civil War, the town and nearby Fortress Monroe became a focal point of military and missionary activities to aid blacks. Early in this contact, a fundamental tension emerged between white views about what was best for blacks and the determination of blacks to define the goals of freedom for themselves. Engs is critical of the missionaries for attempting to impose their own middle-class social norms and denominational religious views upon blacks. Yet he stresses that the freedmen, especially those from the upcountry plantations in the interior, resisted the blandishments of the missionaries and held on to their accustomed social and religious practices. Ironically, those blacks native to Hampton, having experienced a much milder form of slavery than

most of the refugees and having been more closely associated with whites, became purveyors themselves to their "less enlightened" fellows of the values of hard work, sobriety, and thrift.

At the end of the war Hampton blacks sought to obtain an education, a good job, land, and the political power to defend their freedom. Their initial optimism gave way to disillusionment but not despair as they confronted a shift in Northern white attitudes toward viewing blacks as not just different but inferior. The logical application of these views by those who wanted to assist blacks was to develop teacher training institutes to equip black leaders to serve as civilizing agents among the masses of freedmen.

Out of such assumptions Hampton Institute was born, and its first principal, General Samuel Chapman Armstrong, indelibly stamped it with his own educational philosophy for blacks. Wittingly or not, however, Armstrong also helped to develop a partnership between his school and the black elite in Hampton, which Engs credits for much of the progress that blacks made locally. The leaders in the black community at Hampton rejected Armstrong's paternalism and the limited opportunities of vocational education as they used the Institute to serve their own purposes. Ironically, about one in ten of the Institute's graduates went on to Northern schools to pursue a profession—usually with Armstrong's blessings and frequently with his intervention on their behalf. Equally ironic, Hampton graduates who remained in the town generally became active politically and assumed an assertive stance in local race relations, frequently to the benefit of the Institute.

The story of black Hampton is intriguing, though blacks there responded to freedom in much the same way as those described in the accounts of Willie Lee Rose, Joel Williamson, Peter Kolchin, and Leon Litwack. Engs's use of "romantic racialists," a term borrowed from George Fredrickson, to describe the tendency of missionaries to judge blacks by middle-class standards is unfortunate; it ignores the class distinctions, documented in this study, that shaped behavioral norms even among blacks. The unique feature of the black response in Hampton, according to Engs, was the success with which "much of the Northern plan for Southern reconstruction worked there" (p. 200). He attributes this success to the peculiar economic, demographic, social, and political features of Hampton.

Based on solid research, conceptually clear, and well written, Engs's study is a sympathetic treatment of Hampton blacks that fits their lives into the broader human strivings of a tumultuous era.

JOSEPH H. CARTWRIGHT
Murray State University

JAMES OLIVER HORTON and LOIS E. HORTON. *Black Bostonians: Family Life and Community Struggle in the Antebellum North*. New York: Holmes and Meier. 1979. Pp. xv, 175. Cloth \$17.50, paper \$8.95.

ELIZABETH HAFKIN PLECK. *Black Migration and Poverty: Boston 1865-1900*. (Studies in Social Discontinuity.) New York: Academic Press. 1979. Pp. xviii, 239. \$17.50.

Taken together, these two books provide an overview of a century of black life in the city of Boston, covering economic conditions, families, community and religious organizations, and politics. This is intensive coverage of a small and not particularly representative group of people. Boston blacks numbered only 2,261 in 1860, and the city was rather removed from the main routes of black postbellum migration (which in any case was not large in the aggregate before 1900). In both cases the size allows the authors to take a 100 percent sample of the black population, tracing the fortunes of individuals and households from census to census. Both before and after the war, the studies find that even in the liberal city of Boston, blacks suffered from discrimination and were heavily concentrated in menial and unskilled jobs. From the perspective of the national black population, however, the Boston group was something of an elite.

James Oliver Horton and Lois E. Horton, who are respectively historian at George Washington University and sociologist at George Mason University, pay some attention to developing a quantitative description of occupational levels, literacy, residential concentration, and behavioral patterns such as marriage, migration, and taking in boarders. Their chief concern, however, is demonstrating the existence and vigor of the antebellum black community in Boston, particularly the tradition of concerted social protest. To this end they cite an impressive array of sources, such as petitions, political tracts, membership lists, antislavery society minutes, and municipal records. They identify both formal and informal associations, from churches and debating societies to gambling houses and barber-shop gossip. The most vivid parts of the book are the accounts of the rescues of fugitives from slavery, including many dramatic successes but also heart-breaking failures in cases like those of Thomas Sims and Anthony Burns.

The quantitative materials are often informative, but they are not the strongest part of the Hortons' book. (Between pages 2 and 4 there is a substantial discrepancy in two reported figures for the 1840 black population that is not explained even though it affects several statements in the text.) But the pic-

ture painted by the Hortons of the black political community is moving and persuasive. They identify and describe many black activist leaders, but they also show that in moments of crisis the black community displayed a solidarity and participation that extended to large numbers of quite ordinary, unsung working-class men and women.

This tradition of community identity was severely strained by the inflow of Southern black migrants to the city between 1870 and 1900, which is the subject of Elizabeth Hafkin Pleck's study. Differences and tensions between older black families and new arrivals would hardly be surprising, except that the pre-1900 immigrants were themselves something of a select group. Pleck's approach is more heavily quantitative and concerned with explicit hypothesis testing. By using not only the federal manuscript census but also Boston marriage records, she is able to ascertain the towns and counties of birth for the Southern migrants. She finds that they mainly came from a handful of counties in the Virginia Tidewater and were disproportionately from urban backgrounds (Richmond being the largest single source). The majority were literate. Since blacks were no more than 2 percent of the Boston population before World War I, the city was not exactly overrun by ignorant black field hands at that time.

The remarkable finding is that neither the Boston traditions nor the favorable background were much help. Pleck shows that black upward mobility was severely limited, especially in contrast to the Irish. They were shut out of jobs in department stores, factories, trade unions, and the public sector, remaining predominantly in menial and unskilled work. Those avenues of advancement that were occasionally open (chiefly self-employment and small shops) were vulnerable to high failure rates and downward skidding, even for blacks with education and skills. In a few instances Pleck's conclusion might be changed by more rigorous specification and statistical testing, as when she downplays the importance of the absolute size of the black population in promoting black jobs. But the basic story of blocked opportunity is spelled out clearly.

An important part of the book is the chapter on black families. The author specifically retracts some of the conclusions of her influential 1972 article, which argued that the "two parent household" was the dominant family form among blacks. Her subsequent research with longitudinal data shows that many of these unions were transitory and short-lived. It was not the pathology of black culture that created this situation, however, but the inability of black males to move into secure, well-paying positions as they matured. This interpretation of black family history gives the book a broader significance

than its somewhat specialized coverage might suggest.

GAVIN WRIGHT
University of Michigan,
Ann Arbor

WILLIAM TOLL. *The Resurgence of Race: Black Social Theory from Reconstruction to the Pan-African Conferences*. Philadelphia: Temple University Press. 1979. Pp. ix, 270. \$19.50.

This book has an interesting subject. William Toll has attempted to trace the ways in which black intellectuals thought and planned strategy in the face of the challenges of the post-Civil War era down to 1919 when the Pan-African Congress was held in Paris.

Most of the period was one in which the lot of the black intellectual, like that of blacks generally, was almost hopelessly discouraging. Most students of the period know that W. E. B. DuBois differed profoundly from Booker T. Washington as to what strategies blacks should pursue. What Toll does is to attempt to tell the reader what the other black intellectuals of the period thought. Some of them held political offices, some taught, and others were also in a position in which they were obliged, to some extent, to conciliate white opinion. Considering the circumstances, many of them spoke up with a commendable forthrightness.

Toll makes many shrewd observations about various black intellectuals. One of his difficulties is, however, that he has written a short book about a large number of people. He has not learned the knack of combining analysis with quotations, thus allowing readers to judge partially for themselves the traits of character of a person or the value of his ideas. What readers often get instead is a brief analysis of the views of a person they do not know well or may never have heard of, often with little aid in explaining why that person took a particular position.

In one instance there is an odd statement in the book that may make the reader wonder whether Toll himself is a racist. There is a quotation from Alexander Crummell chiding the black politicians of the 1890s. "Believe me there is no race of people on the American soil who have greater native genius than this race of ours," said Crummell. "Our only difficulty has been opportunity. The mongrel everywhere inches forward . . . , the Negro through the very instinct of his innate genius, modestly retires, and hence is rarely estimated at his true worth." Crummell himself was apparently disparaging mulattoes, and it is odd to notice that Toll includes his statement as if it were a justified criticism of those black politicians who "lacked an ap-

preciation of race." It may be, it is true, that Toll did not intend to imply any criticism of the supposed innate traits of mulattoes.

The book has a great many errors. The name of Charles W. Chesnutt is consistently misspelled, though he is one of Toll's important figures. Toll also has an unfortunate fondness for such words and phrases as "perigrinations," (for well-traveled), "entrepreneurial [sic] ethos," "cliques of cohorts," "enthused," "nascent ethnic group," "syndrome of dependence," and "launched a journalistic career." This is too bad because Toll knows a great deal about his subject. A good editor could have helped to transform this book from a seriously flawed work into something quite good.

THOMAS F. GOSSETT
Wake Forest University

RICHARD GRISWOLD DEL CASTILLO. *The Los Angeles Barrio, 1850-1890: A Social History*. Berkeley and Los Angeles: University of California Press. 1979. Pp. xiv, 217. \$16.95.

Richard Griswold del Castillo's study of Mexican Americans in nineteenth-century Los Angeles is a worthy addition to the rapidly growing literature on the Chicano urban experience. A generally solid, constantly revealing book, it combines painstaking research, careful analysis, and clear exposition.

Following an introductory chapter synthesizing the pre-1846 history of California, the book proceeds topically. Four chapters, each encompassing the 1850-90 period, address such topics as socio-economic changes, the development of Chicano consciousness, and the Chicano political decline. Thirty-six statistical tables, constructed mainly from the author's culling of multiple sources, provide fascinating insights into such varied subjects as year-by-year Chicano intermarriage, occupational shifts, jury participation, and changes in property-holding. The book would have been more readable and cohesive, however, had the author avoided pocking his text with lengthy, distracting, methodological discussions and instead placed these in explanatory footnotes or appendixes.

As portrayed by Griswold del Castillo, Los Angeles Chicanos emerge not as the homogeneous, passive victims of stereotypical fame, but as internally diverse, active participants in the simultaneous struggles to maintain their sociocultural fabric and to capture a part of the American Dream. The author effectively demonstrates that the Chicano decline occurred not because of cultural weaknesses but as the almost inevitable result of Anglo prejudice, numerical domination, and control of political and economic institutions.

In addition to its broad overview, the book is also

replete with interesting historical tidbits: while women headed only 13 percent of Los Angeles households in 1844, by 1880 they led more than 31 percent of Mexican-American families; in post-conquest Los Angeles Mexican Americans expanded their presence as bakers, blacksmiths, and carpenters, but disappeared completely from the ranks of hatmakers, masons, and tailors; and the Anglo-controlled Los Angeles school board rejected Chicano pleas for bilingual education three times during the 1850s.

The book's only major flaw is the author's inconsistency in using national and ethnic categories. At times he treats pre-1846 Los Angeles as part of Mexico, which it was. At times he seems to treat it as already part of the U.S. Southwest, which it was not. At times Los Angeles seems to be floating in nonnational space. For example, the table on birthplaces of 1844 Los Angelenos presents 2,018 as being born in Los Angeles, but only 110 as being born in northern Mexico (p. 9), most unlikely since Los Angeles was part of northern Mexico. Another table lists 1,469 Spanish-speaking persons in 1844 Los Angeles, but only 135 Mexican-born residents, with the Mexican-born composing only 9.2 percent of the city (p. 40). Then where were the other 1,334 Spanish-speaking residents born? Another table cites a "total Mexican-American work force" of 373 in 1844 (p. 53), which is dubious, since Los Angeles was then part of Mexico, not the United States, so there were probably few, if any, Mexican Americans in the city. It is a shame that Griswold del Castillo should have fallen into these categorical inconsistencies and inaccuracies, because they taint an otherwise admirable book and fine piece of scholarship.

CARLOS E. CORTÉS
*University of California,
Riverside*

WILLIAM A. BULLOUGH. *The Blind Boss & His City: Christopher Augustine Buckley and Nineteenth-Century San Francisco*. Berkeley and Los Angeles: University of California Press. 1979. Pp. xvi, 347. \$19.95.

William A. Bullough has markedly increased our knowledge of both San Francisco and urban history by his political biography of Christopher Buckley. The book follows the career of this city boss from his earliest ventures into politics, to his salad days of the 1880s, to his subsequent eclipse. Bullough's main concerns are Buckley himself, his political strategies, the changing metropolis, and the increasingly obsolescent institutional context in which Buckley operated. Using this quadripartite model of analysis, Bullough has produced a precocious defense of the Blind Boss, which largely agrees with

the classical apologia for machine politics and bossism. In the author's skillful hands, the media monster—"The Blind White Devil"—is transformed into Christopher Augustine Buckley, self-made man, political realist, devoted husband and father, and sometime gentleman vintner from the Livermore Valley.

Although Buckley's personality and gifts are prominently featured, Bullough's volume is consistently structuralist. The tension between the structure of a dynamic metropolis and a static city charter produces the opportunities for informal government exploited by the boss, and the boss himself is conditioned in most ways by the city as well. In a sense, everything about the story is deterministic—urban growth makes the 1856 city charter obsolete and necessitates an informal government to supplement the formal one, and urban life creates and molds a political man who can cope with the city for about ten years. Then the metropolis changes again, rendering Buckley obsolete and necessitating new political men and structures better suited to its needs.

Some critics may be mildly disturbed by this structural determinism and perhaps, to a lesser extent, by the relative neglect of the socioeconomic bases of politics and of the analysis of election returns, or by the author's unwillingness to venture much beyond the boss-reform duality of urban political analysis. The main flaw, however, in this superb account of the Blind Boss is its episodic treatment of public policy, since much of the classical apologia for machine politics rests upon the assumption that bosses managed their cities better than their reform successors would. The many failures in the realm of public policy undermine the author's conviction that San Francisco's boss overcame the obsolescence of the charter, managed social and economic conflict, and so on.

Forthcoming studies that narrowly concentrate upon the policy or socioeconomic aspects of city politics may well challenge some of Bullough's findings, but they will merely supplement rather than replace this full, fair-minded, and subtle treatment of a misunderstood political figure.

ROGER W. LOTCHIN
*University of North Carolina,
Chapel Hill*

CARL F. KAESTLE and MARIS A. VINOVSIS. *Education and Social Change in Nineteenth-Century Massachusetts*. New York: Cambridge University Press. 1980. Pp. xxi, 349. \$22.50.

The past decade or so has witnessed the emergence of an exciting and ideologically charged historiography of American public education. "Idealist" or

"democratic" interpretations of institutional innovation have been challenged by studies stressing themes of social control and class conflict. The historians engaged in this revisionist enterprise have been distinguished by their critical stance toward our educational past, but the genuine value and appeal of this body of work derives, I think, from another quality that is apparent in several key works: it is ambitious history marked by powerful interpretive thrusts and bold theoretical synthesis. In seeking the sources of educational change, these historians have looked toward broad social processes, most particularly urbanization and industrialization, in their pursuit of theory and interpretation.

Carl F. Kaestle and Maris A. Vinovskis, in their study of public education in nineteenth-century Massachusetts, question the central themes of this historiography. They also exemplify what seems to be a growing methodological conservatism in several areas of American historiography. On page one they announce that they "do not present a theory of educational development in this volume." If one might expect theory in what the dust jacket for this book calls "social science history," a different lesson is here offered for the profession. What one finds here instead is the disaggregation of broad questions and the inventive manipulation of quantitative data.

This relentless narrowing of questions works against the development of an over-arching thesis. It does, however, produce some important clarifications and amplifications of particular points, though the general thrust of the historiography they are challenging stands up better against the data than their commentary suggests. Probably their strongest contribution to our sense of nineteenth-century educational history derives from their distinction between rural and urban educational history. The recent literature has focused on urban schools; Kaestle and Vinovskis show that many of the conventional issues in education had different meanings in rural and urban contexts. For example, worries about school attendance took two different forms. In rural Massachusetts the proportion of school-age children attending school was high, but the intensity, or total class days in the school year, was low. In the city more were truants, but those who attended had a more intense educational experience. Emphasizing such rural-urban differences helps them to make another important point about centralization: much of the history of public education in the nineteenth century is the gradual extension of the urban model over rural education as well.

Kaestle and Vinovskis also work hard to distinguish, with chronological as well as analytical precision, between commercial and manufacturing

influences. Here too they make an important contribution: educational development seems to correlate positively with commercial indicators and inversely with manufacturing ones in respect to most key questions. What is most interesting about their consideration of economic variables, however, is the broad synthesizing concept that seems to be lurking, unloved and unembraced yet implicit, in their data. There seems to be a connection between educational development and the development of a capitalistic spirit in all economic sectors (rural and urban). But such a broad and diffuse theoretical synthesis would, no doubt, be suspect to historians so intent on "sharpening" historical questions, so explicitly hostile to causal statements phrased as "one to one relationships" as opposed to a series of ecological clusters, and so naively wary of ideology.

THOMAS BENDER

New York University

PAOLO E. COLETTA. *Bowman Hendry McCalla: A Fighting Sailor*. Washington: University Press of America. 1979. Pp. vii, 210. \$10.25.

The author of a *New York Times* obituary published in 1910 asserted that Bowman McCalla was probably one of "the three best known American naval officers of recent times," and Paolo E. Coletta considers him to have been a "minor major figure" (p. iv). Both statements seem exaggerated, the first because George Dewey, William T. Sampson, Winfield S. Schley, Alfred T. Mahan, and Robley D. Evans, to name but a few, must have been at least as well known seventy years ago; the second because McCalla's importance seems to have been slight when compared with that of several of his contemporaries.

Graduating from the Naval Academy at the end of the Civil War, McCalla served in routine fashion afloat and ashore until 1881, when he became detail officer in the Bureau of Navigation under the formidable Commodore John G. Walker. In this position, which he held for six years, McCalla was responsible for officer assignments. He also advocated various reforms, winning friends among his progressively inclined fellows while making enemies of some others. In his first command, he gained an unenviable reputation as a disciplinarian who resorted to illegal punishments. Complaints led to a general court martial and suspension from the Navy for three years. He then returned to active duty, distinguishing himself as a cruiser captain and landing-force commander in the Spanish-American War, the Philippine Insurrection, and the Boxer Rebellion. His exploits—and the wealth acquired by marriage—enabled him to be restored to his original place on the Navy List. He was promoted

to flag rank but failed to obtain the fleet command he thought his due before his retirement in 1906.

In retirement, McCalla wrote a lengthy memoir, on which Coletta relies heavily, although the book's thirteen-page bibliography contains an impressive array of other sources. Reliance on one's subject's memoir, no matter how correct it may be with regard to dates, assignments, and so forth, is likely to be unwise; it has led Coletta to accept McCalla's views almost without question and to argue his case in very uncritical fashion. The most obvious example of Coletta's partiality is his attempt to explain McCalla's most flagrant misconduct—slashing an intoxicated sailor with his sword while the sailor was confined in double-irons. Less than a year later, McCalla disgraced his service and himself by his drunken antics in a foreign port, an incident that Coletta records virtually without comment. This lack of objectivity, beneficial to neither McCalla nor Coletta, overshadows the book's other defects, many of which could have been prevented by competent editing.

ROBERT ERWIN JOHNSON
University of Alabama

ALLON GAL. *Brandeis of Boston*. Cambridge: Harvard University Press. 1980. Pp. xi, 271. \$16.50.

In this study of Louis D. Brandeis, lawyer, progressive, Zionist, and later Supreme Court jurist, Allon Gal has confined his account largely to Brandeis's Boston residency, from 1879 to 1916. Based on intensive study of a wide variety of sources, the author has sought to understand and explain Brandeis's social, ethical, and psychological development during these years.

Born in Louisville, Kentucky, Brandeis grew up in a prosperous, liberal, cultured family without any particular religious bent. After study in Kentucky and in Germany, Brandeis entered Harvard Law School in 1875, where he established friendships with both professors and fellow students, some of which he maintained for many years. Some two years after graduating at the top of his class, he joined in a law partnership with classmate Samuel D. Warren, a member of a prominent Boston family. For two decades this was a highly successful law firm that had as clients representatives of Boston's various ethnic groups including Brahmins, Jews, and Irish.

This mosaic changed, however, as Brandeis assumed a leading role in reform drives against special interest forces in the city. Illustrative of these crusades were his opposition to the New Haven Railroad merger that alienated some of Boston's banking leaders, his fight against the corrupt Democratic machine of John Francis Fitzgerald (Honey

Fitz), and his confrontation with the United Shoe Machinery Company that had the support of some of the city's oldest families. In the course of these campaigns Brandeis shifted from a Mugwump to a progressive and became friends with Robert M. La Follette and Woodrow Wilson. According to the author, the central pressure against President Wilson's appointment of Brandeis to his cabinet as attorney general came from the Boston community. During these years of struggle he became increasingly associated with Jewish organizations and identified himself more with Jewish society in Boston. Eventually, he became a leader in the Zionist movement, which he saw as "the best method for preserving the most notable accomplishments of Jewish civilization" (p. 207).

Though described as a biography, this work is rather more specialized than that. Gal, for example, seldom discusses Brandeis's family or social life except to stress his increasing alienation from Boston society. Essentially, this is a study of Brandeis's intellectual growth and those forces that helped mold his thought during his years as a Boston attorney. Within his self-imposed limits, Gal has written a carefully researched, erudite study of one of the great progressives of the twentieth century. It adds measurably to our knowledge of the man and the period.

ROBERT S. MAXWELL
Stephen F. Austin State University

PHILIP S. FONER. *Women and the American Labor Movement: From Colonial Times to the Eve of World War I*. New York: Free Press. 1979. Pp. xi, 621. \$15.95.

Many book titles promise too much, but the title of this book is both broader and narrower than the contents. On the one hand, there was no "labor movement" in the colonial and early national periods and until the middle 1820s not even a work stoppage in which women participated. On the other hand, a chapter on black women before the Civil War, mostly free black women (a few of whom apparently attempted to form associations), is an unexpected bonus in a book about women and the labor movement. For background Philip S. Foner draws from his *History of the Labor Movement in the United States* (1947-60). On this he superimposes an impressive richness of detail about working women and their efforts to organize.

According to Foner, some one hundred female weavers took part in the first factory workers' strike at Pawtucket, Rhode Island, in 1824. In the 1830s women textile workers in New England frequently "turned out," and in the 1840s these operatives formed some of the earliest unions in American industry. Militant shoebinders founded the first na-

tional union of women workers, the Daughters of St. Crispin, in 1859. But these attempts fell victims to employer resistance, social disapproval, male antagonism, and hard times. In the post-Civil War period both the National Labor Union and the Knights of Labor affirmed the equality of all workers and made at least sporadic efforts to organize women. In the Knights, particularly, thousands of women "found a sisterhood" and were among the most militant unionists of the 1880s. The American Federation of Labor, in its turn, gave lip service to the organization of women but only feeble support. The few women's unions that existed at the turn of the century were largely the work of the women themselves.

Approximately one-half of this book is concerned with the period from 1900 to 1915. Foner covers, among other topics, the work of the National Women's Trade Union League (founded in 1903 and the first national body whose purpose was to organize women workers), women's involvement in the Industrial Workers of the World (I.W.W.), the Waistmakers' Strike of 1909-10, and the Lawrence Strike of 1912—all topics treated elsewhere but here brought conveniently together. Foner's sympathies are clearly with the women.

A straightforward narrative account, *Women and the American Labor Movement* is a good treatment of the general subject for the years it covers. In addition to printed monographs, Foner has searched public documents, union records, a wide selection of newspapers and periodicals, and unpublished dissertations. His short biographies of women labor leaders are especially commendable. The book deserves a better index, for at least two-thirds of the index pages include errors in alphabetizing, a few so serious as to impair the usefulness of the book as a reference work. Foner plans a second volume to bring his subject to the present.

IRENE D. NEU
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Bloomington

JAMES P. JOHNSON. *The Politics of Soft Coal: The Bituminous Industry from World War I through the New Deal*. Urbana: University of Illinois Press. 1979. Pp. xii, 258. \$13.50.

The history of American industries is in large part a success story, focused on the wealthy and powerful. In the studies of our modern economy and political economy, we see recounted again and again the affairs of the moguls and their giant corporate empires. It is thus refreshing to read in James P. Johnson's saga of soft coal's politics an engaging analysis of an industry that never quite made it. The author shows in this thoroughly researched and well-writ-

ten study how the industry's regional structure and intense competition thwarted all those who sought stability, either through private or public means.

The reader finds here no record of business dominance of public affairs. During World War I, the industry's erstwhile leaders so bungled things that the government took control of production and prices under the Fuel Administration. Similarly, in the twenties the national spokesman for soft coal became John L. Lewis of the United Mine Workers, who more than the coal capitalists led the efforts to bring competition under control. Again, in the New Deal years Lewis was dominant in creating a coal code under the National Recovery Administration and in pushing for the passage of the Guffey Acts, which brought the industry under the authority of the National Bituminous Coal Commission. For a time the NRA code seemed to work. Prices and profits started a slow upward climb. But only World War II rescued the industry from depression—as it did the rest of the economy.

There is little to criticize, much to praise about this book. While one might have wished for some entirely new ideas about the interpretive context, Johnson deserves applause for the skillful manner in which he uses his case study to answer some of the central questions that have dominated this field of history in recent years. Thus, he considers whether the analogue of war modeled American responses to the Great Depression and finds that the two situations were dissimilar, indeed were polar opposites, in most regards. Johnson also rejects the idea of industrial self-government as a central theme of business-government relations. Neither a liberal nor a New Left interpretation fully captures the political history of this fragmented industry. Johnson's conclusions are similar to those of K. Austin Kerr and Ellis W. Hawley, two authors who have made important contributions to our understanding of America's modern political economy. Given the high quality of Johnson's research, writing, and analysis, he fully deserves comparison with these accomplished scholars. Johnson's descriptive passages and interpretations stamp him as an author whose future work will have a significant impact upon this field of American history.

LOUIS GALAMBOS
Johns Hopkins University

JAMES B. LANE. *"City of the Century": A History of Gary, Indiana*. Bloomington: Indiana University Press. 1978. Pp. xi, 339. \$12.50.

How one evaluates this book depends much upon whether it is viewed as a scholarly historical account or as a popular journalistic presentation. According to the author, his intention was "to write a

general introduction to Gary's history with special emphasis on specific individuals—both common people and civic leaders" (p. x). The table of contents implies that the author has written a concise view of Gary's background and its general development from its founding in 1906 to the 1970s. As this reviewer proceeded through the book chapter by chapter, however, he became convinced that the table of contents and the subtitle, *A History of Gary, Indiana*, are more cosmetic than authentic.

There is ample justification for popular journalistic presentations, but seldom if ever can they serve as clones for scholarly histories. As a popular journalistic venture, "*City of the Century*" has considerable merit and appeal. Its sketches, though generally loosely related, offer useful information and perspectives about the context and atmosphere in which Gary was born and has since developed. Many sketches reflect familiarity with useful sources, while others make thin and one-sided use of available evidence. But viewed as a historical account, this book is disjointed, fragmentary, and inadequately researched. It suffers by comparison with *The Calumet Region: Indiana's Last Frontier* (1959), by Powell A. Moore, which covers the history of Gary to about 1933. Hopefully James B. Lane will subsequently produce a sequel to this volume that will both add to and update Moore's informative, readable, and scholarly volume.

DONALD F. CARMONY
Indiana University,
Bloomington

JAMES M. SMALLWOOD. *An Oklahoma Adventure: Of Banks and Bankers*. (Oklahoma Horizons Series.) Norman: University of Oklahoma Press, for the Oklahoma Heritage Association. 1979. Pp. xiv, 242. \$9.75.

Although many of the states in the Sunbelt and the Southwest experienced a spectacular surge of growth in the 1970s, the historical literature concerning their development is still exceedingly sparse. Since rapid population growth in these areas is likely to continue during the remainder of this century, the need for scholarly studies of their growth in the last one hundred years is particularly great. Among the many works needed to analyze the westward shift of power in America during this period, those dealing with the region's economic history are among the most essential. Almost every aspect of Western economic growth requires analysis in depth, and the field of banking is no exception.

James M. Smallwood's narrative account of the development of banks in Oklahoma from eighteenth-century beginnings to the present day is therefore welcome. With a generous sprinkling of

anecdotes he recounts the beginnings of merchant banking in eighteenth- and early nineteenth-century Oklahoma and increasing specialization by 1890 when the area became a territory. Thereafter, increasing population and the expansion of commerce and agriculture created a pressing need for banking services that was met by hundreds of new banks before statehood (1907). According to the author, these institutions functioned well, guided only by the bankers' self-restraint and by territorial regulations, copied directly from the older states. More than two-thirds of the book deals with the twentieth century: Smallwood describes the effects of the oil boom of the 1920s, the impact of the Great Depression and the Second World War, and the rise of agribusiness and the petrochemical industries after 1945 on Oklahoma banking.

The study is adequately researched and competently written. Smallwood combed the archives of the Oklahoma Bankers Association and the random records kept by more than a score of small rural banks. He also consulted government documents, newspapers, and magazines. In the absence of secondary works on the subject, the author had few guideposts and had to chart out virgin territory. This book may not be the last word on the history of banking in Oklahoma. But as the first word, it opens up a hitherto obscure field to further inquiry and thus makes a genuine contribution to the history of Oklahoma and the twentieth-century West.

GERALD D. NASH
University of New Mexico

JOHN SAMUEL EZELL. *Innovations in Energy: The Story of Kerr-McGee*. Norman: University of Oklahoma Press. 1979. Pp. xiv, 542. \$17.50.

This book traces the history of a company noted for its innovative approaches to energy production. As John Samuel Ezell puts it, "The goal has been to write a 'traditional' history of a major corporate development with emphasis on the human elements who made it work" (p. xiii). This is precisely what he has done.

The firm began as Anderson and Kerr Drilling Company, which commenced contract oil-drilling operations in the flush Oklahoma oil fields of the late 1920s. Robert S. Kerr, a lawyer, and his brother-in-law James Anderson, a knowledgeable oilman, were in the right place at the right time. Their combined talents enabled the company to survive the era of excess production and economic depression of the 1930s. Anderson left the firm in 1936 and Dean A. McGee, chief geologist for Phillips Petroleum, joined it shortly thereafter. Kerr devoted his time increasingly to politics, serving as Oklahoma's governor and then as United States

Senator. However, he stayed in close touch with his company and maintained formal titles of leadership there throughout his public career, inevitably giving rise to charges of conflict of interest. Kerr was the enthusiastic risk-taker; McGee was his "balance-wheel." Together they made a formidable entrepreneurial team.

Kerr-McGee pioneered in offshore drilling after World War II. It remained prominent in this field, though it lost its leadership position because of McGee's caution. On the other hand, he led it to a position of national stature in integrated energy operations. In 1952, for example, the company acquired its first uranium-mining company and went on to become a key concern in this field. Meanwhile, petroleum operations were expanded. In the late 1960s, oil, gas, and contract drilling provided about two-thirds of the company's net income while uranium contributed about a quarter.

Senator Kerr died in 1963, leaving McGee to lead an expanding enterprise in such areas as fertilizer manufacture, coal mining, and helium production. Ezell ends his coverage of these developments in 1977 when Kerr-McGee had a gross annual income in excess of \$2 billion and ranked 101 on *Fortune's* list of the 500 largest industrial corporations.

As the history of a significant American energy company, this book appears at an opportune time. It records another American business success story, where, despite occasional reverses, hard work and talent win out. To the author's credit, he does not duck such issues as conflict-of-interest charges against Senator Kerr or the mysterious death of Karen Silkwood, a Kerr-McGee employee who was allegedly about to blow the whistle on the company's security practices in its uranium operations. However, little new light is shed on these matters, and the chronological organization of the book sacrifices some depth to breadth of coverage. The net result is a useful company history of the traditional type, as the author promised.

ARTHUR M. JOHNSON
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Orono*

DAVID E. KYVIG. *Repealing National Prohibition*. Chicago: University of Chicago Press. 1979. Pp. xix, 274. \$21.00.

David E. Kyvig's book promises an explanation of the repeal process. To provide it he focuses upon the organized antiprohibitionists, for two reasons. First, sufficient quantitative data is not available to analyze shifts in public opinion, and such analysis, if it were possible, would probably not improve our understanding of how the process developed. Second, the repeal movement played a key role by provid-

ing a means for shifts in public opinion to become politically effective.

The movement portrayed here is the same narrowly based group described by earlier scholars, although Kyvig draws attention to the Women's Organization for National Prohibition Reform and its claims to a membership larger than that of the WCTU. Leaders of the Association Against the Prohibition Amendment (AAPA) were distinguished both by their wealth and by their obsession with growing federal power; Kyvig emphasizes the latter over the former in explaining AAPA resistance to prohibition. No matter what their motives, major repeal organizations employed the same broad range of arguments. None made much progress until after 1929, when the Depression increased popular support for repeal. Meanwhile, AAPA leader John J. Raskob used his position as Democratic Party national chairman to commit the party to repeal, thereby creating a channel for rising repeal sentiment. Once repeal was accomplished, AAPA leaders' decentralist philosophy led them into futile opposition to the New Deal through the Liberty League. Kyvig concludes by arguing that prohibition's passing signified abandonment of the progressive belief in regulating individual behavior through law.

As a description of the leadership of the repeal movement, Kyvig's study adds depth to our understanding of motive and conduct, although the connection between ideas and action needs to be tested through comparison with dry leadership and with other wealthy businessmen. As an explanation of the repeal process it is inadequate. The plea of insufficient data rings hollow since Kyvig has not analyzed the county-level voting data already gathered for at least two states. His dismissal of voting data in advance simply reveals his ignorance of quantitative method. The assertion of organized antiprohibitionists' key role is supported only by following his subjects through the Democratic party's internecine struggles in 1931 and 1932. This procedure unfortunately cannot provide the assessment of the roles of antiprohibitionists outside the repeal organizations that his argument requires. The claim for repeal as a watershed in government's willingness to regulate individual behavior will not convince those who remember that prohibition itself was aimed at institutional change, nor those familiar with government action against groups such as cannabis users during and since the New Deal.

When a full study of repeal is written, *Repealing National Prohibition* will be useful for its examination of the repeal leadership. But Kyvig's book is not that study.

JACK S. BLOCKER, JR.
*Huron College,
London, Ontario*

MICHAEL L. BERGER. *The Devil Wagon in God's Country: The Automobile and Social Change in Rural America, 1893-1929*. Hamden, Conn.: Shoe String Press. 1979. Pp. 269. \$17.50.

Of all the unintended consequences of our automotive love affair, perhaps the most ironic was the car's modernization of rural life. Originally a tool of the back-to-nature movement at the turn of this century, the car was sold to affluent city dwellers seeking to escape a disturbing urban-industrial landscape. Motoring offered an easy return to the frontier experiences idealized by Frederick Jackson Turner in 1893. When car manufacturers began to exploit the farm market after 1910, their pitch was similarly nostalgic: the automobile would preserve country life by ending the poverty and loneliness that drove so many rural people to the city. The results were very different. The car disrupted rural America's institutions, regionalized its commerce, and urbanized its social relationships.

In a thoughtful but somewhat disorganized discussion of rural family life, community relations, leisure, religion, education, and health, Michael L. Berger suggests the complex trade-offs facing country people as they switched from horse to car. In multiplying the number of miles one could travel in a day, the car upset a variety of traditions, values, and institutions that had depended on a smaller, horse-bound spatial radius. For example, while the car did help relieve the perennial isolation of farm people, especially women and children, it probably increased their dissatisfaction by raising expectations. Cars carried them to clubs, movies, dances, and regional schools, where they were exposed to subversive ideas. Moreover, while motorization increased the number and range of a family's social contacts, these acquaintanceships may have been more tentative, less sustaining than those with next-door neighbors, upon whom farmers relied so deeply in pre-car days. Similarly, as rural people traveled greater distances to more sophisticated stores and doctors, the smaller village entrepreneurs who had served carriage-borne clients went out of business. Motorized shoppers and patients now enjoyed more freedom and better quality, but these goods and services were rendered more impersonally. With the passing of the country store and country doctor, many small villages disappeared altogether. In all, the car substituted a more regionalized network of road-oriented consumers for the older, more localized community of land-locked producers.

In documenting these changes, Berger carefully avoids an easy nostalgia for the pre-car country life. Individuals clearly preferred the car. Yet Berger is far too reluctant to weigh the social costs and benefits. The book leaves too many issues unsettled. For example, at one point Berger implies that motoriza-

tion may have hurt many family farmers because it increased their production when agricultural markets were already saturated; prices thus began to fall just at the time that the costs of farming began to rise. Unfortunately, he does not pursue this, other than to allow that the automobile's alleged contribution to agricultural prosperity was "open to question" (p. 45).

Overall, the book contains much valuable information, but it lacks a clear thrust. Berger concludes that the "structure of rural social life" changed vastly (p. 212), but he does not define this structure very clearly. The book lacks the specific geographical focus of Norman Moline's *Mobility and the Small Town* (1971) or the Lynds' *Middletown*. It does not clearly differentiate between regions or between farm and small village. Yet, at the same time, Berger is too hesitant to generalize, to connect to a theory of social change. The book has a theme, but not a thesis. Berger does evoke the complexity of social change, but he does not really explain it.

WARREN J. BELASCO
University of Maryland,
Baltimore County

C. JOSEPH PUSATERI. *Enterprise in Radio: WWL and the Business of Broadcasting in America*. Washington, D.C.: University Press of America. 1980. Pp. xii, 366. Cloth \$19.50, paper \$11.75.

C. Joseph Pusateri has written a fine business history of CBS-affiliate WWL, a New Orleans radio and television station unique not for its programming but its ownership. This is the only commercially significant station in the United States owned by a college or university, in this case Jesuit-run Loyola University. The author has made extensive use of WWL's surprisingly rich station files (which rarely survive), appropriate federal records in Washington, and the archives of Loyola University, 1922 to present. Pusateri focuses on business matters, problems of separation of church and state, evolution of broadcasting in the South, and Loyola's success in keeping the tax-exempt status of the university's largest source of revenue. There are also two chapters about station KWKH of Shreveport, the personal empire in the late 1920s of a genuine crank, W. K. Henderson, who attacked chain stores as "dirty low down daylight burglars" (p. 100) and who lined his own pocket by peddling Hello World Doggone Coffee. The Federal Radio Commission yoked KWKH and WWL together by ordering them to share the same wave length, which explains why a book about one radio station has much to say about another. Huey Long for a time befriended Henderson and broadcast frequently over KWKH.

The bargain basement appearance of Pusateri's book—paperback, two type faces, camera-ready

copy, six blank numbered pages—and its plodding style might lead some to assume the worst. Actually this is a fine contribution to our understanding of a generally ignored mass medium, for the author has uncovered such complete documentation as to how air time was sold, how legal battles were fought in Washington, and how the Jesuits agonized over having an educational station run on sound business principles, even though this meant finally allowing a weekly religious hour “even” for Baptists. Pusateri has missed some amusing ironies, but his research will allow less charitable readers to form their own conclusions. Particularly valuable is Pusateri’s careful treatment of WWL’s entry into television broadcasting, something the best book about an individual station, William Peck Banning, *Commercial Broadcasting Pioneer: The WEA Experiment* (1946) could not do. Business historians as well as students of mass media will be interested in Pusateri’s account of a modern industry’s growth amidst so many changing directives from Washington.

DAVID CULBERT
Louisiana State University

JEFFREY L. MEIKLE. *Twentieth Century Limited: Industrial Design in America, 1925–1939*. (American Civilization Series.) Philadelphia: Temple University Press. 1979. Pp. xiv, 249. \$17.50.

Twentieth Century Limited is a pioneering study of the American origins of the industrial design profession. A handful of other scholarly studies of the profession’s formative years have recently appeared, but Jeffrey L. Meikle’s is easily the most comprehensive and penetrating to date.

As Meikle makes clear, the industrial design profession began in America and in the late 1920s. Its initial practitioners were barely influenced by contemporary developments in European art and architecture, which, one might otherwise assume, would have decisively affected them. Most early industrial designers, moreover, were already commercial artists or advertising and stage designers.

A combination of factors, Meikle convincingly shows, accounts for the profession’s beginnings: increases in the American standard of living and in leisure time; pervasive fears within American business of overproduction and underconsumption of goods for the new consumer mass markets; improvements in American art education and abandonment of the long-standing American sense of artistic inferiority vis-à-vis Europe; and, above all, the growing purchasing power of women. The motivation behind industrial design was thus as much economic as artistic.

Far from dampening the prospects of the fledgling profession, the Great Depression enhanced

them. For American business then desperately sought consumers of its products from among those still able to afford them. Where originally only individual items were (re)designed—ranging from pencil sharpeners to telephones to refrigerators—gradually whole offices and households began to be systematically treated, followed eventually by entire buildings and vehicles. “Streamlining” became the most popular style of industrial design and the symbol of the esthetic ideal of modernity, cleanliness, efficiency, and dynamism its practitioners all sought.

Meikle provides succinct but vivid biographical sketches of each of the most prominent industrial designers: Henry Dreyfuss, Norman Bel Geddes, Raymond Loewy, and Walter Dorwin Teague. Through both their forceful personalities and their influential design studios they attempted to transform America into a veritable technological utopia. The culmination of their collective efforts was the 1939 New York World’s Fair. As the Fair’s motto proclaimed: here was the “World of Tomorrow.”

Geddes’s General Motors Futurama gave 1960 as the date of utopia’s realization, but the America of 1960 resembled that of Futurama only in part: in its skyscrapers, superhighways, and other material improvements, but not in the social and cultural dimensions that Geddes and his fellow visionaries—like most earlier technological utopians—simply assumed would automatically follow those material changes. Streamlining itself, ironically, became an early—and unintended—victim of the planned obsolescence its proponents wished to apply everywhere else. Their utopia, they naively believed, not only would but, given its genius, should remain intact. Meikle’s clever title refers to the quantitative and perhaps the qualitative limits of their vision.

A modest discussion of the role of technological advance in the evolution of utopian thought would have placed *Twentieth Century Limited* in fuller historical perspective. But the book nevertheless stands as a model work in integrating art, architectural, cultural, and business history. Temple University Press deserves credit for producing a book that is at least as well designed as the objects its author describes.

HOWARD P. SEGAL
University of Michigan,
Ann Arbor

C. HOWARD HOPKINS. *John R. Mott, 1865–1955: A Biography*. Grand Rapids, Mich.: William B. Eerdmans. 1979. Pp. xvii, 816. \$22.50.

Fifteen years of research have been devoted to this exhaustive and thoroughly documented biography. Few may remember that for sixty years Mott was a central figure in almost every world-wide Christian enterprise, that he was an intimate of presidents

from McKinley to Hoover and a friend of many other heads of state, and that in 1946 he received the Nobel Peace Prize. Mott liked to think of himself as a lay evangelist in the tradition of his mentor, Dwight L. Moody—and he was. But he was not cut quite to Moody's pattern, for he was a polished and sophisticated speaker who emerged as a leader of consequence during his student days at Cornell University. And his great gift was as an organizer, promoter, and chairman of the board.

Mott's salaried career was with the Y.M.C.A. For many years he was simultaneously general secretary both of the Intercollegiate and of the Foreign Departments of the Y.M.C.A. of the United States and Canada, and then for an extended period executive secretary of the Y.M.C.A. of the United States. Mott's greatest contributions, however, were made in areas of volunteer service. He was the moving spirit and dominating presence (the perennial president, chairman, or general secretary) of organizations he often was instrumental in founding—the Student Volunteer Movement, World Student Christian Federation, Foreign Mission Conference of North America, International Missionary Council, Church Peace Union, World's Alliance of Y.M.C.A.s, Interchurch World Movement, World Council of Churches. Mott also engaged in a few diplomatic activities and served on several presidential committees and commissions.

Mott's career provides a window for viewing the Protestant enthusiasms that helped shape early twentieth-century America, and Hopkins's book provides almost every detail. A single flaw is a tendency to omit dates in the narrative, thus giving few pegs to place events in sequence (when, for example, was the WSCF founded?).

WINTHROP S. HUDSON
*University of North Carolina,
Chapel Hill*

A. GLENN MOWER, JR., *The United States, The United Nations, and Human Rights: The Eleanor Roosevelt and Jimmy Carter Eras*. (Studies in Human Rights, number 4.) Westport, Conn.: Greenwood Press. 1979. Pp. xii, 215. \$17.50.

Human rights represent one of the most persistent and potentially explosive areas of concern in international relations. The importance of this topic warrants more attention from historians than it has so far received. We are indebted to political scientist A. Glenn Mower, Jr., for this brief but penetrating account of America's record in the human rights field. Mower chose to limit his study to two periods, what he calls the Eleanor Roosevelt "era" and the Jimmy Carter era. This somewhat detracts from the value of the book, as an equally careful analysis of

our human rights experience for the 1950s and 1960s would have made this work an authoritative treatment of the topic.

Within the parameters he set for himself, Mower's work can best be described as workmanlike and competent. He carefully avoids becoming an advocate of either partisans of giving human rights a top priority in foreign policy making or those traditionalists who seek to mute human rights at every opportunity. He believes that there are so many variables when dealing with the matter that policy makers must approach each human rights situation on a case to case basis. A strong point of his work is to give us an appreciation of just how difficult it is for any administration to exert much leverage over other countries in the human rights field, and he does an excellent job in showing how the Washington bureaucracy functions in trying to sort out the conflicting information that flows into the State Department from other nations. Mower argues that human rights had a very high priority under Truman, which he attributes in large part to the unflagging zeal and influence of Eleanor Roosevelt. This accords human rights a higher priority than this reviewer had thought was the case during Truman's presidency, but Mower makes a convincing case for his view. Perhaps the most interesting part of the work is Mower's description of Eleanor Roosevelt's emergence as a champion of human rights; this portion of the book would make valuable reading for any student of this period.

As a whole this study deals with case histories of human rights diplomacy rather than examining in great detail the processes of decision making. Instances are given, usually briefly, of particular human rights issues, and the work would have been more valuable had the author given us some of these instances in greater detail. Mower believes that the Carter commitment is so strong that the achievements of his years will be seen as a separate era in the development of human rights diplomacy. In all, Mower has made a valuable contribution where it was badly needed.

THOMAS M. CAMPBELL
Florida State University

GEORGE C. HERRING. *America's Longest War: The United States and Vietnam, 1950-1975*. (America in Crisis Series.) New York: John Wiley and Sons. 1979. Pp. xiii, 298. \$6.95.

Marilyn Blatt Young, in the 1979 Stuart L. Bernath Memorial Lecture of the Society for Historians of American Foreign Relations, lamented the recent trend in academic writing as well as in novels, movies, and the press to revise the history of the Vietnam War. Rather than confronting the effort of six

administrations in Washington to deny the right of self-determination and the immorality of the tactics employed by the American military, scholarly and popular accounts have rationalized intervention and the ultimate failure of American objectives. Guilt and defeat have led not only to revisionism but also to another discernible trend, that is, to forget about the war. As described by Josiah Bunting III in a paper, "Vietnam: A Memory Erased," presented at the 1978 Citadel Conference on War and Diplomacy, there has been a "virtual obliteration of the Vietnam War from our national consciousness." These reactions observed by Young, Bunting, and others underscore the need for clear, straightforward analysis of the Vietnam War. *America's Longest War* provides a response to revisionism and obliges its readers to confront fully the American-Vietnamese experience from 1950 to 1975.

George C. Herring's study calls attention to a number of recurrent patterns in the making of Vietnam policy: optimistic expectations, derived from the belief that American power could achieve "nation-building" as it had presumably done in other countries; ignoring of reports that questioned the prospects for a viable noncommunist government; following the "middle course" within the changing context of policy options; apprehension over the domestic and international implications of "losing" Vietnam. At the base of the involvement beginning with support of the French-backed Bao Dai regime in 1950 lay the unquestioned assumption that a noncommunist state in Vietnam was vital to American security. Throughout the ensuing quarter century that objective demanded a steadily increasing commitment of American resources. France's unwillingness to follow Washington's advice on military strategy and colonial policy so frustrated American leaders that they welcomed the demise of French power in 1954 and took the opportunity to foster unilaterally an anticommunist regime in the southern part of Vietnam. Yet by 1960 the government headed by Ngo Dinh Diem was under pressure from a renewed insurgency. To meet that crisis, the Cold War rhetoric of the Kennedy administration was translated into an ambitious program of counterinsurgency, which significantly increased the American stake in Indochina. The escalation of the Johnson years was based on misplaced confidence in the capability of air power and combat units to force Hanoi's acceptance of a divided Vietnam. American intervention saved the Saigon government, but all of Washington's efforts could not remedy its fundamental weaknesses. The Nixon administration, under intense domestic pressure to withdraw, encountered the intractability of both the North and South Vietnamese governments that only led to continued warfare in the name of "peace with honor." The ultimate collapse of the Saigon

regime two years after the vague settlement of 1973 suggested that the "American effort to create a bastion of anti-Communism south of the seventeenth parallel was probably doomed from the start." By attributing Vietnamese nationalist revolution to external forces, the United States failed to recognize its internal dynamics and found itself "at the mercy of local forces, a weak client, and a determined adversary."

This is the third major study of the war to appear within the last two years. Gunther Lewy's *America in Vietnam* upheld the legality and morality of American involvement; it ranks as perhaps the fullest revisionist account. *The Irony of Vietnam* by Leslie Gelb and Richard Betts argued that despite the failure of American objectives, the decision-making system worked effectively. Herring implicitly criticizes Lewy's thesis, while reinforcing, at least in part, the Gelb-Betts conclusions. Broader in scope than these other recent books, *America's Longest War* provides an insightful analysis of the influence of major leaders, public opinion, and bureaucratic competition on the making of America's Vietnam policy. In sum, Herring has written a thorough and judicious history of the Vietnam War.

GARY R. HESS

Bowling Green State University

PHILIP ABBOTT. *Furious Fancies: American Political Thought in the Post-Liberal Era*. (Contributions in Political Science, number 35.) Westport, Conn.: Greenwood Press. 1980. Pp. x, 265. \$23.95.

Furious Fancies is an able book about the complex world of contemporary American political thought. Philip Abbott explores many of the influential thinkers of our age: John Rawls, Daniel Bell, Robert Nozick, Robert Paul Wolff, among others. He also presents theorists widely known only in the parochial world of political science: Robert Dahl, Theodore Lowi, Henry Kariel, and John Schaar, to name four, but he managed to convince me that they merit broader attention. Abbott's range is wide, perhaps too extensive at times, though it is inevitably true that he leaves notable gaps. For example, he ignores the political thought of both religious and feminist thinkers.

Abbott contends that, while most American theorists believe they have abandoned much or all of liberalism and joined a post-liberal revolt, the truth is that there have been few escapees from liberalism. Like the brilliant view of Louis Hartz, Abbott's position depends on a broad (if more sophisticated) definition of liberalism. Also like Hartz perhaps his argument loses a bit of its cogency just because his definition of liberalism is so inclusive. In Abbott's hands, his view is stimulating and perhaps correct,

but any perspective on American political thought that sees Marcuse and Bell, Kariel and Dahl, Nozick and Schaar as creatures of liberalism may seem too broad.

The great strength of this book is its stimulating individual chapters. Many of them are excellent as single essays and deserve enthusiastic praise. This is particularly true of chapters that treat the liberationists (such as Marcuse), Robert Paul Wolff, W. C. McWilliams, and John Schaar. The discussions of Nozick, Dahl, and Kristol also shine. The chapter on Rawls is the weakest; it lacks direction and is not well integrated into the book. But it is an exception. Abbott also writes well, sometimes superbly. He is a delight to read.

Combined with the pleasing substance of his discussions of individuals, his writing sometimes gives a brilliant cast to this book. At points, however, there is too much of what Abbott concedes is "almost unrelenting criticism" (p. 241). Much of it is unrelated to the argument of the book and occasionally gives the impression that intelligent and creative men are fools.

Abbott concludes his book with ideas toward resolution of the problems of American political thought. He proposes an interesting celebration of friendship as an alternative to the liberalism of strangers and the post-liberalism of emotive communitarians. His idea is intriguing and much more explanation (and defense) of it would have been welcome. But even so, Abbott has done a capable job and made a significant and laudable contribution to the study of American political theory. He deserves congratulations.

ROBERT FOOTH FOWLER
University of Wisconsin,
Madison

CANADA

A. B. MCKILLOP. *A Disciplined Intelligence: Critical Inquiry and Canadian Thought in the Victorian Era*. Montreal: McGill-Queen's University Press. 1979. Pp. xii, 287. Paper \$9.95.

In this original and ambitious volume A. B. McKillop attempts the novel task of sketching an outline of Canadian academic thought from 1840 to 1900. The early Victorians, he suggests, had little doubt that the critical spirit would undermine the essential tenets of the European intellectual heritage. In its defense they marshaled three systems of thought. The assertion of Thomas Reid that reason could not question the great, self-evident truths found a

welcome reception in colonial universities. Natural theology, as taught by the Canadian disciples of William Paley, staunchly proclaimed that design in nature was evidence of an immanent God. Finally, scientists such as William Dawson argued that Darwin, by theorizing on causation, transgressed the legitimate boundaries of Baconian empirical method. Such was "the triumvirate of intellectual orthodoxy that dominated many educated Anglo-Canadian minds" until the 1870s (p. 95).

Evolutionary naturalism shattered the intellectual constraint of the older ethos and ushered in an age that expressed its moral concern in terms of objectivity, tolerance, and respect for individual variation. W. D. LeSueur proclaimed a progressive view of history and an organic conception of social reality. John Watson and other idealists preached that history represented the evolution of reason and that freedom consisted not in denying external constraints but rather in acting in accordance with the dictates of this universal principle. Among their audiences were those like Salem Bland who would later become leaders in the Social Gospel movement. McKillop contends that many twentieth-century Canadian intellectuals reflected this philosophical persuasion.

In order to sustain his formulation, McKillop sprinkles the book with an array of supporting characters, at least some of whom are misconstrued. R. M. Bucke, for example, is said to have felt that "the mind and body were essentially distinct" (p. 162). In fact, Bucke built his reputation as a leading spokesman for somatic psychiatry on the argument that man's "moral nature" was a simple function of the sympathetic nervous system. Of greater significance, however, are a host of minor, indeed, nameless, characters—"the generations of senior arts and divinity students" taught by Watson and his colleagues (p. 216). McKillop is admittedly writing "internal" intellectual history, but the applicability of his thesis must be considered in broader terms. It seems highly improbable, for example, that many readers followed the likes of Watson through the thickets of Edward Caird's neo-Hegelian "objective idealism." Archibald MacMechan of Dalhousie University was a convinced disciple of Watson's fellow-traveler, George Paxton Young, yet he never advanced beyond an intuitive appreciation of his philosophy. Remnants of Watson's metaphysical conclusions may have lingered in Canadian thought; his methodology clearly did not.

Despite such cavils, the book is a welcome contribution to the diminutive shelf of Canadian intellectual history. Although often marred by an opaque style, it is based on extensive research and is animated by the author's sympathetic humor and scholarly enthusiasm. Important themes in nineteenth-century Canadian thought are identified

and previously neglected individuals rescued from obscurity. More significantly, such thinkers are lifted beyond their colonial confines and placed in the larger context of Western thought.

S. E. D. SHORTT
Queen's University

HARVEY J. GRAFF. *The Literacy Myth: Literacy and Social Structure in the Nineteenth-Century City*. (Studies in Social Discontinuity.) New York: Academic Press. 1979. Pp. xxii, 352. \$27.50.

Harvey J. Graff has used an imaginative array of quantitative and qualitative sources to investigate what he calls "the literacy myth" and its place in the relationships between schooling and society in the mid nineteenth century. The myth suggests that primary schooling and literacy are necessary for economic and social development, the establishment and maintenance of democratic institutions, and individual success and advancement. Graff is critical of historical and sociological treatises on education and modernization that feature statistical analyses of social trends without taking into account individuals and their perceptions, and he has used quantitative data on particular persons in three Ontario cities as his chief sources. Thoroughly familiar with recent studies of Anglo-American social, cultural, and educational history that bear on his topic, Graff also demonstrates sensitivity to the political and ideological implications of his research. Determined to stake out a frankly revisionist interpretation heavily indebted to notions of "cultural and ideological hegemony," Graff also makes readers aware of the "contradictory and complex" conclusions his research has generated. The result is a book that, while studded with overstatement, stands as the most useful and provocative exploration of its subject currently in print.

Graff begins his analysis with a survey of school reformers in Ontario who, drawing self-consciously from the examples provided by colleagues in England and the United States, established a public school system dedicated to socializing children to be "properly schooled, morally restrained" adults. Literacy, in this view, provided reformers a "central instrument and vehicle" that could be used in the "reordering and reintegration of the new nineteenth century society."

Having described the ideology of the reformers and educators, Graff uses his Ontario data to argue that while illiteracy may have limited individual success in the nineteenth century, literacy in no way ensured material prosperity or cultural progress. Literacy played at most "a reinforcing or mediating role" in a society where "class, ethnicity, and sex were the major barriers of social inequality." Al-

though valuable to the goals and needs of industrial capitalist society as well as to those of individuals, literacy did not benefit those who possessed it (usually imperfectly) in the ways advertised by its promoters. Nor did illiteracy bar the way to material success or even modest progress in the ways expected by those who persisted in describing the unschooled as a congeries of rootless degenerates whose outsider status threatened the stability of society and drained off resources necessary for future progress.

The heart of Graff's study, and his most original contribution, is his comparison of illiterates in the census of three Ontario cities (Hamilton, London, and Kingston) to a control group of literates in Hamilton. Fully aware of the limitations of such data, he examines these groups in terms of ethnicity, occupation, wealth, home ownership, family formation, persistence in the cities, inter-generational mobility, and school attendance. The illiterates (less than 10 percent of the population and disproportionately Irish Catholics and women) made up over one half of the unskilled common laborers, but of all the poor only 13 percent were illiterate. Graff shows that illiteracy did not "cause" or even serve as the "primary determinant" of either the pace of industrialization or criminal behavior. Within the cultural life of Ontario, literacy had at most an indirect effect, and Graff adds his voice to those who argue for a modification of modernization models to take into account the persistence of oral and visual communication as significant cultural features of industrial society.

Although the book suffers from a good deal of repetition, as well as a surfeit of words like "clearly" and phrases like "there can be no doubt," it ought to be carefully read and taken into account by all students of nineteenth-century Anglo-American social history.

WILLIAM ISSEL
San Francisco State University

LATIN AMERICA

HILDEGARD KRÜGER. *Der Cabildo von Asunción: Stadtverwaltung und städtische Oberschicht in der ersten Hälfte des 18. Jahrhunderts (1690-1730)*. (Europäische Hochschulschriften, Reihe III, Geschichte und ihre Hilfswissenschaften, number 126.) Frankfurt am Main: Peter D. Lang. 1979. Pp. viii, 222.

For two hundred years after the Conquest of the 1530s the Paraguayan "creole-mestizo elite of Asunción was not only an extended family, but also a political-military community" (p. 111). This small

group, descended from the conquerors, sought economic advancement in the yerba mate trade, rather than in latifundia agriculture. Possession of small encomiendas of Indians gave social prestige to the upper class, as did rank in the provincial militia, but most important for the promotion of their interests was domination of the Cabildo, or City Council, of Asunción. By the 1600s positions on the Cabildo were generally no longer elective but purchased, and with no hesitation the elite of Asunción used their political power to safeguard and promote their own economic and social interests. In these pursuits, they ignored higher authority, deposed royal governors and elected temporary replacements in extreme circumstances, and confronted their competitors in the yerba trade, the Jesuit missions to the south.

Not only is this work the first to investigate Asunción's Cabildo for an extended period of the colony, but also it is refreshingly realistic because economic interests and social prestige factors are analyzed as motivations of the elite-dominated Cabildo, rather than the common nationalistic interpretation of that institution being a popular defender of the province's interests. The author discusses Spanish urban history, the conquest and creation of Paraguay, the subsequent isolation and neglect of the province, and the peculiarity of Asunción as a frontier city. Noteworthy are the sections defining the structure and functions of the Cabildo, the formation of the Asunción elite, and that class's social and economic interests.

The *clase capitular* of Asunción played a crucial role in the Comunero Rebellion of 1717-1735. The myth that this movement was a democratic precursor of Independence is again destroyed as the author demonstrates that domination of the yerba trade occasioned the rebellion in opposition to the damaging competition of the Jesuits. But, as a result, the elite lost internal power and prestige by 1730 when the rebellion escaped Cabildo control. The rise of the rural elements in defiance of the Cabildo and then the reimposition of royal authority in 1735 ended the political power of the elite.

Integral to this work are lists of Cabildo members during this forty-year era, their terms of service, methods of acquisition of office, and genealogical tables demonstrating elite kinship relationships. Sources are the *Actas Capitulares* of the Cabildo, the little used *Nueva Encuadernación*, *Testamentos*, and *Propiedades* of the Asunción Archive, and extensive secondary material. The chapter organization is rather erratic, but the book is an important contribution to Paraguayan history, the history of the colonial Río de la Plata, and Spanish American urban history.

JERRY W. COONEY
University of Louisville

ENRIQUE OTTE. *Las Perlas del Caribe: Nueva Cádiz de Cubagua*. Caracas: Fundación John Boulton. 1977. Pp. 620.

Cubagua, a small desert island off the coast of Venezuela, was a focal point of intense economic activity in the first quarter of the sixteenth century. It was the site of the Spanish settlement of Nueva Cádiz de Cubagua founded to exploit the pearl fisheries located near its shores. For some twenty-five years, until careless methods of exploitation destroyed its source of wealth, it was one of the most prosperous places in the Caribbean. It also was the center of a ruthless trade in Indian slaves utilized as divers. Enrique Otte, a longtime researcher in the Archivo General de Indias in Seville, has written an account of the history of Cubagua based mainly on extensive data derived from documents in the sections of Justicia, Contratación, and Contaduría of that archive. The principal contribution of this volume is the use of these original sources, most of which have never been utilized before.

The work is divided into two parts. The first section deals with the exploitation of the pearl fisheries, the organization of the enterprise, and methods of extraction. The operations of the pearl trade are discussed as well. Part two describes the town of Nueva Cádiz de Cubagua, its economy and society. The volume also contains a long appendix containing transcribed documents relating to the text.

The establishment and continued existence of the settlement of Nueva Cádiz depended on a steady supply of water and Indian slaves that were secured from the Venezuelan mainland with great difficulty. In addition, the soil of Cubagua was so dry that the inhabitants were totally dependent on the import of food and other necessities. Maize and cassava were brought in from Hispaniola and Puerto Rico while wine, olive oil, and other staples came directly from Seville or via the Greater Antilles. From the beginning, the trade in food commodities was controlled by a select number of Sevillian merchants, their agents, and associates in the Caribbean. These same men, mainly Andalusians but also Basques and merchants from Burgos in addition to foreigners like the Genoese, monopolized the trade in slaves and pearls as well. Similarly, they also formed the governing elite of Nueva Cádiz. The author's presentation of the social structure of Nueva Cádiz is sketchy due to the fact that the primary records essential to the reconstruction of any local Hispanic society have been lost. This volume is further weakened by faulty style and organization. Much of the important information has not been incorporated into the text but has been relegated to the footnotes, a massive corpus of 2,086. Encumbered by this overwhelming apparatus, the book becomes dry and monotonous reading and is

likely to dissuade all but the most patient and persevering readers from pursuing it from beginning to end. Finally it lacks an appropriate conclusion that would have given the author an opportunity to further synthesize his material and to place it within a broader framework than he has done. In sum, despite the limitations of this volume, we must be grateful to the author for extracting an important collection of documents and making their contents available to historians.

RUTH PIKE
Hunter College,
City University of New York

DAVID NICHOLLS. *From Dessalines to Duvalier: Race, Colour and National Independence in Haiti*. (Cambridge Latin American Studies, number 34.) Cambridge: Cambridge University Press. 1979. Pp. xi, 357. \$29.50.

Historians share with Diogenes the impossible task of searching for "the truth." Most historians recognize the limitations of their craft; it is only when we close the shutters on our lanterns and imagine what we want to see, even for a supposedly good cause, that our craft becomes craftiness. I do not disagree with David Nicholls that the writing of history influences the future, but I am troubled by his remark that "might it not therefore be better to recognize the fact and to write an account of the past which will have an impact on the future in a way which the author regards as beneficial . . ." (p. 16).

Despite my suspicion of what Nicholls may regard as beneficial for mankind, I did not find many cases of openly biased writing. One is his view of Dessalines as the champion of social justice. A person need not be an apologist of neo-colonialism, as Nicholls claims, to see in Dessalines something less

than a paragon of virtue. According to Nicholls, Dessalines tried to suppress mulatto-black conflict, considered himself the spokesman of disinherited blacks, and offered his own daughter Célimène in marriage to Pétion "as a token of his determination to break down colour lines" (p. 38). Of course Nicholls fails to point out the complications in Pétion's refusal: Célimène was pregnant by another man, and rumors were beginning to circulate as to Dessalines's impending overthrow. Furthermore, Nicholls applauds Marxist writer Jacques Stéphen Alexis for his correct view of the Haitian past and attacks black intellectuals of the Negritude tradition for blurring economic class conflict with racism.

The main thesis of Nicholls's book is that race generally united Haitians in the nineteenth century. Even though Haitians continued to consider France as the Mecca of culture and Africa as barbarous, they took pride in being black and strove to prove racial equality factual. Color, on the other hand, divided Haitians, mainly along cultural rather than racial lines. After the U.S. occupation in 1915, race also divided Haitians as black writers of the Negritude tradition took a racial stance in rejecting Haiti's colonial past. A generation ago the ideas of race and color found much elaboration in James Leyburn's *Haitian People*, but Nicholls has done a distinct service for the study of Haitian history by revealing their complexity.

By far the best feature of Nicholls's book is his excellent historiographical review. There is no other work in English that gives such thorough coverage of Haitian writers, the influences that worked upon them, and the goals that they wished to achieve. This should attract scholars, even those who find the deeply polemical tone of the author's work objectionable.

THOMAS O. OTT
University of North Alabama

Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

ALFRED D. CHANDLER, JR. and HERMAN DAEMS, editors. *Managerial Hierarchies: Comparative Perspectives on the Rise of the Modern Industrial Enterprise*. (Harvard Studies in Business History, number 32.) Cambridge: Harvard University Press. 1980. Pp. ix, 237. \$16.50.

ALFRED D. CHANDLER, JR., and HERMAN DAEMS, Introduction. ALFRED D. CHANDLER, JR., The United States: Seedbed of Managerial Capitalism. LESLIE HANNAH, Visible and Invisible Hands in Great Britain. JÜRGEN KOCKA, The Rise of the Modern Industrial Enterprise in Germany. MAURICE LÉVY-LEBOYER, The Large Corporation in Modern France. MORTON KELLER, Regulation of Large Enterprise: The United States Experience in Comparative Perspective. OLIVER E. WILLIAMSON, Emergence of the Visible Hand: Implications for Industrial Organization. HERMAN DAEMS, The Rise of the Modern Industrial Enterprise: A New Perspective.

CAROL R. BERKIN and CLARA M. LOVETT, editors. *Women, War, and Revolution*. New York: Holmes and Meier. 1980. Pp. xiii, 310. Cloth \$27.50, paper \$9.75.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

Edward S. Laine's review of my book, *In Time of Storm: Revolution, Civil War, and the Ethnolinguistic Issue in Finland* (AHR, 85 [1980]: 655), left me puzzled. His approach was to criticize the book both for doing what it does not and for not doing what it does. This kind of shadow-boxing is misleading and confusing. He attacked the book for being "clearly in the Finnish nationalist tradition" and for "underscor[ing]" the legal and legitimate basis of Finnish independence—unique and odd claims to anyone familiar with the book or with my previous publications and their reception. Both the upholders of and the opponents of the nationalist and legitimist traditions should be startled. Other critics, more knowledgeable about the topic than Laine, have taken the opposite view.

Academician Eino Jutikkala, the editor of the *Finnish Historical Review*, reviewed the book and commented that "it is national hubris to search emotion-based attachments for his motivation. . . . Finland is only a representative example to him of a country. . . . nationalism and socialism created their own polarizations." Although complimenting me for "scholarly criticalness," Jutikkala criticized me for not adequately stressing Finnish aspirations for independence (*Uusi Suomi* [1979]). Another reviewer, Lauri Hyvämäki, wrote, "Sometimes it is said that a historian must have no fatherland, party, or relatives. Hamalainen fulfills the first two requirements excellently, for, although born in Finland, he analyzes coolly and objectively, as if from some other planet. . . . He keeps under tight cover any possible antipathies and sympathies . . . ; no native scholar could have gotten so much out of so difficult a topic in such a solid manner" (*Kanava*, 9 [1979]: 570–72).

As for my alleged fixation with legality, nowhere

have I underscored, or even mentioned, "legally obtained" or "legitimate" independence. Quite to the contrary, I do not think a narrowly legalistic approach to the revolution and civil war fruitful, and I expressed criticism of such narrow legalism in my book.

In commenting on my point about violence, Dr. Laine claimed that I have ignored earlier violence and forgotten the linkage between violence-activists-Jagers-White guards. In fact, I used precisely that theme in explaining the prominence of the Swedish speakers among the Whites, as should be clear to anyone who has read my book (for example, pp. 15–16, 73–75: "Younger . . . Swedish speakers . . . moved toward active resistance and . . . violence," and "the subsequent Jager movement . . . can be regarded as successor to the Activists"). Laine also ignored the difference between the impact of large-scale terror in the streets in front of crowds of onlookers and violence more removed from the public's eye.

Concerning Dr. Laine's doubt that a social revolution took place in Finland, available space allows the repeating of only a few principal indicators, such as the replacement of the previous ruling creed, power structure, and elite by a new dominant ideology and political, administrative, and judicial institutions manned by a new personnel of working-class origins, suppression of all nonsocialist political meetings, organizations, and publications, collapse of the former employers' authority, and redistribution of control and ownership of property.

To claim that a book takes positions that it does not and to pretend that it excludes matters that are integral parts of its theme are unfair, to say the least. Dr. Laine has set up a straw man to knock about—perhaps counting on the negative ring of the word "nationalist" in the Anglo-American scholarly tradition and upon the effect of guilt by association.

Dr. Laine's comment on my alleged "most significant . . . findings" make little sense to anyone with a knowledge of the topic. It does not speak well of his carefulness either to talk about "the immediate response" of a "faithful translation." I wonder also whether he read the book with care or whether he

failed to comprehend what he read. The text and findings should have been clear enough; other reviewers have understood them and judged the book significant enough to write knowledgeable and several-page-long reviews of it.

PEKKA KALEVI HAMALAINEN
*University of Wisconsin,
Madison*

DR. LAINE REPLIES:

Clearly, Pekka Kalevi Hamalainen's recent "puzzled" state, no less than the bombast that it occasioned, has been an astonishing invention of his own manufacture. Indeed, a judicious reading of my review of his book will confirm that in no way—unfairly or otherwise—did I "attack" his work or person but rather credited them with a respectable place in the historiographical mainstream in the study of the modern Finnish state and, in so doing, recommended them to specialists in the field. Apparently still befuddled by the same delusion, Hamalainen now suggests that I, as a reviewer, should not have discussed what *his book did or did not do* in terms of its nature, content, or significance. In presenting such an argument, is he seriously advocating a wholesale reduction in the function and responsibilities of all reviewers in the pages of this journal, or is he just elevating his own work into a special category that would exempt it from the consideration of mere mortals? Were either of these notions less dangerous to the fundamental practice of intelligent criticism, I would not have bothered to respond to Hamalainen's outburst.

In the same vein, Professor Hamalainen's tactic to pit me against his colleagues in Finland, those with whom I have even less quarrel, is a distasteful canard. All reviewers bring to their work their own individual perceptions, experiences, and knowledge and, as well, are obliged to impart their syntheses in a manner best suited to their particular readership. Hence, this attempt so crudely to quantify and qualify book reviewers can only serve to debase us all. Whether or not Hamalainen is at present able to appreciate the fact, only the diversity of opinion possible under the current regime of nonimitative book reviewing guarantees his work a fair hearing in widely differing cultural milieus.

If—for argument's sake—a consensus had to be reached among all reviewers of Professor Hamalainen's book in North America, whose opinion should prevail? Since Hamalainen finds fault with mine, perhaps he would prefer that of Mauri Jalava (*Canadian Slavonic Papers*, 22 [1980]: 161–62), who noted the "tendentiousness in his historical analysis," particularly for seeing "the Civil War and the social revolution . . . ignited simultaneously on 27 January 1918." Then, of course, John H. Hodgson's views (*Canadian Journal of History*, 15 [1980]: 150–51)

might win out. He found in Hamalainen's work "what can best be called immature and partisan positions, such as numerous attacks on other scholars" (a point, I believe, also referred to in Hyvämäki's review cited by Hamalainen himself) and later even questioned the very *raison d'être* for the book by suggesting that it "offers very little that is new" and "does not answer one of the few questions that remain six decades after the civil war." At least my review would have provided a suitable book-jacket blurb—to wit, "Hamalainen's work is recommended for specialists in modern Finnish history."

In any event, I eagerly look forward to Professor Hamalainen's next review of someone else's book to see how well he practices what he preaches.

EDWARD W. LAINE
*Public Archives of Canada,
Ottawa*

TO THE EDITOR:

If, as the saying goes, the less one knows about a complex subject, the more difficult it is for him to explain it succinctly, then Richard H. Minear's review of my book, *Japan's Political Revolution under MacArthur* (*AHR*, 85 [1980]: 698), must raise some question as to the depth of his knowledge of the seven-year Allied occupation of Japan.

Though the fourteen chapters of my study are concerned essentially with such things as U.S. occupation policy, the SCAP-drafted constitution, MacArthur's differences with Washington, certain SCAP personalities, relations between SCAP staff sections and between American and Japanese officials, and the power of the Diet—subjects in which students of the occupation have a keen interest—Professor Minear brushes all of this aside with one innocuous sentence.

But he squandered the better part of one of his five paragraphs taking me to task for the few lines I devoted to a book review by Sir George Sansom, geisha, meetings I had with high Japanese military and Imperial Household persons, and a fuss involving two SCAP officials. He frittered away another paragraph questioning my digressive speculation as to whether MacArthur's proposed strategy for fighting the Korean War, if approved by the Truman administration, might have spared us the tragedy of the disastrous ten-year Vietnam War. His last two paragraphs are personal attacks on me for my aversion to a handful of American left-wing writers, who wanted occupied Japan to abolish the Japanese throne and establish a people's republic.

In sum, Professor Minear's total preoccupation with side issues and subordinate items can hardly be taken as a serious review.

JUSTIN WILLIAMS, SR.
Washington, D.C.

PROFESSOR MINEAR REPLIES:

As Justin Williams, Sr., concedes, my review does describe the contents of his book. It also lists the positions Mr. Williams held in the occupation and describes the sources upon which he based himself. The comments that he misconstrues as a personal attack are my attempt to capture his polemical style and describe his political stance. Studying the historian is, for me, far more than a "side issue" or "subordinate item."

RICHARD H. MINEAR
University of Massachusetts,
Amherst

TO THE EDITOR:

I must thank you once again for the attention you have given, in the book review section of the *American Historical Review*, to "The States and The Nation" series of state histories (*AHR*, 35 [1980]: 702-11). All of us who worked so hard on the series to try to make it merit such attention are gratified by that and by the conclusions of your reviewers. We are equally grateful, however, for the unique pleasure you have made possible for us—the pleasure of discovering what good critical minds make of the series *as a series*. Some of our books had been reviewed by comparison before, but you have given us the first comprehensive assessment; and the resulting critical perspective on the character of the series is a great service. Thank you very much!

GERALD GEORGE
American Association for
State and Local History

TO THE EDITOR:

James R. Leutze, in reviewing our *Hitler vs. Roosevelt: The Undeclared Naval War* (*AHR*, 85 [1980]: 735), reported that he had only "minor complaints" about the book itself. Yet to him the front material, with its "real problems," is outrageously immodest. Actually, we did not write or even have an opportunity to soft-pedal the commendatory blurbs, and we are astonished that our critic revealed such ignorance of the publishing process.

Our preface, like many another, is conventionally personal. We explained why we wrote the kind of book we did, and we gratefully acknowledged assistance from a score or more of people, including a number of high-ranking naval officers, mostly veterans of World War II and its preliminaries. Some of these men provided us with valuable oral information, which in some cases they later set down in letters or memoranda. We have deposited all of these unique manuscript materials, plus others, in the

Bailey-Ryan collection at the Hoover Institution, Stanford University.

At no place in the book did we state that the Hitler-Roosevelt "feud" was *the* major cause of the clash between the two nations. But the insulting epithets on both sides certainly grew increasingly venomous prior to the declared hostilities after Pearl Harbor. As we stated in our "immodest" preface, we have for the first time put all of the maritime grievances on both sides in a military-diplomatic setting, and this list includes a number of little-known ships that will never be heard of again. Some of the major incidents, such as those involving the *Greer*, the *Kearny*, and the *Reuben James*, are reasonably well known but are needed to complete the lists for the general reader, whom we primarily had in mind. Naturally, we did little with other secondary accounts that cover these incidents less fully than we have, and we doubt the existence of the reviewer's multipage list of relevant sources that we have "overlooked." We may have looked at them and concluded that they contain nothing essential that we did not already have.

On the eve of Pearl Harbor there were millions of American isolationists who feared that Roosevelt was deliberately trying to drag the nation into Hitler's war. There are still a good many citizens of like mind, our "modest" reviewer to the contrary notwithstanding. The leading authority on Franklin Roosevelt, the Harvard professor who wrote one of our two blurbs, has said that we have done much to dispel this delusion.

THOMAS A. BAILEY
PAUL B. RYAN
Stanford University

TO THE EDITOR:

I should like to add a brief note to Nancy Rosenblatt's review of my book, *A Time of Triumph and of Sorrow: Spanish Politics during the Reign of Alfonso XII, 1874-1885* (*AHR*, 85 [1980]: 645). I plead guilty to shortcomings in both organization and background—these are defects of every history book, particularly those written by others. In respect to the concept of an "internal constitution" used by Cánovas del Castillo, Rosenblatt is correct that elements of this concept had been developed much earlier, but more, I believe, from the moderado than from the liberal side. Cánovas was, however, the first to make it a substantial principle of political law, and he used it in ways rather different from those that either the moderados or the liberals had employed it previously. I would refer Rosenblatt to José Luis Commellas's treatment of the subject in his biography of Cánovas (pp. 157-74).

EARL R. BECK
Florida State University

Recent Deaths

BRAINERD DYER, emeritus professor of history at the University of California, Los Angeles, passed away on April 27, 1980, after several years of seriously declining health. Born in Wheaton, Illinois, on November 9, 1901, he received his A.B. in 1923 from Pomona College, where he was a Phi Beta Kappa, played varsity football and basketball, and joined Phi Delta, a local fraternity. At Harvard University he received the A.M. in 1925 and the Ph.D. in 1932; James Phinney Baxter III supervised his dissertation on the public career of William M. Evarts, a cabinet member under Andrew Johnson and Rutherford B. Hayes.

Dyer's teaching career began at Dartmouth College in 1926. In 1930 he joined the department at UCLA and served until his retirement in 1969; the university recalled him for one course the following year. From 1947 to 1953 he was chairman of the department, and in 1955-56 he was a Fulbright Lecturer at the University of Helsinki. His principal teaching areas were the Civil War and Reconstruction, and American constitutional history, though he regularly taught the freshman survey and graduate seminars. Eighteen doctoral students completed dissertations under his direction.

His dissertation became his first book, published in 1933; his second was *Zachary Taylor*, published by LSU Press in 1946. He wrote a dozen articles, mostly on Civil War topics, for such journals as the *Pacific Historical Review*, *Huntington Library Quarterly*, *Mississippi Valley Historical Review*, *Journal of Negro History* and *Civil War History*. He also wrote a number of biographical sketches for the *Encyclopedia Britannica*, about fifty scholarly reviews, and "Today in History," a daily column in the *Los Angeles Times*. This column reflected his pleasure in bringing history to the general public, as did his membership in the Civil War Round Table, his lectures for UCLA's University Explorer radio programs, his work with the Southern California Social Science Association, and his advisory status on state and national Civil War Centennial commissions.

He was chairman of the UCLA Letters and Science faculty in 1966-67 and secretary of the University of California Academic Senate (South) in

1963-64 and 1965-66. He served on committees of the Southern Historical Association and, in 1956-59, on the Board of Editors of the Mississippi Valley Historical Association. He chaired the program committee for the American Historical Association's San Francisco convention of 1965. The Pacific Coast Branch of the AHA chose him for its council in 1952-55, vice president in 1965-66, and president in 1966-67. His presidential address, "One Hundred Years of Negro Suffrage," appeared in the *Pacific Historical Review* in 1968.

UCLA colleagues remember Brainerd Dyer as a careful, thorough, and fair man who stressed attention to detail and efficient administration of department affairs. He had a strongly developed sense of professional responsibilities and set high standards of performance for himself and others. He had strong views and expressed them, often tartly, yet he did not hold grudges. We who were his doctoral students knew that he could be calmly scathing in seminar, and he marshaled his T.A. staff, sometimes more than we liked. But he would pore over dissertation chapters by the hours, correcting grammar as well as historical errors and summoning a conference within a week or so. He would pursue financial aid for other graduate students besides his own. And his freshman courses were laboratories in which we received a valued initiation into the rituals and responsibilities of being a professor.

Undergraduates considered his classes an initiation too, not only into scholarly expectations but also into the personal dimensions of being a "college educated" man or woman. For he took pains to "manage" a 400-seat auditorium (with seating chart) and occasionally suggested to errant students (by name) that there were certain recognized principles of civilized behavior. Yet he also personally read all the probable F and D papers after we had done so, arranged necessary tutoring, had almost daily office hours, and in one of the many subtle ways that showed his appreciation of a well-rounded college experience, made sure never to schedule a midterm the Monday following a home football game.

Brainerd Dyer is survived by his wife of more

than forty years, Karin, by two children, Martin and Elise, and by two brothers. His friends and colleagues will remember him for his careful scholarship and professional contributions, and many of us will recall our own favorite "Dyer story" as an additional tribute to a man whose work often carried him into the field of biography and who appreciated that our profession is central to the humanities.

JAMES E. SEFTON
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ERNST M. POSNER died in Wiesbaden, Germany, on April 18, 1980. He was born in Berlin on August 9, 1892, and majored in philosophy, history, and languages at the University of Berlin. His studies were interrupted by service in the German army from 1914 to 18. He served in both the Eastern and Western Fronts and was awarded the Iron Cross after being wounded near the Polish frontier. He completed his doctorate in history in February 1920. Shortly thereafter he began an internship at the Prussian Privy State Archives where, after several examinations in 1921, he became a professional archivist. Within a few years he was a leading lecturer at the Institute for Archival Science and Advanced Historical Studies, published several articles on archives and Prussian history, and began drafting the *Acta Borussica*, a documentary history of eighteenth-century Prussian administration. As he studied U.S. constitutional history under Otto Hintze, Posner assisted American historians who were using the archival resources. He thus met Eugene Anderson, Samuel Flagg Bemis, Merle Curti, and Walter Dorn. Under the Nuremberg Laws of 1935, *Hausreferent* Posner, of mixed Jewish-Christian heritage, was reduced in rank and prevented from completing the *Acta Borussica*.

At the suggestion of the American historians he visited the United States in 1938. Despite their urging, he returned to Berlin and, after the *Krystallnacht* pogrom, was imprisoned in the concentration camp at Sachsenhausen. When a general obtained his release, Posner and his wife, Katherina, migrated to Sweden where he lectured on U.S. archives. By late summer of 1939 they were penniless American immigrants.

Solon J. Buck, who had recently transferred his course on archival theory and practice from Columbia University to the American University, selected Posner as course co-adjutor. When Buck was appointed Archivist of the United States in 1941, Posner became adjunct professor of archival history and current administration. He educated two generations of U.S. archivists in their profession and initiated the first graduate courses in records management.

Later, until his retirement in 1961, he served in the American University successively as professor of history and archival administration, director of the School of Social Sciences and Public Affairs, and dean of the Graduate School. In 1943 to 1944 he served successively on the Dinsmore Committee of the American Council of Learned Societies and the American Commission for the Protection and Salvage of Artistic and Historic Monuments in Europe. He was active in the American Association of State and Local History, which gave him an award for distinguished service. His contributions to the deliberations of the International Council on Archives and the related Round Table gained him additional international acclaim. In 1955 he was elected president of the Society of American Archivists. A Guggenheim fellowship and a Fulbright grant in 1957 permitted a year of study at the Vatican and Italian archives. Unfortunately, his studies were interrupted by a heart attack.

The Federal Republic of Germany awarded him the Commander's Cross of the Order of Merit after he rejected the presidency of the *Bundesarchiv*. Posner's publications included *American State Archives* (1964), *Archives of the Ancient World* (1972), and numerous articles on archives, including several concerning foreign archives for use by the U.S. Army. Some of the essays appear in the *Festschrift, Archives & the Public Interest* (1967). During a stay in Switzerland several years ago a leg was amputated. The Posners moved to Wiesbaden for additional treatments for this and other serious ailments. Archivists world wide honor an outstanding teacher, scholar, and gentleman. Speakers at the memorial service in the Cosmos Club, of which he was a member, expressed their love and appreciation for a friend who maintained a bright sense of humor despite his tribulations.

MEYER H. FISHBEIN
Military Archives Division,
National Archives and Records Service

ARTHUR PRESTON WHITAKER died January 30, 1979, at the age of 83. He had been living, with his wife, Alix, in their house at the sea-shore in Avalon, New Jersey. His last, impressive, work, *The United States and the Southern Cone: Argentina, Chile, and Uruguay*, published in 1976 by Harvard University Press, and articles and reviews done after that, attest to a highly developed analytical and critical acuity and to its endurance. His ability, apparent from his early books, *The Spanish American Frontier* (1927) and *The Mississippi Question* (1936), and integrity, like that acuity, continued unimpaired. He once confided that as a boy he collected and devoured the popular tales of heroism and high adventure writ-

ten by G. A. Henty, and certainly echoes of their tone are evident in his own writings. Early, a fine thread of romantic allusion now and again showed through the correct prose and objective stance of this Harvard Ph.D. trained principally in the diplomatic history of the United States. The results was a fascinating amalgam analogous to a Puritan mind atop a ruffled shirt. The combination owes something to his roots. He was born June 6, 1895, in Tuscaloosa, into a family proud of living in the South since 1622, and he resided in Knoxville from 1907 through undergraduate years at the University of Tennessee. It also reflected personal proclivities. A hint of the cavalier was always present in his manner and bearing. Equally obvious was the high value he put on honesty and forthrightness, an emphasis probably to some extent traceable to being the son of an Episcopalian minister. Above all, he displayed a highly developed sense of the rational ("in my extended family," he wrote in a memoir, "there was a strain of bookishness"). That sense, as time went on, became dominant in his writings and personality. He could, at length, work on the Enlightenment within an extremely fitting state of mind, and he enjoyed being elected, in 1953, to the American Philosophical Society, "dedicated to the promotion of useful knowledge." He reacted in similar fashion to being named an honorary member of learned societies in Argentina, Chile, Colombia, and Uruguay.

The entire body of his work, twenty books and a great number of articles and reviews, demonstrates a characteristic attention to detail, to clarity, to precision, and to literary quality. He preferred to write them by hand. After short stints teaching at Florida State College for Women (now Florida State University), Vanderbilt University, the University of Wisconsin, and what was then Western Reserve University, he spent nine years at Cornell, then accepted the newly created chair of Latin American history at the University of Pennsylvania, from which he retired in 1965. At Penn he hated administrative work, did not suffer fools patiently, and gained among students and colleagues alike a reputation for putting the incisive comment or question that often led to visible nervousness in his presence and to continuing awe outside it. During his long and eminent career there, he evoked a kind of respect now nearly extinct. Working with him was the sort of experience that makes graduate school legend. The heightened perception it engendered may not often come our way again. The print hanging on his office wall of a cat eyeing a mouse inspired no peace of mind, at least not in those who identified with the mouse.

To an unusual degree at the time for the dedicated scholar he was, Arthur Whitaker entered into current affairs, even to advising his country's government on Latin American and Spanish matters

and to becoming an officer of the State Department during and just after the Second World War. He once remarked in passing that he had drafted the Act of Chapultepec. In that optimistic heyday of inter-American cooperation, no one was more cautious in outlook yet guardedly hopeful. When wrong, he took pains to set the record straight; quite a few of his essays re-evaluate and update his own previous stands, whether on the Organization of American States, the tenure of Juan Perón, the military in Latin America, or the hemispheric idea.

Arthur Whitaker loved luncheons, especially with sherry beforehand. He enjoyed good company and good ceremony, but he scorned hollow pomposity and mindless chatter. Style was important to him. He excoriated overstatement for effect of any kind, and had least time for what, in one review article, he called "the screaming eagle school of history." His own books again and again touch on sectional and individual interests and on the economic and material aspects of international relations; in what may be his classic, *The United States and Latin American Independence*, and elsewhere, he mentioned frequently the importance of specific commercial motivation and of myriad domestic, regional pressures on the formulation of early United States policy, anticipating many of the younger revisionists. Yet, although definite, he was not doctrinaire; reacting to the oversimplification and selfcongratulation then more accepted in accounts of American history, he was content to point out the complexity of the past rather than to seek all-embracing explanations or to impose his own moral judgments. Himself the product of a rich, conserving, even minority tradition, in the 1920s he brought to a new field in this country, the history of Latin America, the fine qualities and only some of the limitations of a practitioner of what Garrett Mattingly has referred to as the imperial school of American history. His mentors at Harvard were Frederick Jackson Turner and Samuel Eliot Morison. The corpus of his own work reflects the influence of both, of Turner's fascination with the frontier and of Morison's sweeping transoceanic vision and sense of adventure. To these he added an emphasis on the Spanish and Latin American point of view. For time and place his outlook was a relatively cosmopolitan one, nurtured by studies at the Sorbonne and by nomination as a Rhodes scholar and perpetuated in continuing pleasure in traveling, confering, and teaching in Latin America and Europe. Even so, he was most at home shedding light on relatively unexplored areas and regions, and where he turned up dirt and debris he tended to say so, succinctly. Difficult at times, and always demanding, he set a high standard for excellence in historical scholarship.

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Compiled by MARK L. GROVER

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